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Behavior in Small Groups—Feldman

Another Look at Collective Behavior—Weller and Quarantelli

Measures of Organizational Structure—Pennings

*Review Symposium on Shils's The Intellectuals and the Powers
and Other Essays—Nisbet and Warner*

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What Is and What Ought to Be: A Comparison of Certain Characteristics of the Ideological and Legal Styles of Thought¹

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In the ideological and in the legal styles of thought, unlike the scientific style, it is possible to make normative inferences from statements of fact. In the ideological style such inferences are made through concepts in which descriptions and values are inextricably fused. In the legal style, in contrast, it is possible to move from statements of fact to normative conclusions without invoking values along the way. The analysis of the legal style, in turn, is useful in the analysis of certain survey data on civil litigation before the federal district courts.

In the scientific style of thought, there is a sharp distinction between cognitive statements and statements of value or of norms. And it is not possible, using scientific procedures, to make a cognitive statement which has normative implications. It is not possible to infer what ought to be from looking at what is. But most people, most of the time, use languages and styles of thought which are quite different from those of science. They think and talk in modes in which there are no unbridgeable gaps between cognitive and noncognitive statements and in which links between these different types of statements are possible, or even unavoidable.

This paper analyzes some of the formal characteristics of one such style of thought, the legal style, as it exists in contemporary society. It examines the features which make it possible, within this style, to move from statements of fact to normative conclusions. By way of contrast, and in order to highlight certain distinctive characteristics of the legal style, it also analyzes certain formal characteristics of the ideological style of thought. It goes

¹ The author is grateful to many colleagues, including Marc Galanter, Ursula Goehre, William Martin, Louis Mink, Hubert O'Gorman, and Stuart Scheingold for discussion and comments on earlier drafts of this paper; to Maurice Rosenberg and Marvin Frankel for their help in providing a basic, lay person's education in legal terminology and legal processes; to Robert Hellen for his ruthless editing of an over-lengthy draft; to Larry Margolies for legal research; to Berton Pekowsky for years of unusually careful, conscientious research assistance of all types; to the foundations named in n. 16 below and to Wesleyan University for financial assistance; and to the NORC personnel named in n. 16 for their advice and consultations.

on to illustrate, with data from survey research on civil litigation in the federal district courts, how our analysis of the language of the law makes a difference, and is useful, in empirical research.

THE IDEOLOGICAL STYLE OF THOUGHT

The concept of ideology has a venerable history, and writers on the subject have used the term in a wide variety of ways.² In the works of Marx and Engels, the essence of ideology was false consciousness, the distortion in thought of social reality. Most especially, "ideology" meant the distortion of ideas which in fact grow out of objective social and natural conditions into independent entities which follow their own laws.³ In contrast to their characterization of Young Hegelianism, for example, as ideology, Marx and Engels proclaimed the dependence of their own thought upon the stage of capitalist development which European society had reached at the time in which they lived (Engels 1951*b*, p. 126).

Mannheim took from Marx and Engels the identification of "ideology" with false consciousness (Mannheim, n.d., pp. 94-97). But he also used the term in rather different senses. In some contexts, "ideological" seems simply to mean that an idea or a system of thought has social origins (Mannheim, n.d., pp. 84-85, 125).⁴ And in other contexts Mannheim comes close to using the term "ideological" in its original, 18th-century sense to refer to the study of ideas, as distinguished from using the term to refer to some characteristics of ideas, or to their social origins or social functions (Mannheim, n.d., p. 56).⁵

² For a review of some changes in usage since the late 18th century, see Lichtheim (1965, pp. 164-95). Another review of changes in usage since the late 18th century, and a catalog of contemporary uses, are in Naess (1956, pt. B, chap. 1). For a brief catalog of contemporary uses, noting seven different meanings of "ideology," see Burke (1950, p. 104).

³ For example, Engels writes: "Religion arose in very primitive times from erroneous, primitive conceptions of men about their own nature and external nature surrounding them. Every ideology, however, once it has arisen, develops in connection with the given concept-material, and develops this material further; otherwise it would not be an ideology, that is, occupation with thoughts as with independent entities, developing independently and subject only to their own laws. That the material life conditions of the persons inside whose heads this thought process goes on in the last resort determine the course of this process remains of necessity unknown to those persons, for otherwise there would be an end to all ideology" (Engels 1951*a*, p. 360).

⁴ Mannheim (n.d., pp. 80, 84-85). In the first passage cited, "the ideological element in all thinking" is equated with the assertion that "every point of view is particular to a social situation." Or, p. 125, Mannheim writes that "the great revelation" which "the notion of Ideology" affords "is that every form of historical and political thought is essentially conditioned by the life situation of the thinker and his groups." Mannheim uses "ideology" in this sense as part of a nonevaluative approach to the study of ideologies and "ideology" in the sense of false consciousness in connection with an evaluative approach.

⁵ Mannheim comes close, at least, to this meaning of the term "ideology" when he

More recently, many writers have used the term in ways which are far removed from its historical meaning or in ways which tend to deprive it of any distinct meaning at all. Sutton and his colleagues define "ideology" as "any system of beliefs publicly expressed with the manifest purposes of influencing the sentiments and actions of others" (Sutton et al. 1956, p. 2). Bell characterizes "ideology" as "the conversion of ideas into social levers," and as "commitment to the consequences of ideas," accompanied by "passion" (Bell 1962, p. 400). Shklar uses the term to mean "political preferences, some very simple and direct, others more complicated" or, in another formulation, "emotional reactions, both negative and positive, to direct social experiences and to the view of others" (Shklar 1964, p. 4).

These divergent conceptions of ideology, from Marx to the present day, have at least one point in common. In all of these conceptions it is not possible to identify a body of thought as being, or not being, ideological by examining the internal structure of the thought itself. The concept of ideology has usually been associated with the notion of unmasking the other person's ideas. Hence, in order to identify the ideological nature of a body of thought, it is necessary, in most conceptions of ideology, to go outside the ideas or beliefs themselves, and to examine the social positions of those who propound them, or their social functions, or the "passion" which accompanies them. That is, most prevailing conceptions of ideology do not define a distinctly ideological style of thought which differs in its formal characteristics from other styles, and which can be identified in some body of thought by examining the thought itself.

In contrast, it is possible to identify a body of thought (say, the works of Charles Darwin) as being or not being scientific—whether it is good science or bad—by examining such features as the formal characteristics of concepts, the relationships between them, their use in propositions, the relation between propositions and the evidence presented, and the nature of the evidence presented. Similarly, it is possible to identify a body of thought as illustrating pure mathematical, or mythological, or allegorical, or legal styles of thinking. The same should hold for the distinctly ideological style.

In order to identify the distinctive characteristics of this style it is necessary to analyze the very different ways in which cognitive statements are linked to noncognitive ones in the ideological and in other styles. Further, in order to do that job it is important to note the difference not only between *Is* and *Ought*, but also between two different types of *Ought*.

writes, for example, that the common element in the "particular conception" and the "total conception" of ideology "seems to consist in the fact that neither relies solely on what is actually said by the opponent in order to reach an understanding of his real meaning and intention" (n.d., p. 56). In this statement, "ideology" is a methodological injunction about the way in which ideas should be studied.

First, some statements of Ought are prescriptive. They set forth norms, or rules, which define prescribed, permissible, and impermissible actions. Some examples are: "Honor thy father and thy mother," "Buy War Bonds," "Let him who is without sin cast the first stone," and "Never give a sucker an even break."

There are norms of many different types. But for present purposes it is important to distinguish only between norms, on the one hand, and values, on the other.⁶ For, second, some statements of Ought are evaluative rather than normative. They do not state what is prescribed, permissible, or impermissible. They state, rather, what is good or bad, desirable or undesirable, worthy or ignoble. Or they rank goods (or evils) relative to one another. Some examples are: "The unexamined life is not worth living," "Cleanliness is next to godliness," "Better red than dead," "Better dead than red," and "Anybody who hates dogs and little children can't be all bad."

Norms and values can be distinguished on purely formal grounds. But it is often easier to determine whether a statement is a norm or a value by discovering the ways in which they actually operate in social interaction. Deviance from a norm is followed either by negative sanctions or by a belief, among those who hold the norm, that negative sanctions would be justified if they were inflicted. Negative sanctions, or the belief that they would be justified in cases of deviance, are a definitional characteristic of norms. But compliance with norms may or may not be rewarded. As regards many norms, and in many situations, compliance may be taken for granted and may evoke no particular reaction one way or the other.

For example, most people would accept the norm that people should not beat their grandmothers for beer money. They would either punish or countenance the punishment of a person who obtained beer money in this way. Or they would believe that punishment would be justified if it did occur. But most of us do not reward others, or expect them to reward us, for refraining from beating our grandmothers with this mundane end in view.

In contrast, to illustrate a value as distinct from a norm, most scientists believe in the value of doing work of Nobel Prize quality. They honor and

⁶ The distinction between norms and values is familiar and is quite frequent in the literature of sociology. However, sociologists who make the distinction often seem to slur it over, after they have made it. Characteristically, there is no confusion between norms and values when sociologists write about norms. But when they write about values, norms sneak in. I cite no specific references to illustrate this statement because I am not sure that I fully understand all the passages which I have in mind, and to cite them here might do an injustice to their authors. Fuller's discussion of "the morality of duty" and "the morality of aspiration" is a clear statement of the distinction presented in terms which differ from those used here (1964, chap. 1, pp. 3-32).

respect those who do work of that caliber. But they do not punish other scientists, or believe that others may legitimately punish them, for failing to reach that level. There is no norm which reads, "Thou shalt be another Einstein."

On the test of actual operation in social interaction, some statements of Ought which are in the imperative mood, such as "Love thy neighbor as thyself," may turn out to be values rather than norms. Conversely, norms may be stated in the indicative mood, and in the vocabulary of cognition, as in the phrase, "It isn't done," or as in the words of the admiral who told a board of inquiry investigating a collision between two ships that "it would be incorrect for an admiral to be seen on deck in his pajamas."⁷ But for present purposes, differences between types of norms, and between various ways of stating norms, are not so crucial as the overall distinction between norms and values. For without this distinction it is not possible to distinguish between the ideological and the legal styles of thought.

It is important, however, to distinguish between different types of value concepts. For ideologies characteristically include a type which we might call "impure" value concepts. First, there are pure statements of value. Such statements—for example that truth, beauty, and freedom are good—have empirical referents only in the sense that one might classify people or organizations or societies as truthful, say, and therefore good, or untruthful and therefore bad. The classification of referents in terms of the value is determined solely by (1) the meaning of the value and (2) the extent to which the people or other referents in question measure up to the value. There may be no truthful and therefore no completely good people in the world. But such a finding would not empirically invalidate the pure value assertion that truth is good. For the very notion of empirical tests does not apply to such a statement in the first place.

Second, some value concepts are impure. They include purely cognitive components. They therefore have empirical referents in a sense in which pure value concepts do not. That is, they include some components which are empirically true or false. And the classification of referents in terms of such a concept depends not only on (1) the meaning of the value component and (2) the extent to which the people or other referents in question measure up to the value, but also on (3) the purely cognitive components which the concept includes.

The concept of "progress" is such a concept. That concept combines certain cognitive assertions about directionality in human history and about

⁷ Rear Admiral G. J. B. Crabb of the Australian navy, quoted in *New York Times*, June 12, 1969, p. 6.

the reasons for that directionality with the value assertion that the goal of history's movement is desirable and good.⁸ If the cognitive component were removed, there would be no concept of "progress," but simply the pure statement of value that some envisaged future would be desirable. If the value component were removed, there would also be no concept of "progress," but simply the value-neutral cognitive assertion that there is some sort of directionality in human history. The very meaning of the concept depends upon the combination of cognitive and value elements. That is the test of an "impure" value concept. As illustrated below, concepts of this sort are central to the logic of ideologies. We therefore call them "ideological concepts."⁹

Now, for a system of belief to be an ideology (as the term is used here) it must contain at least one ideological concept, as defined above. A body of thought is not an ideology simply because it juxtaposes statements of fact with other sorts of statements, between the covers of a single book or within the same paragraph. It is an ideology only if, in addition to pure statements of fact and imperatives for action (and perhaps, also, pure statements of value), it includes one or more ideological concepts, in the sense of a concept whose meaning depends upon a fusion of cognitive and value elements.

Such concepts have a central place in ideological thinking. They make it possible, in the peculiar logic of ideologies, to move from cognitive to prescriptive statements. Madison's tenth Federalist Paper, on factions, is a good example of this procedure.

Madison's concept of "faction" is an ideological concept. For a faction is not simply one organization, or class, or social grouping of some sort which pursues its goals or interests, along with, or against, other social groupings. A faction is, rather, "a number of citizens, whether amounting to a majority or a minority of the whole, who are united and actuated by some common impulse of passion, or of interest, adverse to the rights of other

⁸For a brief statement of the essential elements in the idea of progress, see Bury (1932, p. 5).

⁹It is important to distinguish between ideological concepts, as defined here, and cognitive concepts which happen also to have some value loading. It is not possible to explicate the notion of "progress" without including both cognitive and evaluative components. It is not possible to explicate the notion of the "American Way of Life," as it is usually used, without including the cognitive notion that there are some unique features about American society, as compared with all other societies, with the evaluative notion that these unique features are desirable and good. In contrast, it is possible to explicate the notion of "motherhood" without including any value elements, even though "motherhood," in addition to its cognitive meaning, usually has some value connotations attached to it. "Progress" and "American Way of Life" are ideological concepts, while "motherhood" is a cognitive one which happens also to have a penumbra of value about it.

citizens, or to the permanent and aggregate interests of the community" (Madison, n.d., p. 54).

This concept, unlike the concept of "interest group," has both purely cognitive elements and value components such as "permanent and aggregate interests of the community." Without the value components it would be possible to classify the United Packinghouse Workers, the Boy Scouts of America, the NAACP, the Chamber of Commerce of Muncie, Indiana, and the White Citizens Councils as being, or not being, "interest groups." But without the value components it would be impossible to classify them as being, or not being, "factions." For the cognitive classification of "factions" requires value criteria as well.

Madison's arguments in favor of the proposed Constitution did not simply juxtapose statements of fact with the recommendation that the proposed Constitution be adopted. Instead, Madison systematically links cognitive and prescriptive statements together, and infers what ought to be from looking at what is, via the central ideological concept of "faction."

Briefly, he argues that factions are universal in human societies (cognitive); that they are universal for a variety of reasons, including the fact that "the latent causes of faction are sown in the nature of man" (cognitive); but that we should do something about them (prescriptive), because, by definition, they are adverse to "the rights of other citizens or to the . . . interests of the community" (ideological, that is, combining cognitive and evaluative elements). Madison then goes on, by an argument which combines cognitive, evaluative, prescriptive, and (in the sense defined here) ideological statements, to rule out certain courses of action and to conclude that the proposed Constitution should be adopted.

Note that this analysis of Madison's argument has nothing to do with his own private feelings, passions, biases, class interests, or concern for converting ideas into "social levers." The distinctly ideological nature of his central concept of "faction," and of his mode of reasoning, would be the same if he had had no feeling one way or the other and had written for money, or if he had never published the essay and had therefore never used his ideas as "social levers." Nor is the point, for present purposes, the historical and social origins of Madison's ideas. The point is simply that there are distinctly ideological concepts, distinct in their fusion of cognitive and value components, and that there is a distinctly ideological style of thought, in which ideological concepts make it possible to proceed from assertions of what is to assertions of what ought to be, and to move from a way of understanding the world to imperatives for action.

In legal thinking, too, assertions of fact have normative implications. Such implications are possible in part because some legal or jurisprudential concepts—usually very general ones such as "due process" or "rule of law"

—are ideological concepts, as defined above. In fact, the very concepts of “law” or “legality” in some usages are ideological.¹⁰ And to the extent that law, or theories about law, include ideological concepts, it is possible to use the logic of ideologies, as illustrated above, in order to reach normative conclusions from statements of fact.

But, as the next section of this paper will show, there are also quite different procedures in the legal style of thought for arriving at normative assertions from cognitive ones. It is possible, and quite common, in the legal style to bridge the gap between statements of fact and their normative implications without invoking values.

THE LEGAL STYLE OF THOUGHT

Many jurists and philosophers of law who differ among themselves in other respects agree in defining law, or legal thinking, in terms of rules. Shklar defines “legalism” as “the ethical attitude that holds moral conduct to be a matter of rule following, and moral relationships to consist of duties and rights determined by rules” (Shklar 1964, p. 1).¹¹ A. Y. Vishinsky, at a congress in Moscow in 1938, said: “Law is the aggregate of rules of conduct—of norms: yet not of norms alone, but also of customs and rules of community living confirmed by state authority and coercively protected by that authority” (Vishinsky 1951, p. 337). Hart contends that “the union of primary and secondary rules,” while it does not define the whole of a legal system, is “at the centre of a legal system” (Hart 1961, p. 96). Fuller writes: “In one aspect our whole legal system represents a complex of rules designed to rescue man from the blind play of chance and to put him safely on the road to purposeful and creative activity” (Fuller 1964, p. 9).¹²

There seem to be few exceptions to this emphasis on rules.¹³ And the

¹⁰ See, for example, Selznick (1962), who writes (p. 171) as follows: “In framing a general concept of law it is indeed difficult to avoid terms that suggest normative standards. This is so because the phenomenon itself is defined by—it does not exist apart from—values to be realized. The name for these values is ‘legality’—a complex ideal embracing standards for assessing and criticizing decisions which purport to be legal.” Further, “the essential element in legality—is the governance of official power by rational principles of civic order.” In this passage Selznick is stipulating a usage of “law” which combines cognitive and value elements. That is, he is making “law” an ideological concept, as such concepts are defined here.

¹¹ Shklar also includes certain additional components in her concept of “legalism.” “The dislike of vague generalities, the preference for case-by-case treatment of all social issues, the structuring of all possible human relations into the form of claims and counter-claims under established rules, and the belief that the rules are ‘there’—these combine to make up legalism as a social outlook” (Shklar 1964, p. 10).

¹² More generally, see Fuller’s discussion of “the two moralities,” cited in n. 6 above, and the suggestion (1964, p. 15) “that if we look for affinities among the human studies, the morality of duty finds its closest cousin in the law.”

¹³ One exception is Selznick (1962). See pp. 175–76, where, in addition to rules,

emphasis seems unduly one-sided. For the law (at least in the developed legal systems of modern societies) consists not only of rules, but also of purely classificatory categories—categories such as “spouse,” “alien,” “contract,” “juvenile,” and scores or hundreds of others. These concepts are among the terms which the rules include. But they are not rules themselves.¹⁴ And the meaning of such terms is formally independent of the rules in which they are embedded.

For example, there might be a rule that juveniles shall not receive death sentences. The language of that rule (though not its content) might remain unchanged, while the meaning of “juvenile” changes. Or the rule might be changed to allow death sentences for juveniles, while the meaning of “juvenile” remains the same. And different areas of the law differ in the meanings they give to purely cognitive categories. For example, “spouse” or “marriage” for purposes of inheritance law may mean something quite different from “spouse” for purposes of tax law. And both may differ from “spouse” for purposes of immigration.¹⁵

These purely classificatory categories, as distinct from rules of law,

Selznick points to concepts such as “exhaustion of remedies,” “Agency,” and “inter-state commerce” and to “principles” such as the principle that guilt is individual rather than collective. The distinction between principles and rules is developed at length in Dworkin (1967). In distinguishing between rules (e.g., a rule making a will invalid unless signed by a stipulated number of witnesses or a rule setting the maximum legal speed on a turnpike) and principles (e.g., the principle that no man shall profit from his own wrong) Dworkin is distinguishing between different types of legal norms. Our emphasis here, in contrast, is between the normative components and the purely cognitive or classificatory components of the law.

¹⁴ There are, of course, rules for applying classificatory categories—for deciding, for example, whether a person is an “alien” or a “spouse” or a “juvenile”—just as there are standards of cognitive judgments in science and in daily life. But rules for making cognitive judgments must not be confused with normative rules.

¹⁵ See, for example, *Litwak v. U.S.*, 344 U.S. 604 (1953). The defendants had been convicted of conspiring to obtain the illegal entry into the United States, under the War Brides Act, of three aliens married to honorably discharged veterans in ceremonies undertaken with no intention of living together as husband and wife, but solely to gain entry into the United States. On appeal, the Supreme Court sustained the conviction, holding that “by directing in the War Brides Act that ‘alien spouses’ of citizen war veterans should be admitted into this country, Congress intended to make it possible for veterans who had married aliens to have their families join them in this country without the long delay involved in qualifying under the proper immigration quota. Congress did not intend to provide aliens with an easy means of circumventing the quota system by fake marriages in which neither of the parties ever intended to enter into the marital relationship. . . . The common understanding of a marriage, which Congress must have had in mind when it made provision for ‘alien spouses’ in the War Brides Act, is that the two parties have undertaken to establish a life together and assume certain duties and obligations. Such was not the case here, or so the jury might reasonably have found” (344 U.S. at 611). In short, the marriages were not marriages for purposes of the War Brides Act, but, until dissolved, they presumably continued to be marriages for purposes of, say, state statutes against bigamy.

seem worthy of attention in their own right. For the purposes of this paper they are crucial. For, in legal reasoning, they are often the link between cognitive statements made in extralegal language and the application of rules of law to specific cases or events. They make it possible, in the logic of the law, to move from cognitive judgments to normative implications without invoking values.

In spelling out our argument we use material from survey research on civil litigation.¹⁶ Hence, the perspectives of litigating lawyers will color our analysis. But we suspect that the examples reported below shed light on the legal style of thought in general.

In order to see the place of classificatory categories in legal thinking, consider their place in a plaintiff's suit against a defendant. One ingredient in a suit at law is "the facts" of a case. Something happened. Some events took place which led to a dispute, which resulted in a suit. A second ingredient is the legal categories under which those facts may be subsumed. For example, the facts might be that some businessmen get together to talk about prices, and a relevant legal category might be "conspiracy in restraint of trade." A third ingredient is the rules of law in which the legal categories are embedded (for example, antitrust rules allowing triple damages). And a fourth ingredient is a conclusion (by a court, if a suit goes that far) about the disposition of the suit in question.

In simplified form, and leaving procedural or other secondary rules out of our analysis, the logical structure of a plaintiff's case against a defendant consists of a chain of arguments which includes at least these four steps. Whether explicit in the conscious reasoning of the plaintiff's lawyer or not,

¹⁶ Interviewers from the staff of the National Opinion Research Center conducted 460 interviews with attorneys for plaintiffs and 488 interviews with attorneys for defendants, a total of 948 interviews in 527 suits, out of an initial sample of 542 suits terminated during the fiscal year 1962 in six judicial districts (Massachusetts, New York-Southern, Illinois-Northern, Tennessee-Eastern, Texas-Southern, and California-Northern). The author is grateful to Lib Lyman, Pearl Zinner, and Paul Sheatsley, all of whom were at the time on the New York City staff of the NORC, for their helpful advice in the preparatory stages of the survey, and to Lib Lyman for administration of the interviews. In addition to the interviews, mail questionnaires were sent to opposing counsel in 910 suits terminated during the first six months of fiscal 1963 in 37 other districts. Rates of response to the mail questionnaire were slightly higher than 50%, with 475 plaintiffs' lawyers and 492 defendants' lawyers returning usable questionnaires. In general, and with some exceptions (such as that noted in n. 31 below) the two sets of data tell identical or similar stories. The data reported below (with an exception noted in table 1) are from the interview sample. For more complete details on the samples and on the research instruments, see Glaser (1968, chap. 4, appendices D, G). The data were gathered while the author was associate director of the Columbia University Project for Effective Justice, under grants to the Project, whose director was Maurice Rosenberg, from the Ford Foundation and from the Walter E. Meyer Institute, for a study of pretrial discovery in civil litigation before the federal district courts. The data reported below were analyzed under grants to the author from the National Science Foundation, with a supplemental grant from the Russell Sage Foundation, and are part of a larger study of civil litigation.

What Is and What Ought to Be

a plaintiff's case at least implicitly contends: (1) that some facts obtain or some events took place which (2) exemplify some legal concept or concepts which (3) are embedded in a rule of law, or in rules of law, from which one may conclude that (4) in situations of the type in question the courts shall satisfy plaintiff's claim against defendant.

Turning first to "the facts," to the first step in the chain of arguments, the attorney for the plaintiff in a contract suit described "the events which led to the dispute in this case" as follows:¹⁷

Ramel Products purchased a large quantity of steel from Lunar Steel Corporation. The Metling-Sellon Corporation gave a guarantee to Lunar of the indebtedness of Ramel to Lunar for this deal. The guarantee was signed by Thomas R. Metling in behalf of Metling-Sellon. Ramel folded and went out of business owing Lunar \$22,500. Suit was brought against Metling-Sellon Co. on the guarantee and against Thomas Metling . . . for falsely representing that he had authority to sign the guarantee on behalf of Metling-Sellon.

The attorney for the defendants in the same suit reported certain other facts that the plaintiff's lawyer had left out. He reported that Metling-Sellon was Ramel's parent corporation, and Lunar had asked Metling-Sellon to guarantee Ramel's indebtedness; that the guarantee was given in a letter of one sentence; and that Thomas Metling was the assistant treasurer of the Metling-Sellon Corporation.

Both attorneys gave rather sparse versions of the events in question. The facts that both lawyers left out apparently had relevance neither to the plaintiff's claim against the defendants nor to the defendants' defense. From a court's point of view, "the facts" of a case are the legally relevant facts. From a litigating lawyer's point of view "the facts" are those which are legally relevant and those which are tactically relevant. But from neither point of view do "the facts" of a case encompass everything that an exhaustive description might include.

Legally relevant facts are those which affect the classification of the events in question under one or more legal concepts. Courts and litigating lawyers have to find out what happened in a case, as described in ordinary English. For example, "Ramel Products purchased a large quantity of steel from Lunar Steel Corporation." Pure statements of fact are assertions "that a phenomenon has happened or is or will be happening independent of or anterior to any assertions as to its legal effect." A person who is "ignorant of the applicable law can accurately make such an assertion" (Jaffe 1955, p. 241).

But factual assertions in ordinary English are only the first step in the chain of arguments. The second step is the classification of "the facts"

¹⁷ All proper names in this quotation and in the quotations from interviews which follow are fictitious.

under legal categories. At this point, lawyers and judges use a vocabulary which is unique to the law. They use conceptual terms such as "offer," "acceptance," "consideration," "negligence," "contributory negligence," "assumption of risk," "agent," "restraint of trade," and "infringement." Or they use words in ordinary language, such as "wife," "partner," and "employee," that have become legal concepts and have their own technical meanings in the law. And, implicitly or explicitly, they classify the descriptive facts, the facts as stated in ordinary language, under the conceptual language of the law. For example, in the case of *Lunar Steel Corporation v. Metling-Sellon Inc.* and *Thomas R. Metling*, the lawyer for the plaintiff described some of the issues in the case as follows: "Was the guarantee *ultra vires* [beyond its powers]? Did Thomas Metling have authority to sign the guarantee on behalf of Metling-Sellon?"

These are questions about the facts of the case. But they are not purely descriptive questions. They have normative import. A person who knows no law cannot accurately answer them. They cannot be answered in ordinary English. And they cannot be answered in ways that have no consequence for the proper legal outcome of the case.

Some interoffice memoranda; or clauses in a contract of employment; or customary practices in the Metling-Sellon Corporation, or in the trade; and relevant legal precedents, will add up to a general conclusion—Thomas Metling did or did not have the "authority to sign the guarantee" on behalf of his company. If the answer is that he did have that authority, then certain rules of law, rules which hold a principal liable for the acts of duly authorized agents acting within the scope of their authority, almost automatically apply. The issues in this suit, as stated above, were questions about the facts of the case. They were not questions about the content of any legal rule. But these questions were stated in legal categories such that, when answered, certain rules of law would apply. For in legal thought normative implications inhere in purely cognitive classifications.

In ideological thought, by contrast, cognitive judgments have normative implications, and lead to imperatives for action, via distinctly ideological concepts which combine both cognitive and evaluative elements. In ideological thought, some value concept is required in order to move from purely cognitive judgments to normative ones. But in legal thought, cognitive judgments may have, and often do have, normative implications in the absence of any value concept whatsoever. Cognitive categories which (1) define "the facts" in legal terms and which, in turn, are (2) embedded in rules of law do the job in legal thinking which ideological concepts do in ideological thinking. They do the job of providing assertions of fact with normative implications.

To consider the same issues from defendants' points of view, they can respond to a plaintiff's case at any point or combination of points, factual

or legal, in the chain of arguments which makes up that case. Defendants might maintain that the facts are not as plaintiff states them. Or they might argue that the facts do not exemplify the legal concepts which plaintiff says they exemplify. Or they might maintain that the rules of law are not as plaintiff states them. Or, as in nuisance payments or in plea bargaining in criminal litigation, defendants might concern themselves solely with the disposition of a suit—how much money they shall offer to the plaintiff and what the plaintiff will accept.

Now, in roughly 80% of the personal injury suits filed before the federal district courts of the United States, and in roughly 40% of the contract-diversity suits, the only controversies between opposing counsel are on the first, and on the last, of these levels. There are issues of fact and there are negotiations over plaintiff's recovery. There are no issues of law.

In the remaining suits, roughly 20% of the personal injury suits and 60% of the contract-diversity suits, there are issues of law. Among those suits which do entail legal issues, the legal controversies in the vast majority (well over 75%) take place solely on the second level.¹⁸ That is, they have nothing to do with the content of legal rules. Instead, when attorneys disagree on the law of a suit, they much more commonly argue over the application of cognitive concepts to the facts of the case, as distinguished from controversy over the language of the rules.

For example, the following events, as told by the plaintiff's lawyer, led to a personal injury suit in the Federal District Court of Massachusetts: "After the plaintiff made a purchase in the market she was directed to go to a private office, located in another building but part of the complete store, for the purpose of determining the price of the purchases. She had to cross a narrow, dark hall enclosed on each side by a fireproof door. She opened the door, stepped out, and fell down some stairs a foot away from

¹⁸ A diversity suit is one in which the federal courts may take jurisdiction because the parties are citizens of different states in the federal union (and meet certain other requirements, such as a minimum claim of \$10,000), as distinguished from suits which come before the federal courts on other jurisdictional grounds, such as suits by or against the U.S. government, suits under federal statutes, suits involving the rights of citizens under the U.S. Constitution, and others. The percentages of suits in which there are and are not legal issues are based on the interview samples in six federal districts and on the mail questionnaire samples in 37 districts. The results from the two different samples, using two different instruments, with two different sets of possible sources of error, were quite close. The percentages, among suits in which there are legal issues, of those in which the only legal issues entail the application of classificatory concepts to the facts of a suit are based solely on the interviews in six districts. We arrived at "well over 75%" (actually, about 90%) by coding attorneys' responses to the question, "What were the principal points at issue in this case?" and certain follow-up questions. The resulting percentages are necessarily rough, entirely apart from sampling errors or bias in the returns, because most answers were brief, some were not completely clear, and in a few suits the adversaries told rather different stories. The reference to personal injury suits in this paragraph includes diversity suits, suits under federal statutes, and suits filed under the Admiralty jurisdiction.

the door. She fractured her arm." The plaintiff's lawyer described the issues in the case as follows: "Was the plaintiff, under the Massachusetts laws, still a business guest or a mere [licensee]?" This purely cognitive question, the proper classification of the plaintiff as she was walking down the corridor, determined which rules of law applied to the case. For, under the common law, the obligation of hosts to licensees are less stringent than their obligations to business guests.¹⁹ And, in this suit, the determination of the applicable rules and the conclusions apparently followed almost automatically from the classification that the court decided on. The plaintiff lost the suit, her lawyer reports, because "the court held that she left the counter and went to the rear office and therefore was a [licensee]. The defendant was not responsible and there was no negligence. We did not appeal because plaintiff had no money."

Similarly, the courts have had to decide whether men operating newsstands under contract with Hearst Publications were "independent contractors" or "employees" within the meaning of the National Labor Relations Act; whether foremen in the Packard Motor Company were "employees" or "employers" within the meaning of that act; and whether Good Humors are "milk products" or "candy and confectionery."²⁰ Such suits turn upon the resolution of arguments over the application of cognitive concepts, as distinct from rules, to the facts.

In short, to apply a rule to a specific case or (to look at the same process from the other end) to draw normative conclusions from a cognitive statement, is not simply to say that a rule applies or that the events are covered by a rule. There intervenes, in the legal style of thought, the cognitive categories of the law which define the facts in legal terms, and in terms which are included in the language of the rules. The one-sided emphasis on systems of law as systems of rules may lead students of legal thinking to slur over this crucial, intervening step, and therefore

¹⁹ In the original transcript of the interview quoted, the word in brackets, in the two quotations, is "invitee" rather than "licensee." But "invitee" must be an error, either on the part of the attorney or on the part of the interviewer. For the common law, generally speaking, distinguishes between three types of entrants into premises, business guests (sometimes called business invitees), licensees and social guests, and trespassers. It sets more stringent standards of liability for hosts of business guests than for hosts of licensees and more stringent standards for hosts of licensees than for owners of premises which trespassers enter. See, for example, Teller (1948, pp. 75-76) and Morris (1953, pp. 146, 149-50). We have not been able to find any provisions in the *Annotated Code* of Massachusetts which depart from the general tendency of the common law on these issues, and have therefore substituted "licensee" for "invitee" in the quotations from the interview with plaintiff's lawyer.

²⁰ The cases referred to are, respectively, *NLRB v. Hearst Publications*, 333 U.S. 111 (1944); *Packard Motor Car Co. v. NLRB*, 330 U.S. 485 (1947); and *Good Humor Corp. v. McGoldrick*, 289 N.Y. 452, N.E. 2d 881, 882 (1943). For brief résumés of the issues in these suits, see Jaffee (1955, pp. 251, 254, 259, respectively).

to miss a distinctive characteristic of legal, as distinguished from ideological, thinking. For example, Hart (1961, p. 119) writes:

All rules involve recognizing or classifying particular cases as instances of general terms, and in the case of everything which we are prepared to call a rule it is possible to distinguish clear central cases, where it certainly applies and others where there are reasons for both asserting and denying that it applies. Nothing can eliminate this duality of a core of certainty and a penumbra of doubt when we are engaged in bringing particular situations under general rules.²¹

In the analysis presented here, it is necessary to distinguish clearly between "classifying particular cases as instances of general terms" and "bringing particular situations under general rules." The "general terms" are not rules in themselves. They are cognitive categories which, in turn, are embedded in rules, but whose meaning is independent of the rules in which they are embedded. Only after the nonnormative classification has taken place does "bringing particular situations under general rules" come about. This way of stating the matter makes it possible to identify more precisely than would otherwise be possible the nature of the legal issues in the vast majority of the suits in which there are any legal issues at all. And, as seen below, this formulation is fruitful for empirical research on legal processes.

USE OF THE ANALYSIS OF THE LEGAL STYLE OF THOUGHT IN INTERPRETATION OF DATA

Thus far, I have used quotations from interviews with attorneys in a qualitative way, to illustrate certain features of the legal style of thought and to clarify the analysis of those features. The sections to follow will use data from these same interviews in a different and more quantitative way, and will use our analysis of the legal style of thought to make sense of certain empirical findings.

²¹ Similarly, Levi (1949) seems sometimes to distinguish between "rules" and "categories" or "concepts," and sometimes to treat the two interchangeably. For example, on pp. 1-2 Levi describes "the basic pattern of legal reasoning" in terms of rules, of a "three-step process" in which "similarities are seen between cases; next the rule of law inherent in the first case is announced; the rule of law is made applicable to the second case." On p. 6 he writes of three stages in the creation, establishing, and breakdown of a "legal concept." On p. 7 he writes of the "inherently dangerous rule," but most of the examples and of the analysis which follows have to do, not with rules of law, but with the (now outmoded) legal concept, or category, of inherently dangerous objects. Precisely because cognitive classifications almost automatically have normative consequences in legal thinking (or, as Levi puts it, on pp. 2-3, "the existence of some facts in common brings into play the general rule"), lawyers and jurists do not characteristically make sharp and consistent distinctions between concepts and rules, or between cognitive standards which govern the application of concepts to instances and normative rules which state rights, duties, burdens, or privileges.

Among other questions in the interviews, opposing counsel in terminated civil suits were asked: (1) "When you first analyzed your adversary's view of the law governing this case, did you agree, tend to disagree, or disagree strongly with his view of the law in this case?" and (2) "When you first analyzed your adversary's factual contentions, did you agree, tend to disagree, or disagree strongly with his view of the facts of the case?"

Now, if the legal style of thought has distinct characteristics, and if we make the general assumption that the processes which govern consensus and dissensus vary with the nature of the culture in question—that consensus and dissensus in physics, say, come about by processes quite different from those which obtain in everyday morality or in political ideology²²—then the determinants of agreement or disagreement between opposing counsel on the law of a case should be quite different from those which operate when some other style of thought is in question. It is possible, with our data, to make such a comparison without going outside the process of litigation. For in the interaction between opposing counsel (just as in the interaction between any two or more citizens, or between citizens and officials) some elements are lawlike, and are characterized by legality, while others are not. Specifically, we can compare the determinants of agreement or disagreement on the law governing a case with the determinants of agreement or disagreement on the facts of a case, as measured by respondents' answers to the two questions quoted above.

Very briefly, ignoring many details and referring only to defendants' lawyers, interview data not reported here show that defendants' lawyers are

²² We have not been able to find any clear-cut examples of research findings which illustrate this general assumption. Most studies of dissensus and consensus, such as research on experimental groups or studies of consensus within primary groups on voting choices, seem to concern themselves with consensus or dissensus with respect to a single item or topic (such as the choice of presidential candidate) or with a few items (such as opinions on issues in a campaign) which are similar to one another. There must be some studies, somewhere, of consensus and dissensus, within very different styles of thought, in one and the same population. But we have not been able to find or identify any. However, much extant research is suggestive on this point. For example, Vladimir Nahirny, generalizing from his studies of groups and circles of radical intellectuals in Russia during the second half of the 19th century, suggests that the unity of ideological groups depends, in part, upon the ability of all or most of the members of such groups to refrain from individualizing intimate ties, and to experience revolutionary intimacy instead. That is, the unity of such groups depends, in part, on a member's ability "to make his attachment to other members transferable and universalistic" (see Nahirny 1962). Such patterns seem quite different from value homophily of the sort which Merton and Lazarsfeld (1954, pp. 18-66) analyze. For example, Merton discusses the conditions under which "a strong and undisputed *personal* attachment can safely carry the burden of even serious disagreement over certain values" (p. 33, emphasis added), while Nahirny, in contrast, analyzes groups in which the existence of *personal* attachments threatens ideological unity. When juxtaposed, these two works point to the relativity of social processes governing consensus and dissensus. That is, such processes differ, depending upon the nature of the culture in question.

most likely to agree with plaintiffs' versions of the facts when plaintiffs have strong cases or plentiful information, and defendants have independent checks upon plaintiffs' stories. Further, the greater the extent to which defendants' lawyers specialize in representing defendants, the less likely they are to agree with plaintiffs' versions of the facts. None of these and of certain other variables that determine agreement or disagreement between opposing counsel on the facts of a case is the least bit related to agreement on the law. And none of the factors which determine agreement on the law, as seen below, is related to consensus on the facts.

I will use the analysis of the legal style of thought to explain why certain variables which are not related to agreement on the facts do determine agreement on the law. The distinction between classificatory concepts and rules makes it possible to state the logical structure of a case, or of a defense as a chain of arguments about (1) facts, (2) concepts, (3) rules, and (4) conclusions. And the finding that most legal controversies in civil litigation are controversies over the application of cognitive concepts to the facts of a case means that we can interpret the findings reported below in terms of the psychology of cognition, as distinguished from viewing them in terms of processes which determine consensus or dissensus about norms or values. More specifically, we can interpret the findings reported below, not in terms of the psychology of the cognition of descriptive facts, but in terms of the cognitive processes which lead lawyers to be more or less flexible, more or less imaginative, in their application of the classificatory concepts of the law to descriptive facts.

The chain of arguments from facts to conclusions provides the framework for interpreting specific findings. It is important to remember that this chain of arguments may remain only an implicit logical structure. The logical structure and the actual, conscious thinking of attorneys in any given suit are not necessarily the same. Attorneys may or may not consciously spell out the full logical structure. When the relevant concepts are well defined, or when the body of law which bears on a case is so small that both sides must work with pretty much the same concepts and the same rules, lawyers often jump immediately from facts to conclusions. They take concepts and rules for granted. That is what usually happens in run-of-the-mill personal injury suits. But if concepts are incompletely defined or are in flux, if rules are in doubt or are undergoing change, and if there is a considerable body of possibly relevant law, then the full logical structure of a case is more likely to become explicit in an attorney's own thought.

Further, whatever the state of the law, lawyers vary in their litigation styles. Some look at a fairly gross outline of the facts and work for a reasonable settlement, with little concern for their precise legal position. Others want to be quite clear of their legal position before they do anything else. And, as seen below, some have types of practices which make

them more prone than others to agree with their adversaries on the law. Their thinking is less likely than that of their colleagues with different types of practices to make the logical structure of a case explicit.

And whoever the lawyers are, cases vary in the extent to which it is worth the trouble to go through the entire argument. If the plaintiff's maximum recovery and the lawyer's maximum fee are quite small, and if the defendant is willing to come across with an acceptable sum, then the chances are that both sides will let it go at that. The lawyers will never know what their precise legal positions were.

In short, (1) attributes of entire areas of the law, (2) attributes of lawyers, and (3) attributes of particular cases affect the likelihood that lawyers in a given suit will make the implicit logical structure of a case explicit in their own thinking. When lawyers do make that implicit structure explicit, when they do not take concepts and rules for granted, so as to jump immediately from facts to conclusions, they are more likely to disagree with their adversaries' views of the law. For they are then more likely to discover ways in which the application of received concepts to the facts of a given suit is arguable.

It is possible, therefore, to divide factors that influence agreement on the law into three general types. First, institutional determinants such as the frequency of appeals in various types of cases, and the differences between statutory law and the case law, make for more or less change in the law over time, or for more or less law to argue about in the first place. Second, characteristics of lawyers and of their practices, such as the extent of specialized experience in litigation, make them more or less able to argue the law. Third, characteristics of cases such as the amount of money at stake make effort more worthwhile in some cases than in others.

The data which follow exemplify the working of determinants of these three types. They are intended, in this context, to be only illustrative. Hence, instead of a full analysis, we will examine certain determinants of agreement or disagreement with the adversary's view of the law only on the part of defendants' attorneys. And, as regards attributes of cases and attributes of lawyers and of their practices, we focus only on personal injury suits.²³

²³ The analysis of the responses of plaintiffs' lawyers to the question, "When you first analyzed your adversary's view of the law governing this case did you agree, tend to disagree, or disagree strongly with his view of the law in this case?" is rather more complex than the analysis of the answers of defendants' lawyers. The question does not mean the same thing when put to the two sides. Generally speaking, plaintiffs first present their cases, and only then do defendants respond. Generally speaking, then, the question about agreement or disagreement with the adversary's view of the law, when put to defendants' lawyers, asked them how they reacted to the case which their adversaries had previously set forth. If they agree, they may or may not disclose this agreement to their adversaries. If they do agree, and do disclose their agreement, then for plaintiffs' lawyers to report that they agreed with their adversaries' views of the

SOME INSTITUTIONAL DETERMINANTS OF AGREEMENT OR
DISAGREEMENT ON THE LAW

Our data on the first of these three types of determinants are susceptible to a variety of interpretations. But they are consistent with the predictions one would make, based on the observations of various commentators, about the differences between suits filed under statutes and those filed under case law; and between areas of law which differ in the frequency of jury trials and of appeals after trial.

Respecting the differences between litigation under statutes and litigation under the common law, Levi points out:

Concepts created by case law also have some meaning, but the meaning is ambiguous. It is not clear how wide or narrow the scope is to be. Can it be that the words used by the legislature [in a statute] have any more meaning than that, or is there the same ambiguity? One important difference can be noted immediately. Where case law is considered, there is a conscious realignment of cases; the problem is not the intention of the prior judge. But with a statute the reference is to the kind of things intended by the legislature. All concepts suggest, but case-law concepts can be re-worked. A statutory concept, however, is supposed to suggest what the legislature had in mind; the items to be included under it should be of the same order. We mean to accomplish what the legislature intended. [Levi 1949, p. 21]

To be sure, the intent of legislatures is often unclear. But in litigation under statutes there is an original font of law which does not mix holdings with dicta, and in which rules are prospective, not announced for, and to some extent limited to particular cases. There is no original font of common-law concepts, with which all future decisions must in some degree be aligned. There are only prior decisions about particular cases in which holdings and dicta are intertwined. Hence, groups of cases under statutes should be less likely to add up in different ways, or to suggest conflicting lines of development. The result should be less disagreement over the law

law really means that the defendants' lawyers disclosed to the plaintiff's lawyer the fact that they agreed with plaintiff's view of the law. Hence, some of the determinants of agreement on the plaintiff's side turn out to be (1) determinants of agreement on defendant's side (as reported below), coupled with (2) determinants of defendant lawyer's willingness to disclose this agreement to plaintiff (such as legal issues being unimportant in the suits, or defendants' lawyers thinking that they have a strong defense). The process does not work in reverse. These complexities require more analysis than it is possible to present in this paper. Limitations of space also require us to limit ourselves to personal injury suits as regards attributes of lawyers and of cases. Some of the determinants of disagreement over the law in contract suits are quite different from those which operate in personal injury suits. For example, a greater fund of factual information seems to generate disagreements over the law in contract, but not in personal injury litigation. It is apparently possible to understand these differences in terms of a single theory of civil litigation. But limitations of space make it impossible to go into all the issues in this paper.

of a case in statutory actions than in otherwise similar actions under case law.

Most of the data in table 1 are consistent with this argument. As those data show, defendants' lawyers are less likely to agree with their adversaries' views of the law in common-law suits (most contract-diversity and most personal injury-diversity actions) than in roughly similar suits litigated under statutes. There is hardly any more agreement in suits under the Federal Employers Liability Act (which governs suits by employees of railroads against their employers arising out of injuries suffered on the job) than in automobile accident cases, a type of litigation which is highly routinized and stereotyped. But there is considerably more agreement in FELA suits than in other nonmaritime personal injury cases. There is more agreement on the law in maritime personal injury cases filed under a federal statute, the Jones Act (which made the provisions of the FELA applicable to seamen), than in maritime personal injury cases not filed under statutes, those filed under the Admiralty jurisdiction and those filed as diversity suits. And, judging from very small totals, there is slightly more agreement on the law in suits filed under the Miller Act (a federal statute requiring prime government contractors to post bonds, so as to allow subcontractors to sue prime contractors despite the general rule that liens cannot be filed against public property) than there is in contract-diversity cases.

Certain other factors complicate the argument and make our data susceptible to alternative interpretations. For example, the difference between FELA suits and other nonmaritime personal injury suits may reflect specific provisions of the statute in question, rather than generic differences between litigation under statutes and litigation under case law. The FELA abolished contributory negligence, assumption of risk, and certain other defenses which defendants can use in other, nonmaritime personal injury suits. And judicial interpretation of the FELA has tended to move it in the direction of liability without fault (Larson 1952, p. 508).²⁴ Hence, the greater level of agreement on the law in FELA cases may simply reflect the

²⁴ Larson (1952) notes that in suits under the FELA "the actual basis of recovery is much closer to a nonfault type of liability and further from an old-fashioned negligence liability than the printed page of the statute book might lead one to believe. This is the result of a series of Supreme Court cases vastly expanding the role of the jury in FELA cases and narrowly constricting the power of the trial judge to direct a verdict for the defendant because of the inadequacy of the evidence of negligence. This trend led Mr. Justice Jackson, dissenting in *Wilkerson v. McCarthy* [a case which the Supreme Court heard in 1949] to intimate that the FELA has in effect become a nonfault compensation act." Larson believes this assertion to be an overstatement but suggests (1952, p. 508) that Supreme Court decisions sending FELA suits back for jury decisions, replacing directed verdicts, have gone a long way toward reducing the burden of proving negligence in FELA cases, since experience has shown that in a high proportion of cases success in getting the case to the jury is equivalent to success in getting a recovery.

TABLE 1
PERCENTAGE OF DEFENDANTS' ATTORNEYS WHO AGREED, TENDED TO DISAGREE, AND DISAGREED STRONGLY WITH THEIR ADVERSARIES' VIEWS OF THE LAW IN VARIOUS TYPES OF SUITS
A. PERSONAL INJURY SUITS

DEFENDANT ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW	NONSTATUTORY SUITS*					SUITS UNDER STATUTES	
	Personal Injury-- Diversity (Motor Vehicle)	Other Non- maritime Personal Injury-- Diversity	Personal Injury-- Diversity (Maritime)	Personal Injury (Admiralty)	Jones Act	FELA	
Agreed	68%	43%	39%	50%	76%	76%	
Tended to disagree	20	22	45	25	13	12	
Disagreed strongly	12	35	16	25	11	12	
Total	100% (71)	100% (59)	100% (34)	100% (22)	100% (41)	100% (26)	

B. COMMERCIAL SUITS

DEFENDANT ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW	NONSTATUTORY SUITS					SUITS UNDER STATUTES			
	Contract- Diversity (General Commercial)	Contract- Diversity (Insurance)	Contract- Diversity (Other)	All Contract- Diversity (Mail Sample)†	Miller Act (Mail Sample)†	Copyright and Trademark	Patent	Antitrust	
Agreed	34%	30%	...	29%	40%	40%	28%	32%	
Tended to disagree	24	31	31	35	32	14	8	21	
Disagreed strongly	42	40	69	36	28	46	64	46	
Total	100% (91)	101% (39)	100% (11)	100% (102)	100% (11)	100% (29)	100% (25)	99% (28)	

NOTE.—This table and all of the following tables are adjusted for the oversampling of cases which completed trial. In this table, and in all of the following, the percentages are adjusted, but the totals are the actual totals, rather than the hypothetical, weighted totals. Hence, it is not possible to work back from the totals to the presented here do not lend themselves to such an operation.

A small number of the diversity suits, such as suits for wrongful death, may in fact be suits under state statutes.

† Except for these two columns, all data in this table are from the interview sample (see n. 25 above). Suits from the mail questionnaire sample are included here, since the interview sample did not include any suits under the Miller Act.

fact that defendants have fewer possible defenses in these actions than they do in other nonmaritime personal injury suits, and therefore have less room for disagreement in the first place.

Such considerations apparently do not apply, or do not apply to the same extent, to the comparison between suits under the Miller Act and contract-diversity suits, or to the comparison between suits under the Jones Act and other maritime personal injury cases. General contract law seems to apply to cases under the Miller Act.²⁵ And in applying the FELA's abolition of certain defenses to statutory actions for personal injury brought by seamen against their employers, the Jones Act brought such actions into line with general maritime law,²⁶ which governs both maritime personal injury suits filed as diversity cases and those filed under the Admiralty jurisdiction.²⁷

But the history of litigation under the Jones Act does introduce some additional complications. The argument presented above, stemming out of Levi's comparison of case-law concepts with statutory concepts, was static. But data gathered in the early 1930s would probably have shown considerably more disagreement over the law governing Jones Act cases than was the case in the early 1960s. For that act, in the view of some experts, embodies very vague concepts and was quite poorly drafted. Two specialists on maritime law write that "the Jones Act was abominably drafted and

²⁵ One source suggests that most points of dissimilarity between government contracts and private commercial contracts stem from specific mandatory clauses, such as the provisions of the Miller Act requiring prime contractors to post bonds, which contractors must accept if they want to do business with the government (see 1 *San Diego Law Review* 88 [1964]). More generally, Mitchell (1964, pp. 144-45) writes that "the general rule that 'when the U.S. enters into contract relations its rights and duties therein are governed generally by law applicable to contracts between private individuals' finds support in a considerable body of cases."

²⁶ One exception to this statement is the provision in the Jones Act for wrongful death action. Wrongful death is not a cause of action under the common law or in Admiralty.

²⁷ By way of further explanation, it should be noted that seamen and stevedores often have a wide choice of forums. First, they may sue under the Admiralty jurisdiction, which, under the Constitution, is a federal jurisdiction. In such suits there is no trial by jury. Second, where there is diversity of citizenship, the same fact situation can generate a diversity suit in the federal courts, in which there is trial by jury. Or, third, in the absence of diversity of citizenship, seamen may sometimes sue in state courts in which there is trial by jury. Fourth, the Jones Act allows for jury trials in the federal courts even where there is no basis for a diversity suit. However, with certain exceptions such as the one cited above in n. 25, the various forums and various bases of jurisdiction do not entail great differences in the applicable substantive law. Norris (1966, p. 201) writes that "even though a personal injury claim arising from a maritime tort is brought on the 'law' or civil side of the Federal District Court [rather than under the Admiralty jurisdiction], the substantive Federal maritime law controls. The choice of action 'at law' cannot serve to diminish the dimensions of the substantive rights accorded by the maritime law."

the case law construing it became a trackless maze bristling with danger points and honeycombed with pitfalls" (Gilmore and Black 1957, pp. 250-51).

During the decades after the passage of the Jones Act in 1920 there was considerable litigation over the meaning of four central concepts in the text—" [1] *Any seaman* [2] *who shall suffer personal injury* [3] *in the course of his employment* may, [4] *at his election*, maintain an action for damages at law, with the right to trial by jury . . ." (Merchant Marine Act of 1920, sec. 33). But the constraints upon courts to interpret vague concepts in statutes eventually led to relative clarity in the meaning of the disputed terms. Such developments, leading in time to a relatively stable framework of concepts whose meanings (in the absence of further legislative action) are relatively fixed, seem more likely in litigation under statutes than in litigation under case law. In contrast, concepts in case law—as illustrated, for example, by Levi's history of the concept of "inherently dangerous objects (Levi 1948, pp. 9-27)²⁸—seem more likely to undergo continual evolution, until they eventually die out or are replaced by altogether different concepts.

In short, the differences between statutory actions and roughly similar common-law actions in table 1 must be read, in part, in the light of specific provisions of specific statutes; and comparisons which seem more likely to reflect generic differences between statutory and nonstatutory litigations have to be seen historically. What is more, perhaps some statutory law will never undergo the evolution toward concepts whose meanings are relatively fixed. In antitrust, for example, perhaps a continual infusion of highly skilled advocacy into an area of law in which the stakes are very high, the fact situations are complex, and governmental policy changes with the political winds, will prevent such an evolution. We mean simply to contend that, for reasons Levi suggests, there is greater inherent potential for disagreement over the law in common-law actions than in statutory actions, and that the data in table 1 are consistent with this argument.

Entirely apart from this difference, the data in table 1 also show more disagreement over the law in contract and in other commercial suits than in personal injury cases. Personal injury cases and commercial suits proceed through the courts in rather different ways. The data suggest (but cannot demonstrate) certain connections between these institutional features of civil litigation and the frequency of disagreement of the law.

First, the differences in table 1 probably reflect, in part, some conse-

²⁸ For another example, see the section on the history of the fellow-servant rule in the United States in Friedman and Ladinsky (1967). That rule was progressively watered down and narrowed to apply to increasingly fewer situations, until workman's compensation statutes finally made it completely obsolete.

TABLE 2
PERCENTAGE OF ALL TRIALS WHICH ARE JURY TRIALS AMONG
SUITS OF VARIOUS TYPES: FEDERAL DISTRICT COURTS,
FISCAL YEAR 1962

Type of Suit	Trials Held before a Jury	
Personal injury:		
Personal injury—diversity	86%	(2,109)
Personal injury—marine*	47	(300)
FELA	89	(282)
Commercial:		
Contract-diversity	37	(753)
Copyright, trademark	4	(521)
Patent	3	(117)
Antitrust	49	(37)

SOURCE.—*Annual Report of the Director of the Administrative Office of the United States Courts* (Washington, D.C.: Government Printing Office, 1962), table C4.

* This category, used in the source noted above, includes both personal injury suits under the Admiralty jurisdiction, in which there is no trial by jury, and other maritime personal injury suits in which there are jury trials.

quences of the jury system. As seen in table 2, jury trials are much more common in the various types of personal injury cases than in most types of commercial suits.²⁹

In general, judges decide questions of law and juries decide questions of fact.³⁰ Hence, lawyers who are concerned with appeals to a jury are probably less oriented to the law than those who face a bench trial. And if, over time, lawyers in a whole class of suits have relatively little concern for legal issues, then the law governing that class of suits will develop more slowly. Over time, there will develop less law to disagree about. For disagreement over settled doctrine is less likely than disagreement over law that is in flux. And one source of change and development in the law is attorneys' challenges to the ways in which their adversaries apply established legal concepts to the facts of their case.

Second, according to data not reported here, litigants are more likely to appeal decisions after the initial trial in commercial suits than in most types of personal injury cases. Suits that might make precedent rarely do so unless they proceed to the appellate level. Hence, types of suits that

²⁹ Table 2 cites data from fiscal year 1962 simply because the suits in the study reported here were terminated during fiscal 1962 (interview sample) or during the first six months of fiscal 1963 (mail questionnaire sample). The percentages of suits in our samples, among those which completed trial, in which there were trials by jury are generally similar, for all types of suits, to those reported in table 2 for all federal district courts.

³⁰ This generalization breaks down, of course, whenever juries have to decide questions of fact as stated not in extralegal terms but in terms of legal categories. For example, was plaintiff contributorily negligent?

What Is and What Ought to Be

result in relatively frequent appeals are likely to engender more law, and more changes in the law over time, than types that result in relatively few appeals.

What is more, as seen in table 3, legal disagreements between adversaries

TABLE 3
LOSING ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW BY
PERCENTAGE OF SUITS IN WHICH LOSING PARTY APPEALED
AFTER COMPLETED TRIAL

LOSING ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW	SUITS IN WHICH LOSING PARTY APPEALED	
	All Personal Injury	All Commercial
Agreed	11% (49)	22% (18)
Tended to disagree	43 (21)	31 (13)
Disagreed strongly	32 (31)	64 (44)

seem to be a more important determinant of appeals in commercial cases than they are in personal injury suits (reading down the two columns in table 3). And cases in which lawyers for the losing party "strongly disagreed" with their adversaries' views of the law seem roughly twice as likely to result in appeals in commercial as compared with personal injury cases (reading across the last line in table 3).³¹ There is less potential for legal disagreement in personal injury cases because there is less law to begin with, and the disagreements that do exist are less likely to engender new law because they more often remain dangling after trials, without resolution by appellate courts.

LAWYERS AND THEIR PRACTICES AS DETERMINANTS OF AGREEMENT OR DISAGREEMENT ON THE LAW

Personal injury cases and commercial suits differ not only in the ways in which they proceed through the courts but also in the types of lawyers who typically handle them. For example, lawyers in commercial cases are more likely than lawyers in personal injury cases to be members of law firms.³² On the plaintiff's side, at least, lawyers in commercial cases are

³¹ This finding is not replicated in the data from the mail questionnaire sample.

³² Roughly 70% of the defendants' attorneys in personal injury suits in our interview sample are members of law firms, as against roughly 80% of the defendants' attorneys in contract suits. However, a somewhat higher percentage of defendants' lawyers in contract suits than in personal injury suits are solo practitioners. For those defendants' lawyers in personal injury suits who are not members of law firms include not only solo practitioners but also attorneys employed by insurance companies and attorneys employed by other types of business firms (for example, attorneys employed by railroads who defend their employers in suits under the Federal Employers Liability

more likely to belong to relatively large firms.³³ Membership in firms, and in large firms, is in turn associated with graduation from the higher-quality law schools.³⁴ And both plaintiffs' and defendants' lawyers in commercial

Act). On the plaintiff's side roughly one-third of the lawyers in personal injury suits, a slightly smaller percentage on contract suits, and roughly 20% in other commercial suits are individual practitioners. Most of the rest are members of law firms. These data, and those in the notes which follow, like the data in the tables presented here, are adjusted for the oversampling of suits tried to completion, although, with respect to most items, the adjusted and the unadjusted data tell virtually the same story.

³³ In our interview sample, about 2% of the plaintiffs' lawyers in personal injury suits, as against roughly 20% in contract suits and in other commercial cases, belong to law firms in which there are 12 or more lawyers. The same tendency is present, as between personal injury cases and contract suits, on the defendant's side as well. But the differences are not so great. A relatively large percentage of defendants' lawyers in personal injury suits belong to firms with 12 or more lawyers, and in contract suits only a somewhat greater percentage come from such firms. These data are consistent with data gathered in 1960 on lawyers in Manhattan and the Bronx, reported in Carlin (1966, table 12 and pp. 25-26). Our data and Carlin's are not strictly comparable. Our sample includes employed attorneys, while Carlin's was limited to attorneys in private practice. We sampled terminated suits and he sampled lawyers. But his data show that lawyers in individual practice and in small firms (four lawyers or fewer) are considerably more likely than others to have their "main area of practice" in personal injury, while lawyers in medium-sized firms (five to 14 lawyers) and in large firms (15 or more) are considerably more likely than others to specialize in various types of business matters. Handler (1967, pp. 55-56) reports somewhat different findings from a middle-sized city in the Midwest, using data gathered in 1964. Lawyers in larger firms (six lawyers or more) were more likely than others to have business clients and business practices, but were also more likely than the others to spend some time representing plaintiffs in personal injury suits. Handler's classifications of lawyers by their incomes and by the wealth of their clients show greater differences in these respects than does his threefold classification of individual practitioners, small firms (three to five lawyers), and large firms (six or more). These data are, of course, not strictly comparable with Carlin's or with ours, since a "large" firm in Carlin's study consists of 15 or more lawyers, while the cutting point in our data is at 12 or more lawyers.

³⁴ See, for example, table 15 in Carlin (1966, p. 28). Lawyers in large firms (15 or more lawyers) are more likely than lawyers in medium-sized firms to be graduates of full-time, university law schools, and lawyers in medium-sized firms are in turn more likely to be graduates of such schools than are lawyers in small firms (two to four lawyers) or individual practitioners. The percentages, in these four categories, who attended full-time, university law schools are 77%, 43%, 22%, and 19%, respectively. See also tables 19 and 20 in Carlin (1966, p. 31), which show that membership in large firms is associated with graduation from full-time university law schools, independent of standing in law school and independent of religion. Data gathered in 1960 on a sample of 207 lawyers in Detroit also show that lawyers in firms tend to have a higher-quality legal education than lawyers in solo practice (defined so as to include two-men family partnerships). See Ladinsky (1963, table 2, p. 50). For example, 86% of the firm lawyers and 43% of the solo practitioners completed college; 73% of the firm lawyers and 14% of the solo practitioners attended a "national" law school. Note, however, that Handler (1967, p. 57) reports that "the quality of legal education of the lawyers was not related to firm size" in the Midwestern city he studied. However, all but one of the 83 lawyers interviewed (out of 91 in private practice in the city) had graduated from law school, almost 90% had college degrees, and only 5% had attended law schools offering both day and evening courses (Handler 1967, p. 45).

cases tend to have been in practice for a somewhat longer time than their counterparts in personal injury suits.³⁵

Further, lawyers in personal injury suits tend to be more specialized in a variety of respects than lawyers in most types of commercial suits.³⁶ And lawyers in commercial suits tend to spend more time on a case than lawyers in personal injury cases. This finding holds up, with certain major qualifications, when we control for the amount of money at stake.³⁷ The data on

In contrast, in Carlin's New York City sample, only 56% had college degrees and 64% had attended law schools offering both day and evening courses (Carlin 1966, p. 19). And in Ladinsky's Detroit sample less than 70% had college degrees and over 30% had some part-time legal education (Ladinsky 1963, p. 50). In short, there was, to begin with, relatively little differentiation with respect to educational background among the lawyers whom Handler studied. Hence, the variations which Carlin and Ladinsky found in larger cities could not appear.

³⁵ Close to 30% of the plaintiffs' lawyers in personal injury suits in our interview sample, as against almost 50% in contract suits, had been in practice 25 years or more at the time of the interview. The approximate median numbers of years in practice were 16.5 years and almost 22 years in personal injury and in contract suits, respectively. On the defendant's side, the corresponding percentages were 32% and 46%, respectively, and the corresponding medians were 17.1% and 20.0%.

³⁶ For example, attorneys in the interview sample were asked which type of suit comprised the largest proportion of all suits in their litigation practices. Well over 70% of the defendants' lawyers in the various types of maritime personal injury suits (personal injury—diversity—marine, Jones Act, and personal injury—Admiralty) picked "personal injury—marine" in response to the question, and about 15% picked "other personal injury." Similarly, about 80% of the defendants' lawyers in nonmaritime personal injury suits picked "other personal injury." In contrast, about 40% of the defendants' lawyers in contract-diversity insurance suits and about 60% of the defendants' lawyers in other contract-diversity suits picked "corporate and commercial." A fairly high percentage of defendants' lawyers in the suits over insurance contracts picked "other personal injury," and a fairly high percentage in the other contract suits picked the residual, miscellaneous "other" category. Similarly, relatively small percentages in copyright and in trademark suits report that they specialize in those types of litigation, and relatively high percentages picked the residual "other" category. Among defendants' lawyers in commercial suits, only those appearing in patent and in antitrust cases seem to be as highly specialized in those types of litigation as personal injury lawyers are in personal injury litigation. Our data show that lawyers in personal injury suits are more specialized than lawyers in commercial suits in still other respects as well. For example, a higher percentage of all cases in the litigation practice of lawyers in personal injury suits than of cases in the practices of lawyers in commercial suits are of the type which they most frequently litigate. And lawyers in personal injury suits tend to spend a higher proportion of all their working time on litigation than do lawyers in commercial suits. In short, lawyers in personal injury suits are more specialized with respect to (1) area of law, (2) litigation as against other types of legal work, and (3) litigation in a single area of law.

³⁷ Attorneys spend more time per case in commercial litigation than they do in personal litigation, in part because the higher the monetary stakes, the more time they spend on a case, and commercial suits tend to involve larger sums than do personal injury suits. Controlling for monetary size (as measured by plaintiffs' lawyers' estimates of maximum possible recovery), the tendency for lawyers to put more time into a commercial suit than they put into personal injury cases continues to hold among the smaller and among the larger suits, but not in the middle-sized cases. Among defen-

this point are in line with the common knowledge of the legal profession that personal injury litigation tends to be more perfunctory than commercial litigation. There are more run-of-the-mill cases that lawyers can handle in a routine manner.

These differences between the attorneys who handle different types of suits have consequences for the frequency of disagreement over the law that governs a suit. The characteristics of attorneys, or of their practices, which incline them to agree with their adversaries' views of the law are more frequent among lawyers in personal injury cases than they are among lawyers in commercial suits. For example, attorneys in personal injury suits are more likely than attorneys in most types of commercial suits to specialize in one area of law; and both in personal injury cases and in contract suits, attorneys who specialize in personal injury are more likely than those who do not have this speciality to agree with their adversaries' view of the law. Defendants' attorneys in personal injury suits spend more of their total working time on litigation than defendants' attorneys in contract suits, and among defendants' lawyers in personal injury cases, those who spend a high proportion of their working time on litigation are more likely than the others to agree with their adversaries' view of the law.

In short, looking only at cases of a single type, some lawyers are more able than others to find, or to create, points of disagreement. The logical structure of a case and the place of legal precedent or of established doctrine in that logical structure suggest some reasons for these differences among lawyers.

Plaintiffs' lawyers subsume the facts of a case, as they construe them or choose to present them, under legal concepts. They invoke rules of law to conclude that the facts, thus conceptualized, point to a judgment for plaintiff. In doing so they sometimes use what Llewellyn calls "the orthodox doctrine of precedent," or "the strict view," of the ways in which established concepts and rules should be applied to particular cases. In this view:

Every case lays down a rule, the rule of the case. The express ratio decidendi is *prima facie* the rule of the case, since it is the ground upon which the court chose to rest its decision. But a later court can reexamine the case and can invoke the canon that no judge has the power to decide what is not before him, can, through examination of the facts or of the procedural issue, narrow the picture of what was actually before the court and can hold that the ruling made requires to be understood as thus restricted. In the extreme form this results in what is known as expressly

dants' lawyers this tendency is not present when the maximum recovery is roughly \$20,000-\$50,000. Among plaintiffs' lawyers it is not present when the maximum recovery is roughly \$10,000-\$20,000. On both sides of these ranges, however, lawyers spend more time on a commercial suit than they do on a personal injury suit of roughly the same monetary size.

"confining the case to its particular facts." This rule holds only of redhead Walpoles in pale magenta Buick cars. [Llewellyn 1951, pp. 66-67]

But in ordinary civil litigation, plaintiffs' lawyers do not usually slice the law so finely. If they have a *prima facie* case, if at first glance the facts and the law seem to point their way, there is no reason why they should. If the facts seem to fall under established concepts embodied in received rules of law that would indicate a recovery for plaintiff, they can let it go at that. They can use the "loose view" of precedent:

For when you turn to the actual operations of the courts or, indeed to the arguments of lawyers, you will find a totally different view of precedent at work, . . . the *loose view* of precedent. That is the view that a court has decided and decided authoritatively, *any* point or all points on which it chose to rest a case, or on which it chose, after due argument, to pass. No matter how broad the statement, no matter how unnecessary on the facts or the procedural issues, if that was the rule of the court laid down, then that the court has held. . . . In its extreme form this results in thinking and arguing exclusively from *language* that is found in past opinions, and in citing and working with that language wholly without reference to the facts of the case which called the language forth. [Llewellyn 1951, pp. 67-63]

It is easy for lawyers to follow the "loose view." Little time and little money are required. It is difficult to follow the "strict view." Skillful lawyers, careful legal research, and minute examination of the facts may be required. Meanwhile, fees mount up. Some cases are not worth it. Some lawyers are not up to the task. They are inexperienced or unskilled. Or, as specialists in personal injury litigation, they have become used to handling large numbers of relatively stereotyped cases, involving few legal issues, in a relatively perfunctory fashion. The conceptual response of such lawyers to the facts of a case is likely to be less subtle and less flexible than the response of lawyers who have more, and more varied, experience. They are less likely to see ways, or even to look for ways, in which the facts of the next case they encounter may not fit perfectly under received legal concepts. Instead, they simply go along with the adversary's application of seemingly applicable concepts and rules to the facts of a suit. They agree with their adversary's view of the law.

Llewellyn's distinction between the "strict view" and the "loose view" of precedent suggests, for example, that defendants' lawyers in personal injury suits who specialize in personal injury litigation should be more likely than those who do not specialize in this way to agree with their adversaries' views of the law. The strict view is more calculated to produce disagreements over the law. In order to use the strict view, lawyers must see fine and legally relevant distinctions between similar sets of events. But lawyers who specialize in personal injury litigation do not have a type

of practice that requires them, or trains them, to see fine, legally relevant distinctions.

Past experience with a relatively stereotyped kind of litigation, such as personal injury cases tend to be, makes defendants' lawyers in the next case they confront less likely, or perhaps less able, to find bases for disagreement on the law. As seen in table 4, those who report that personal

TABLE 4
DEFENDANT ATTORNEYS' AGREEMENT WITH ADVERSARIES' VIEWS OF
THE LAW BY TYPE OF SUIT COMPRISING THE LARGEST
PROPORTION IN THEIR LITIGATION PRACTICE

DEFENDANT ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW	TYPE OF SUIT COMPRISING LARGEST PROPORTION	
	Personal Injury	Other Than Personal Injury
Agreed	55%	27%
Tended to disagree	22	20
Disagreed strongly	23	54
Total	100% (221)	100% (28)

NOTE.—Data include only personal injury cases.

injury cases comprised "the largest proportion of all cases" that they had "litigated over the past year or so" are more likely than their less specialized colleagues to agree with their adversaries' views of the law.

Similarly, as seen in table 5, the greater the percentage of their working time they spend on suits of the type which "comprises the largest proportion" of their practice, the more likely they are to agree with the other side on the law that governs a case. And, as seen in table 6, defen-

TABLE 5
DEFENDANT ATTORNEYS' AGREEMENT WITH THEIR ADVERSARIES' VIEWS OF THE LAW
BY PERCENTAGE OF ALL CASES THEY LITIGATE THAT ARE OF THE TYPE
THEY MOST FREQUENTLY LITIGATE

DEFENDANT ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW	PERCENTAGE OF ALL CASES THAT ARE OF THE MOST FREQUENT TYPE			
	50% or Less	51%-79%	80%-94%	95% or More
Agreed	36%	58%	57%	65%
Tended to disagree	61	20	19	19
Disagreed strongly	4	22	24	16
Total	101% (21)	100% (62)	100% (83)	100% (78)

NOTE.—Data include only personal injury cases.

TABLE 6

DEFENDANT ATTORNEYS' AGREEMENT WITH THEIR ADVERSARIES' VIEWS OF THE LAW BY PERCENTAGE OF THEIR WORKING TIME SPENT ON LITIGATION

DEFENDANT ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW	WORKING TIME SPENT ON LITIGATION				
	Under 40%	40%-64%	65%-89%	90%-99%	100%
Agreed	35%	45%	71%	68%	57%
Tended to disagree	44	32	17	15	22
Disagreed strongly	21	24	12	17	21
Total	100% (15)	101% (28)	100% (58)	100% (77)	100% (73)

NOTE.—Data include only personal injury cases.

dants' lawyers who spend roughly two-thirds or more of their time on litigation are more likely to agree with their adversaries' views of the law than those who spend less time on litigation and more time on other types of legal work; and those who spend less than 40% of their time on litigation are least likely to agree. More experience with a fairly stereotyped kind of litigation makes defendants' lawyers less likely to question the ways in which their adversaries have applied received concepts to the facts of a case.

In another sense of "experience," however, the less experienced lawyers are more likely to agree with the other side's view of the law. As seen in table 7, defendants' lawyers in personal injury suits who have been in practice only five years or less are considerably more likely than their more experienced colleagues to agree on the law. And the old-timers, those in practice more than 30 years, are considerably less likely than their younger colleagues to agree.

There is experience that makes for stereotyped responses to facts that seem to fit under received legal concepts. There is experience that provides

TABLE 7

DEFENDANT ATTORNEYS' AGREEMENT WITH THEIR ADVERSARIES' VIEWS OF THE LAW BY THE NUMBER OF YEARS THEY HAVE BEEN IN PRACTICE

DEFENDANT ATTORNEY'S VIEW OF ADVERSARY'S VERSION OF THE LAW	YEARS DEFENDANT ATTORNEY HAS BEEN IN PRACTICE				
	Under 6	6-10	11-15	16-30	Over 30
Agreed	83%	59%	51%	53%	36%
Tended to disagree	14	27	20	26	25
Disagreed strongly	3	15	29	21	39
Total	100% (23)	100% (32)	100% (85)	100% (85)	100% (28)

NOTE.—Data include only personal injury cases.

variety and that breaks down stereotyped response sets. The effects of these two types of experience, of years in practice and of specialized work in personal injury litigation, are in part dependent on each other. As seen in table 8, the effect of concentration becomes weaker over the years. It is

TABLE 8

DEFENDANT ATTORNEYS' AGREEMENT WITH THEIR ADVERSARIES' VIEWS OF THE LAW BY NUMBER OF YEARS THEY HAVE BEEN IN PRACTICE BY PERCENTAGE OF ALL CASES THEY LITIGATE THAT ARE OF THE TYPE THEY MOST FREQUENTLY LITIGATE

YEARS DEFENDANT ATTORNEYS HAVE BEEN IN PRACTICE	PERCENTAGE OF ALL CASES OF THE MOST FREQUENT TYPE		
	Under 80%	80%-94%	95%+
Less than 10	48%* (12)	64%* (18)	78%* (22)
10 or more but less than 25	48 (42)	55 (46)	64 (37)
25 or more	60 (29)	53 (19)	55 (19)

NOTE.—Data include only personal injury cases.

* Percentage of defendant attorneys who agreed with adversaries' views of the law.

strongest among those who have been in practice for 10 years or less, and does not exist among those who have been in practice for 25 years or more. Similarly, the tendency for fewer years in practice to go along with agreement on the law depends upon the extent to which a lawyer's litigation practice is concentrated in one field. And this tendency is strong only among those whose work is highly concentrated (95% or more of their cases in a single field).

Experience that provides variety, that builds up legal skills—many years in legal work, or a varied litigation practice—makes for more inventive, more flexible responses. Such experience makes defendants' lawyers more able to challenge their adversaries' application of received legal concepts to particular facts. But experience that provides relatively little variety—specialized work in personal injury litigation—builds up stereotyped responses and makes defendants' lawyers less able to challenge their adversaries' application of received doctrine to the facts of a particular case.

MONETARY SIZE OF A SUIT AS A DETERMINANT OF AGREEMENT OR DISAGREEMENT ON THE LAW

The experience and the type of practice of defendants' attorneys also alter the relationship between the amount of money at stake in a suit and the likelihood of disagreement over the law. Defendants' lawyers in personal

injury suits are slightly less likely to agree with their adversaries' views of the law in the larger cases, those in which the potential recovery for the plaintiff is \$10,000 or more, than in the smaller cases. Finding areas of disagreement on the law requires time as well as skill. Some cases are not worth it.

But the amount of money at stake has consequences for agreement on the law that depend, in part, on who the lawyer is. As seen in table 9,

TABLE 9
DEFENDANT ATTORNEYS' AGREEMENT WITH THEIR ADVERSARIES'
VIEWS OF THE LAW BY MONETARY SIZE OF CASE BY TYPE
OF SUIT COMPRISING LARGEST PROPORTION IN
DEFENDANT ATTORNEYS' LITIGATION PRACTICES

TYPE OF SUIT COMPRISING LARGEST PROPORTION	MAXIMUM AMOUNT PLAINTIFF'S ATTORNEY EXPECTED TO RECOVER	
	Less than \$10,000	\$10,000 or More
Personal injury	63%* (98)	48%* (67)
Corporate and commercial	25 (16)	69 (9)

NOTE.—Data include only personal injury cases.
* Percentage of defendant attorneys who agreed with their adversaries' views of the law.

larger potential recoveries make for less agreement on the law only among defendants' lawyers for whom personal injury suits comprise "the largest proportion" of all the suits they had litigated during the preceding year. Among the others, for some reason, the relationship seems to be in just the opposite direction. And, as seen in table 10, money makes the greatest difference for those who have been in practice between 10 and 25 years, and apparently no difference at all for those who have been in practice 25 years or more.

Greater potential recoveries make for less agreement on the law only among those lawyers whose practice or experience would otherwise make them prone to accept their adversaries' views. And, in data not reported here, the greater the potential recovery, the more working days attorneys spend on a case. Both in respect to the amount of effort spent on a case and to inducing defendants' lawyers in personal injury suits who are otherwise prone toward agreement to find points of disagreement with their adversaries' views of the law, money is the enemy of sloth.

THE LEGAL STYLE OF THOUGHT AND DISAGREEMENTS BETWEEN ADVERSARIES OVER THE LAW GOVERNING A SUIT

None of the determinants of agreement or disagreement on the law which are reported above are related to attorneys' answers to the question, "When

TABLE 10

DEFENDANT ATTORNEY'S AGREEMENT WITH THEIR ADVERSARIES'
VIEWS OF THE LAW BY MONETARY SIZE OF CASE BY NUMBER
OF YEARS DEFENDANT ATTORNEYS HAVE BEEN IN PRACTICE

YEARS DEFENDANT ATTORNEYS HAVE BEEN IN PRACTICE	MAXIMUM AMOUNT PLAINTIFF'S ATTORNEY EXPECTED TO RECOVER	
	Less than \$10,000	\$10,000 or More
Less than 10	69%* (32)	60%* (15)
10 or more but less than 25	61 (62)	44 (39)
25 or more	42 (24)	39 (18)

NOTE.—Data include only personal injury cases.

* Percentage of defendant attorneys who agreed with their adversaries' views of the law.

you first analyzed your adversary's factual contentions, did you agree, tend to disagree, or disagree strongly with his view of the facts in this case?" There are few differences between various types of suits with respect to agreement or disagreement on the facts. Nor do the degree of specialization in personal injury or in litigation, years in practice, or the amount of money at stake affect the extent of disagreement on the facts. Instead, the quite different factors noted earlier seem to govern.

On the assumption that processes which govern consensus and dissensus are relative to, and vary with, the characteristics of the culture in question, these findings support the contention that the determinants of agreement on the law reflect a distinctly legal style of thought. Our analysis of these determinants was, in large part, an analysis of issues in the psychology of cognition—of the conditions under which attorneys use concepts broadly or strictly, under which they make fine or blunt distinctions between similar sets of events, under which they do or do not take concepts and the general normative implication of concepts for granted, so as to jump from descriptive facts to normative conclusions in a specific case.

These cognitive processes reflect the workings of a distinctive culture and of a distinctive set of institutions. Ordinary language, the language of primary groups, is syncretic. It blends concepts into sensory perceptions, and statements of value into statements of fact. Parents who say, "You are a good baby," do not regard themselves as applying a general concept to one instance, among many others, of that concept. They regard themselves as making a direct, sensory description of that unique little babe. The language and the logic of policy statements do just the opposite. Policy statements characteristically assume certain goals as desirable and go on, using cognitive language, to recommend means for reaching those goals.

Unlike both the language of ideology and the language of the law, and like the language of science, they do not assert any intrinsic connection, other than a means-ends relationship, between the cognitive and the normative components of a policy document.

The law is quite different. In the logic of the law there are intrinsic connections between the facts as described in ordinary English, the facts as subsumed under legal categories, and the normatively proper outcome. But there is also potential tension, or arguability, in the relations between the facts as described in ordinary language and the facts as described by legal categories.

The adversarial system (insofar as adversaries appeal to established doctrine), appellate review, media for communicating appellate decisions to the bench and to the bar, the formal training of lawyers, and the need of modern enterprises that operate in far-flung markets for predictable social environments, all sustain a venerable tradition of formal, calculable, rational law. That tradition does not blend concepts with sensory perceptions. Quite the contrary. Calculability requires general concepts and general rules.

But social and technological change, conflicting social interests, the adversarial system (insofar as one adversary argues that established categories do not apply to the facts at hand), and the absence of bureaucratic controls over judges in lower courts (which could limit their freedom to vary the application of concepts to instances), often make the application of general concepts to specific events arguable. Hence, potential tension between concepts and instances is built into the social institutions of the law.

The potentially arguable relations between concepts and instances become manifest only if the implicit logical structure of a case—facts, concepts, rules, and conclusions—is explicit in an attorney's own thinking. Thus, everything that tends to make attorneys question, or to enable them to question, the implicit logical structure of an adversary's case—flux, variety, and uncertainty in the relevant areas of law; less stereotyped experience or higher stakes in personal injury litigation; or, in data not reported here, plentiful facts in contract suits—makes disagreement on the law of a case more likely.

The psychology of cognition may have less to do with the dynamics of assent in other types of institutions, types that sustain quite different styles of thought. But given this kind of institution and this style of thought, the psychology of cognition is crucial to the granting or withholding of assent. To this extent, at least, it is possible to claim that rationality is a characteristic of the law, by comparison with many other institutions and styles of thought. But, of course, the rationality of the law is not that of science. Nor is it the rationality of ideology. In the legal

style of thought, it is possible, once rules are given, to move from statements of fact to normative conclusions without invoking values. "It is the law that. . . ." To invoke values along the way is to raise the possibility that existing rules should be called into question, and may imply (psychologically, if not logically) that rules are not valid simply because they exist. But legal thinking can (and often does) proceed from facts to normative conclusions without invoking values. Hence, legal thinking seems to be inherently conservative. Legal culture is tilted in the conservative direction, entirely apart from the ties which lawyers or judges have with the ruling classes and ruling institutions of their society. Nor is the rationality of the law that of ordinary people. Legal institutions, therefore, have an ever-present potential for moving away from the lay sense of justice, unless laymen themselves (such as jurors) hold that movement in check.³⁸

A CONCLUDING NOTE ON IDEOLOGY

For purposes of this paper, our discussion of the logic of ideologies was intended to illuminate what is distinctive about the legal style of thought, as contrasted not with the scientific style but with another, nonscientific style in which it is possible to move from assertions of fact to normative conclusions. It is possible in the legal style of thought, but not in the ideological, to move from facts to their normative implications without invoking any value concepts.

To be sure, the values which legislators, or judges, or litigants hold (along with economic interests, political expediency, and other considerations) may contribute to the choice of desired outcome. Values may have an impact upon the legislative drafting or on the judicial formulation of rules. Once the rules are given, however, and no matter how they came into being, it is possible within the legal style of thought to proceed from statements of fact to their normative implications without invoking values,

³⁸ In data not reported here, it appears that, among plaintiffs both in personal injury cases and in contract suits who did secure recovery, the more important legal (rather than factual) issues were in a suit, the less satisfied the plaintiff was with the outcome, and the more important legal issues were, the smaller the likelihood of agreement between plaintiffs and their attorneys on the satisfactoriness of the outcome. (Attorneys' reports of their clients' satisfaction or dissatisfaction are our only source of data on clients.) It also appears that the more important legal issues are in a suit, the stronger defendants' attorneys think their defenses are, but only when they are otherwise weak (e.g., when they have very little information at their disposal). Legal issues provide a second line of defense for defendants, apparently making it possible, in some unknown percentage of suits, to bring about outcomes which deviate from the lay sense of justice. For some examples of lay rationality correcting the law in criminal litigation (e.g., in effect bringing a doctrine of assumption of risk into the criminal law), see Kalven and Zeisel (1966, chaps. 16-19, pp. 221-97).

as distinct from norms. The contrast with the logic of ideologies helps to make this point clear. And this contrast is the main reason for including a discussion of the ideological style in this paper.

In addition, however, two further points concerning ideologies seem relevant. First, Arne Naess has denied that the concept of ideology is scientifically useful (Naess 1956, pp. 168, 171). It seems possible, however, that if the identification of certain formal characteristics of the legal style of thought had fairly immediate consequences for the analysis of data on civil litigation, then the identification of certain features of the ideological style should also have consequences for research.

It would seem useful, for example, for researchers on attitudes and for sociologists of knowledge consistently to maintain the distinctions among (1) pure statements of value, (2) cognitive terms or ideas to which various value loadings may be attached, and (3) ideological concepts, that is, ideas whose meanings, entirely apart from historically and individually variable value loadings, depend in principle upon the fusion of cognitive and value components. And it would seem useful to study the conditions under which each of these three does and does not enter into public discourse.³⁹

Second, many readers may think of my analysis of the ideological style of thought as a denigration of that style. Those who accept the received wisdom of scientific philosophy may assume that ideological concepts, as defined here, can only represent a failure to distinguish between types of statements—cognitive and noncognitive—which have to be kept apart, and that I have identified the characteristically ideological sort of intellectual confusion.

Nothing could be further from my intention. I have attempted, in the first instance, to identify scientifically certain formal characteristics of two styles of thought which happen to exist in contemporary societies, just as anthropologists study how the natives think. But the question of one's own attitudes toward these styles of thought almost inevitably arises.

There is clearly no reason to assume that scientific styles are somehow more legitimate or valid for all purposes than other styles, that all others are somehow departures or lapses from the scientific style, and that all other styles of thought are to be judged by a scientific model.

³⁹ For example, it seems likely that a strong value consensus in a group, or social circle, or community in which no one publicly challenges that consensus tends toward the use of cognitive terms which have a value loading (in the group or community in question) as distinct from ideological terms, as defined here. When professors in the middle of campus crises discuss possible courses of action, their consideration of some proposals is often cut short automatically when someone points out that a proposed course of action could lead to violence. "Violence" or "violence on campus" are cognitive concepts with a penumbra of value which is widely and strongly shared, so widely and so strongly that the purely cognitive statement "X will lead to violence" does service as a binding recommendation to refrain from doing X.

Scientific thinking seeks to separate inquiring subjects from the objects of their inquiry. Other forms of inquiry seek instead to unite subjects and objects. One of these other forms is ideology. That form allows, simultaneously, for understanding the objective world, for evaluating it, and for acting upon it. For such purposes, an ideological concept such as "institutionalized racism" may be more valid than a more neutral, more scientific concept would be, if "institutionalized racism" more accurately expresses the actual experience of masses of people.

The ideological form of inquiry may subsume both science and the law. It may subsume science because, as noted above, ideologies include, among other components, purely empirical statements. And ideologies subsume law because (following Hart on law as the union of primary and secondary rules) there can in principle be neither legal justification for, nor legal attacks upon, the ultimate "rule of recognition" in a legal system. But there may be ideological ones. Some particular rules of law may be attacked or defended as inconsistent, or consistent, with the supreme law of the land. But the supreme law itself has no legal foundation. The supreme law may be attacked or defended ideologically, but not legally.

These brief allusions to the relative places, as I see them, of scientific, legal, and ideological thought must leave many readers, both lawyers and those with a scientific turn of mind, unsatisfied and unconvinced. But even after lengthy discussion, I suspect, the only ultimate answer to those who doubt the equal legitimacy of quite divergent styles of thought is the words of Paul Eluard: "Let them be regarded as fools, those who say that there are a thousand ways to describe the world."

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Generational Differences among Blacks in the North¹

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A generational classification is virtually a standard procedure for research on European and Asian migrants and their descendants in the United States. An analogous distinction is rarely applied to the southern- and northern-born components of the black population living in the North. Analysis of educational attainment among northern black residents indicates the existence of substantial differences between these two generations with respect to this important characteristic. Likewise, decomposition of the effect of population increase on changes in urban racial segregation indicates that growth in the northern- and southern-born generations has opposite consequences. Therefore, changes in the southern-born proportion among the black population of the North may greatly distort inferences about the causes of long-term shifts in the conditions of blacks living in that region. There are certain implicit theoretical assumptions made when generation is overlooked in research on blacks or any other racial or ethnic group. Although these assumptions may be valid in some contact settings—particularly for later generations—their validity can usually be resolved through empirical research.

It is virtually a standard procedure to distinguish between nativity generations in the study of European migrant groups to the United States. The first generation in the country (the foreign-born) generally differs substantially from the second generation (native whites of foreign parentage) on a variety of characteristics ranging from education and occupation to mastery of English. In turn, the third generation differs from their parents, and so forth. Presumably these differences among the first few generations reflect more than the operation of general societal changes such as creates the gap between parents and their offspring in the society as a whole.²

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² The term "generation" often confuses two distinct meanings. On the one hand, it frequently refers to persons with a common age, such as a birth cohort. The second usage of the term is employed here, however, referring in a genealogical sense to the number of ancestral steps removed from the original migrants into a country or a specified region (see United Nations 1958, p. 6). Even with endogamous marriages, the genera-

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Rather, some of the shifts between the immigrant and later generations reflect special processes of change which in earlier periods at least were described as "assimilation" or "acculturation." Whether this model was totally applicable to the European and Asian migrant groups is subject to a controversy that will not be considered in this paper. However, it is safe to say that there was at least some validity to the model in the United States. To be sure, one must recognize that this process of intergenerational change need not occur (indeed does not in some settings of race and ethnic contact), or it may occur with respect to some characteristics but not others.

A second reason for considering generations stems from the opportunity structure in the areas from which groups emigrate. Migration often involves a change in the opportunity structure for such central characteristics as education, occupation, opportunities for accumulating wealth, and the like. Temporal comparisons of a given racial or ethnic group accordingly require consideration of changes in their generational composition. A comparison between 1900 and 1970 in the educational attainment of all persons of Italian origin in the United States, for example, would be unacceptable if the changing generational composition of the group was not considered. Not only has the United States undergone substantial changes in educational opportunities during this period, but the generational composition of the group has also changed. Presently, a much larger proportion of persons of Italian origin are of second or later generation and therefore exposed to the educational opportunity structure in the United States. Generation is thus a key variable in studies of group changes over time. As a necessary control, generation is no different from such other basic demographic variables as age and sex.

Most studies of northern blacks are insensitive to the generational dimension, although there are exceptions, such as in the recent research of Farley (1970), Crain (1970, 1971), Crain and Weisman (1972), Duncan (1965), and Blau and Duncan (1967). It would be a vast oversimplification to think of blacks who migrate from the South to the North as simply analogous to the foreign-born migrants to the United States. But if there is some utility to such an approach (see, for example, Hauser 1965), then clearly studies of blacks in the North should take their generation into account. In short, the generational distinction is of analytical value if either of the following conditions holds: black migrants to the North differ from those born in the North (either favorably or unfavorably); or there are intergenerational changes between southern-born blacks who live in the North and their northern-born offspring that reflect the operation of more than general societal forces.

tional position of offspring becomes ambiguous when the parents each belong to different generations.

TEMPORAL CHANGES IN THE COMPOSITION OF NORTHERN BLACKS

The migration of blacks from the South to the North is well known, but less fully appreciated is its impact on the generational composition of blacks residing in the North. An examination of the nativity of blacks in the North since 1870 illuminates some of the difficulties in analyzing trends over time. Table 1 gives the percentage of the North's black population

TABLE 1
PERCENTAGE OF NORTHERN BLACK POPULATION
BORN IN THE SOUTH, 1870-1970

Year	Percentage Born in South
1870	32.9
1880	32.1
1890	33.8
1900	38.9
1910	42.0
1920	52.3
1930	58.3
1940	47.1*
1950	50.2*
1960	43.4
1970	32.1

* Data for nonwhite in 1940 and 1950. Comparable percentages for nonwhites in 1930 and 1960 are 50.0 and 37.9, respectively.

who were born in the South by decades beginning with 1870.³ Of the 450,000 blacks living in the North in 1870, about a third were of southern birth. Except for a slight drop in 1880, this percentage increased in each succeeding decade through 1930, when 58.3% of the nearly 2.5 million blacks residing in the North were of southern birth. The number of southern-born nonwhites increased between 1930 and 1940 (as it has in every succeeding decade between 1880 and 1970), but their percentage of the northern nonwhite population declined. This no doubt reflects the impact of the depression decade on migration. A rise in the percentage is observed between 1940 and 1950. However, there was a sharp decline between 1950 and 1960 and again in the most recent decade. Among blacks living in the North, the southern-born component was only 32% in 1970—the same level as in 1880. Because of the massive movement of blacks to the North earlier, in the future the South will probably be a decreasing

³ "South" is defined as the South Atlantic, East South Central, and West South Central divisions; "North" includes all other divisions of the United States. This usage is followed throughout the paper. The relatively small number of foreign-born blacks is excluded from the computations shown in table 1.

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source of black growth in the North, and it is reasonable to expect a further decline in the southern-born component of the northern black population.

The implications of this pattern for the analysis of interdecade changes in the social characteristics of blacks living in the North is generally overlooked. It is important, however, insofar as differences exist between the southern- and northern-born generations residing in the North. If the latter component enjoys various socioeconomic advantages over the former throughout the period, then interdecade comparisons between 1880 and 1930 will tend to underestimate any improvements in the conditions of northern blacks, since the rising percentage with southern birth will depress the results. On the other hand, declines in the southern-born component in recent and probably future decades will tend to overstate the societal sources of improvement among blacks living in the North. If the opposite were to hold such that southern-born blacks in the North enjoy certain advantages over their northern-born compatriots, then the temporal pattern observed above will still cause difficulties. Under such circumstances, however, the earlier decades would overestimate general societal improvements for blacks in the North, and the more recent decades would understate these improvements. In either case, the enormous changes in the relative size of the southern-born component in the North must be taken into account.

EDUCATIONAL DIFFERENCES

The 1960 census provides an unusual opportunity for comparing educational attainment between the northern- and southern-born generations of the black population residing in the North. Normally, the census omits a birthplace classification when reporting the social characteristics of the black population. Therefore, it is usually difficult to determine whether data on blacks living in the North conceal substantial generational differences. The median levels of education for nonwhites living in the North, when decomposed into region of birth (table 2, cols. 1 and 2), are consistently higher for the northern-born segment. The widest gap is a massive two and three-tenths years for nonwhite men 65 and over; the smallest gap occurs among the youngest age group, where the northern-born enjoy advantages of one-tenth and three-tenths years for males and females, respectively. In all cases, however, the northern-born have higher levels of educational attainment than do southern-born residents of the North (the differences are shown in col. 5).

Comparing these medians with the educational figures among the native offspring of immigrants to the United States (almost entirely a population of European origin), one finds that the northern-born nonwhites still have lower levels (compare cols. 2 and 3). Nevertheless, northern-born non-

TABLE 2

EDUCATIONAL DIFFERENCES BETWEEN GROUPS RESIDING IN THE NORTH, 1960

MEDIAN EDUCATION AMONG RESIDENTS OF THE NORTH				NONWHITES: NORTHERN- BORN MINUS SOUTHERN- BORN (COL. 1 — COL. 2) (5)	NATIVE OF FOREIGN OR MIXED PARENTAGE MINUS NORTHERN- BORN NONWHITES (COL. 3 — COL. 2) (6)	
Nonwhites		Natives of Foreign or Mixed Parentage (3)	Blacks (4)			
Born in the South (1)	Born in the North (2)					
Male:						
20-24	11.5	11.6	N.A.	11.5	.1	N.A.
25-34	10.3	11.4	12.4	10.8	1.1	1.0
35-44	8.9	10.9	12.1	9.6	2.0	1.2
45-64	7.5	8.7	9.5	8.1	1.2	.8
65 and over .	5.3	7.6	8.4	6.0	2.3	.8
Female:						
20-24	11.8	12.1	N.A.	12.0	.3	N.A.
25-34	11.0	11.9	12.4	11.4	.9	.5
35-44	9.8	11.1	12.1	10.3	1.3	1.0
45-64	8.2	8.9	9.8	8.4	.7	.9
65 and over .	6.3	7.9	8.6	6.7	1.6	.7

NOTE.—Data in column 2 obtained by subtracting educational distribution for nonwhites specified in column 1 from all blacks living in the North; N.A. = not available. Although medians are reported here and in tables 4 and 5, comparisons based on the entire distribution generate similar conclusions.

whites are closer to the American-born offspring of immigrants than they are to southern-born nonwhites residing in the North (comparisons are given in col. 6). With one exception, women 45-64 years of age, northern-born nonwhites are closer to the American-born offspring of the immigrants. For example, the gap between southern- and northern-born nonwhites in the 25-34 age group among women is nine-tenths; by contrast, the gap between the latter and second-generation offspring of the immigrants is five-tenths.

Studies of the North normally do not distinguish blacks by their region of birth. The figures for all blacks living in the North (col. 4) fall about halfway between the northern- and southern-born components of the nonwhite population. The only exception is for women 20-24 years of age. The medians for all blacks living in the North are, if anything, generally closer to those for the southern-born component. Although the median for a population is not simply a weighted average of its components' medians, normally we would expect it to reflect the relative numbers in each subpopulation. In 1960, among those 25 and over, slightly more than half of both male and female nonwhites living in the North were born in the South (50.6% and 53.3%, respectively).

These generational differences within the northern black population may

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not hold for other important social characteristics or may not be consistent over time. For example, fertility data summarized by Cho, Grabill, and Bogue (1970, pp. 159–61) indicate a less consistent pattern of regional differences in the North after age is standardized (see also Farley 1970, pp. 114–17). Nevertheless, the pattern for education indicates that the absence of generational controls among blacks in the North can greatly distort the results through omission of an important explanatory factor.

RESIDENTIAL SEGREGATION

The generational distinction suggested here may also uncover dramatically different causal effects that are normally masked when the investigator works only with the total black population. This is illustrated by the changes in racial segregation in northern cities between 1940 and 1960. The study is restricted to the 26 northern cities for which data are available on both segregation (Taeuber and Taeuber 1965a, pp. 39–40) and region of birth of nonwhite residents in 1940 (U.S., Bureau of the Census 1944, table 35). These necessary limitations are unfortunate on two counts. Taeuber and Taeuber, in their outstanding study of urban segregation, find the impact of nonwhite population change on segregation is more important during the 1950–60 period than in the preceding decade (1965, p. 75). Unfortunately, region-of-birth data for the nonwhite population are unavailable for cities in 1950. Second, since the Taeuber and Taeuber study was not restricted to the smaller number of cities in the North with region-of-birth data for nonwhites, they were able to employ a more elaborate causal analysis of segregation involving the use of five independent variables. The examination of changes in segregation is limited here to an exposition of the analytical gains obtained when distinctions are made between black generations within the urban North.⁴ A broader multivariate interpretation of segregation changes is not possible.

Between 1940 and 1960, for these 26 cities, the correlation between segregation changes and the percentage change in the nonwhite population is $-.52$.⁵ The regression coefficient of segregation on population change is

⁴ Accordingly, the difference scores employed by Taeuber and Taeuber for describing segregation changes was used here in order to maximize comparability and thereby illustrate the utility of the generational approach. However, the reader should note that a strong argument has been advanced in recent years in favor of decomposing such scores into two separate variables (see, for example, Blau and Duncan 1967 and Hawkes 1972).

⁵ Percentage nonwhite change is measured here in a different manner than that employed by Taeuber and Taeuber, who divide the 1940 nonwhite population by the 1960 nonwhite population. Here it is the nonwhite population in 1960 minus 1940 which is divided by the 1940 figures. This procedure will not alter the results, but it does make it more convenient later to decompose nonwhite increase by generational components.

—1.1. Thus, segregation tends to decline with relatively large increases in the nonwhite population during the period. About 27% of the variance in the dependent variable, changes in segregation, is accounted for by this zero-order correlation. This compares with 9% and 41% of the variance during 1940–50 and 1950–60, respectively, explained by Taeuber and Taeuber with an analogous variable for a much larger number of cities.

The question at hand is whether decomposition of each city's nonwhite population change into its southern- and northern-born components helps to explain changes in segregation. Further, does such a decomposition aid in explaining the linkage between population change and segregation change? The previously used independent variable, $N_{60} - N_{40}/N_{40}$ (where N refers to the nonwhite population in a given city and the subscripts 60 and 40 refer to the years 1960 and 1940, respectively), is decomposed into two variables, s and n , which refer to the southern- and northern-born components. Thus, $(n_{60} - n_{40}/N_{40}) + (s_{60} - s_{40}/N_{40})$ are equal to the original variable, which made no distinction between the region of birth among nonwhites living in northern cities. The zero-order correlations between segregation changes and either northern-born or southern-born population increases are in both cases negative as before (table 3). However, the

TABLE 3
REGRESSION ANALYSIS: CHANGES IN RESIDENTIAL
SEGREGATION IN 26 CITIES AS A FUNCTION OF
NONWHITE POPULATION CHANGE, 1940–60

Variable	r	b
YX_1	-.52	-1.10
YX_2	-.52	-1.10
YX_3	-.44	-1.95
$YX_2 \cdot X_3$	-.54	-5.78
$YX_3 \cdot X_2$38	4.29

NOTE.— Y = change in racial segregation; X_1 = total nonwhite population change; X_2 = northern-born nonwhite population change; X_3 = southern-born nonwhite population change.

partial coefficients are extremely suggestive; segregation tends to go down with increases in the northern-born component, but goes up with increases in the southern-born component of the nonwhite population. The unstandardized partial regression coefficients of segregation changes on the northern- and southern-born are in radically opposite directions, -5.8 and $+4.3$, respectively. Thus, neglect of the generational differences within the black population of the North may mask opposite forces within the black population. The coefficient of multiple determination, R^2 , is .43 for segregation on these two birthplace-specific variables. This is in contrast with the

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r^2 of .27 when changes in segregation are viewed as a simple function of total nonwhite population change. Thus, the variance explained is radically increased when the nonwhites are decomposed by region of birth.⁶

Interpretation of these admittedly "ecological correlations" calls for further data on the specific northern- and southern-born residential patterns in the urban North. To my knowledge, such information is unfortunately not readily available. A comparison of residential location between recent migrants and earlier residents is not entirely suitable, since the two subgroups each include a substantial mixture of both northern- and southern-born blacks. An analysis of migrants to Chicago between 1935 and 1940 found that the small number of blacks from the North and West were less concentrated in the black zone than were those from the South (Freedman 1964, p. 199). One can speculate that the different patterns may reflect substantial socioeconomic differences between northern- and southern-born blacks, which in turn have some bearing on their residential location. Under any circumstances, the inverse relationship between segregation and the percent increase in nonwhites (not distinguished by region of birth) turns out to mask very opposite effects. Increases in the southern-born tend to raise segregation, whereas increases in the northern-born tend to lower racial segregation. The possible bearing of these results on long-term trends and the future outlook is great. However, the point here is to suggest the utility of a generational distinction among blacks in the North for causal analysis.

MIGRATION AND GENERATION

A generational approach to the black population is apt to create certain confusions with migration status that ordinarily do not occur in studies of international migration. Among immigrants to the United States and their descendants, the terms "generation" and "migrant" are easily understood. The migrants to the United States are foreign-born and hence members of the first generation. Their offspring born in the United States are the second generation, and so forth. To be sure, there are certain complications, such as when the parents are of different generations or if the foreign-born children are very young when their families immigrate (see, for example, Price and Zubrzycki 1962). By contrast, black internal migration may inadvertently confuse migration status with generational status. It is clear that blacks in the United States are a group with an average of many generations of descent in the nation. Thus, the first generation of blacks

⁶ In general, one might expect two independent variables to explain more of the variance than just one, but the substantial increase observed here reflects more than chance improvement. Likewise, the two partial correlations differ from each other by more than four times the standard error.

in the North means only that they are of southern birth and now live in the North. Even more important, many of the new migrants to a given northern city are not first-generation blacks as defined here. Many are persons born elsewhere in the North or returning to the city where they were born.

As a consequence, knowledge about the social characteristics of black migrants to major northern urban centers is not the same as knowledge about the characteristics of the southern-born component within this migration flow. It is clear that black in-migrants to a number of leading urban centers between 1955 and 1960 are "of substantially higher socioeconomic status, on the average, than the resident Negro population in a number of large metropolitan areas" (Taeuber and Taeuber 1965*b*, p. 439). However, this important finding need not mean that the southern-born component of the migration stream to a given northern center is of higher status.

Indeed, the advantage migrants enjoy over earlier residents of the North during the 1955-60 period is a function of the intra-northern movement of blacks who were living in the region by 1955. Southern blacks migrating to the North during those five years were actually lower in educational attainment than blacks of comparable age who were living in the North before 1955. Table 4 indicates the median years of school completed for various age- and sex-specific segments of the nonwhite population living in

TABLE 4
MEDIAN YEARS OF SCHOOL COMPLETED AMONG NONWHITES RESIDING IN THE NORTH IN 1960, BY RESIDENCE IN 1955

Sex and Age	Same Northern Division in 1955 and 1960 (1)	Different Northern Division in 1955 (2)	Born in South and Residing in South in 1955 (3)
Male:			
25-29	11.3	12.4	11.4
30-34	10.6	12.2	10.1
35-44	9.9	11.8	8.7
45-54	8.3	9.6	7.2
55-64	7.2	8.3	6.3
Female:			
25-29	11.8	12.5	12.0
30-34	11.3	12.3	10.7
35-44	10.4	12.0	9.1
45-54	8.7	9.6	8.0
55-64	7.9	8.5	6.9

SOURCE.—U.S., Bureau of the Census (1963, table 8).

NOTE.—Data for column 3 are estimated under the assumption that persons migrating from the South in 1955 to the North in 1960 were born in the South (except those whose birthplace is specified as the same as the division of residence in the North in 1960). The actual numbers of southern-born nonwhites who migrated from the South to the North between 1955 and 1960 are available by age, but not by education, in table 5 of the source cited above. These actual numbers are from 93.5% to 98.4% of the estimated figures which specify education.

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the North in 1960. Columns 1 and 2 indicate the educational level of nonwhites who were living in the North during both 1955 and 1960. However, column 1 refers to those who had not changed divisions within the North during the period. By contrast, column 2 gives the educational attainment of nonwhites who had changed divisions within the North between 1955 and 1960. The substantial differences between the two columns indicate that movers between northern divisions were consistently higher in education than those who did not shift. Among women, for example, those who had not changed northern divisions during the five-year period were from six-tenths to one and six-tenths years lower in education than women of comparable age who had changed divisions within the North.

By contrast, the estimated education of southern-born in-migrants from the South during this period was considerably lower than of those who had changed divisions within the North during this period (compare col. 3 with col. 2). Even more to the point, the southern-born in-migrants were also lower than nonwhites who had remained in the same northern division since 1955. (There is an exception, however, for the 25-29-year-old group.) Thus, the apparent superiority of black education in the North through in-migration was due to the circulation of blacks from one northern geographical division to another during the 1955-60 period. Southern-born in-migrants to the North from the South during the same time had lower median levels of education than did those of comparable age and sex who were already living in the North in 1955. Indeed, these in-migrants tend to be lower in education than nonwhites who remained in the division of the North during this period.

THE OMISSION OF GENERATION: AN EXPLANATION

Given the widespread usage of the generational variable in studies of European and other immigrant groups, why is the generational factor so uncommon in studies of blacks? This question is particularly puzzling since the results presented above suggest that generational effects may be very important. In one sense, it is impossible to answer this question since there may be as many reasons for omission as there are studies of blacks. The factors may range from budgetary limitations to its actual (or assumed) insignificance as a variable to something as prosaic as oversight. But if it is impossible to answer "why," one can nevertheless determine the implicit assumptions made when generation is excluded from analyses of racial (or ethnic) groups. A clarification of the rationale for tabulating a given social characteristic by age race or ethnic group, and generation (either singly or in various combinations of these three attributes) will help illuminate the theoretical issues underlying a researcher's decision to exclude or include a generational variable.

In nearly all cases of contact between ethnic groups, there will be no more than a partial overlap between age and generation attributes. This holds for both international migration (where generation refers to descent from a foreign origin) and for internal migration (where generation refers to a nation's subdivisions). In the early stages of a migration process, one can at least visualize an ethnic group arriving which consists exclusively of certain ages. Under such a circumstance, knowledge of a subject's ethnic or racial group would make age and generation superfluous. But normally one can anticipate that a cross-tabulation of age and generation within a given ethnic group will not produce any empty cells. In other words, members of a given ethnic group with a common age will belong to different generations; others will have a common generation but differ in age. To be sure, the age and generation factors will not be independent for a given group. In 1960, the median age for first- and second-generation Yugoslavian men in the United States was 60 and 38 years, respectively. Likewise, among nonwhite women living in the North, 30% of those between 15 and 19 years of age were born in the South, compared with 61% among those 40-44 years of age. However, the cross-tabulations between age and generation will generally have entries in all cells. Normally, only in the decade when the first wave of migration occurs will there be even the slightest grounds for expecting total redundancy in cross-tabulations between generation and age for a given group. Accordingly, the researcher is generally confident that age and generation effects, if present, will not fully overlap.

THE MEANING OF AGE, GROUP, AND GENERATION

Age.—The use of an age variable in social research is so widespread and valuable that its rationale is rarely stated. Essentially, one tabulates a social characteristic by age in order to take into account (or study) differences within a population that occur as a consequence of either changes within the society that are reflected in different age cohorts (see Ryder 1965) or because there are age-related phenomena that would be expected even within the most extreme example of what Ogburn called "a stationary society" (1964). Examples of both usages come to mind readily. If the educational opportunities are changing in a society, this will be reflected in the years of school completed among successive cohorts. Or for behavior regarding marijuana changes, we might well expect to find the exposure varying by age groups. But even where the society is totally static, there are age-specific probabilities associated with a wide range of phenomena ranging from such demographic processes as mortality, marriage, and fertility to social psychological concerns with attitudes.

In either case, a cross-sectional tabulation between age and the relevant characteristics will provide the investigator with a rough control on both

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the age-related differences that are a reflection of changes in the society and those which are found under conditions of societal stability. In passing, one should note that a simple cross-tabulation by age for any one time period does not allow the investigator to distinguish between these two very different processes.⁷ Under any circumstances, age may be either a control for the investigator to get at certain research questions in which age differences "interfere," or age differences may be a direct focus of the research goal.

Racial or ethnic groups.—Groups of this type are normally studied because they are of intrinsic interest in themselves or because it is important to take group differences into account as a "control" when dealing with other phenomena. In either case, it is generally assumed that the groups differ from one another at least in part because of forces imposed by the larger society, for example, discrimination, and/or because of certain internal characteristics such as values, personality, language, or other cultural characteristics. In turn, these differences are presumably not fully explained by age or other variables that cut across different racial or ethnic groups.

Generation.—Application of a generational distinction to a racial or ethnic group implies certain subdivisions within the group that are not simply a reflection of age, sex, or other universal categories. More specifically, the generational distinction is applied when it is assumed that the characteristics of the group are in some way related to the genealogical distance of their descent from a certain point of origin: a foreign country in the case of international migrants and their descendants; the southern region in the case of blacks living in the North. Two well-known illustrations of this generational effect are immigrant groups in the United States and the impact of rural origins on fertility in urban centers. In the case of European migrant groups, it is rather obvious that members of the same ethnic group will differ by generation for many characteristics after age is taken into account. Among persons of Norwegian origin in the United States, for example, we would expect a sharp difference between first, second, and later generations in their mother tongue composition. Regarding fertility, Goldberg has demonstrated a generational effect in Detroit that was stronger than even socioeconomic status (1959). This occurs when male residents are distinguished in terms "of two-generation urbanites" and "farm migrants" based on their fathers' background.

There are two basic conditions under which a generational factor is relevant. First, the generational factor will be used if the culture, personality, values, or social knowledge (for example, information about job opportunities) is related to genealogical descent within a group. Hence, its omission implies that such characteristics are unrelated to generation or are insigni-

⁷ Temporal data are required on two or more succeeding cohorts as in, for example, Lieberman (1965).

nificant causal factors. Second, at least the first two generations will be relevant if life chances are affected by location. Under such circumstances, the immigrant and second-generation populations in a given area must be distinguished, albeit there may be no reason to specify various later generations. The life chances of adult immigrants from a foreign country are presumably different from those of their compatriots of the same age who were born in the receiving nation. Thus, the omission of generation implies that the life chances of blacks who migrated from their region of southern birth to the North as adults are the same as those of northern-born compatriots of the same age.⁸ Admittedly, in all regions the life chances of blacks have been severely hampered by discrimination, but the omission of generational considerations implies that the forms and intensity of discrimination have been uniform in all regions.

The utility of a generational approach to cultural and social psychological characteristics is illustrated by a striking finding in the Kerner Commission report. According to surveys following the Detroit and Newark race riots, blacks raised in the North were far more likely to riot than were black residents who had been raised in the South. In Newark, three-quarters of the rioters were raised in the North, compared with little more than half of those not involved. The differences are even greater among blacks in Detroit. Again, three-fourths of the rioters were raised in the North, but in this case little more than a third of those not involved had been raised in the North (National Advisory Commission on Civil Disorders 1968, p. 332).

The danger of omitting generational effects on life chances is demonstrated in a study of socioeconomic changes among various nonwhite groups between 1940 and 1960 in the United States (Schmid and Nobbe 1965). A remarkable upward movement in education is reported for males 25 years of age and older of Japanese origin. Their median jumps from 8.8 to 12.4 during a 20-year span. However, no consideration was given to the generational effect operating during this period. As table 5 makes clear, almost all of this change in educational attainment among Japanese men is to be explained in generational terms. First, due to the absence of immigration, the percentage of foreign-born among Japanese males declined from 80 to 27 during this period. Second, there was remarkably little change in the educational medians among the foreign- and American-born generations during this period. For the foreign-born component, the median increased from 8.3 to 8.8 years during this period; for the American-born, the median increased from 12.2 to 12.4 years. Hence, almost all of the change was simply due to shifts in generational composition among

⁸ A more elegant analysis would also require that the first generation also be distinguished by the age at which they migrated.

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TABLE 5

EDUCATIONAL ATTAINMENT OF CHINESE AND JAPANESE IN THE UNITED STATES,
25 YEARS OF AGE AND OLDER, BY SEX AND GENERATION, 1940-60

ETHNIC GROUP BY SEX AND BIRTHPLACE	MEDIAN EDUCATION		PERCENTAGE FOREIGN-BORN	
	1940	1960	1940	1960
Japanese:				
Male:	8.8	12.5	80	27
Foreign	8.3	8.8
United States	12.2	12.4
Female:	8.6	12.4	76	37
Foreign	8.1	10.5
United States	12.1	12.3
Chinese:				
Male:	5.6	9.2	68	62
Foreign	5.3	8.3
United States	6.2	12.3
Female:	5.0	10.7	53	53
Foreign	1.6	7.3
United States	8.6	12.4

NOTE.—Hawaii and Alaska excluded in 1940, but included in 1960 (except for group figures unspecified by nativity).

Japanese in the United States.⁹ The enormous jump from 8.6 to 12.4 for women of Japanese origin is also to be explained in similar fashion. The percentage of foreign born among Japanese women declined from 76 to 37. The educational median for Japanese women born in the United States increased only slightly during the period from 12.1 to 12.3; the sharper increase was actually among Japanese women of foreign birth, from 8.1 to 10.5. (This jump may reflect the influence of Japanese war brides arriving after World War II.)

In fairness, one should note that the changes for Chinese educational attainment during this period are not to be explained in the same terms. For example, the median education for American-born Chinese males in 1940 was 6.2. All this says is that generational effects may vary, but they are still relevant. Namely, the absence or weakness of a generational effect, when empirically demonstrated, helps to reject certain hypotheses about the sources of a given racial or ethnic group's position. The point of this illustration with Japanese educational data is clear-cut. The absence of a generational control may create misleading interpretations of the causes and nature of a group's changes over time.

⁹ Because of the absence of age- and nativity-specific educational data for 1940, it is not possible to directly control for changes in age composition within the generations between 1940 and 1960. However, application of the 1960 educational rates to the 1940 age composition indicates that changes in age composition are of relatively minor consequence.

In brief, the investigator who fails to classify blacks in the North by generation implies two phenomena. First, there are no differences in life chances within the group due to the differential intensity and range of discrimination in the subregions of the United States. Second, the internal characteristics of blacks are unrelated to generation or, if related, have no consequence for the phenomenon under study. If life chances are different or if the culture, personality, and other internal attributes of blacks are related to nativity, then generation should be considered. Tabulations by age are generally not a suitable substitute for generational controls as defined here in terms of genealogy.

COMMENT

The results presented in this paper indicate that generation is of substantial consequence for the analysis of at least some important characteristics of blacks residing in the North. Researchers, as well as the Census Bureau, should give serious consideration to the use of a generational cross-tabulation in studies of northern blacks. However, a word of caution is in order. The generational effect need not be present or of much consequence for all phenomena. For example, Crain and Weisman find that generational effects are rather minimal for several social psychological attributes (1972). Moreover, the available data do not permit separation of a generational effect from that due to the greater frequency of rural origins among blacks born in the South. Analysis of rural-urban differences among the southern-born migrants should indicate how much of the generational effect is due to the rural component within the southern-born population. Nevertheless, by making explicit the assumptions underlying the usage of a generational factor in research, at the very least one can see how its omission implies certain untested hypotheses of causality.

The author recognizes that polemical misunderstandings may arise from this thesis. Namely, it may be seen as an argument to view the black situation in the North as simply analogous to that faced by European and Asian immigrants and their descendants. Nothing could be further from the truth than such a gross and oversimplified view (see, for example, the results of Taeuber and Taeuber 1964; Lieberman 1963, 120-32). The goal of this paper, however, is to draw attention to the possible operation of these effects. Whether generational distinctions are relevant for those further removed by genealogy from southern origins is yet an open question. For example, I know of no data comparing second generation northern blacks with those in the third generation.¹⁰

¹⁰ The author is currently involved in a project which will determine the generational effect for a larger number of dependent variables as well as between second and third generations in the North.

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In short, intergenerational controls are more or less a standard procedure in dealing with immigrant groups and their offspring. The same should be the case for blacks residing in the North.

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The Preindustrial Family in America: A Further Examination of Early Magazines

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This research parallels a previous investigation of the authors of pre-industrial American family patterns during the 1741-91 period. The present investigation involves a content analysis of colonial magazines for 1794-1825. Special attention is given to power patterns, types of attitudes between the sexes, actions toward premarital and extra-marital sex involvement, and motivations for entering the marital relationship. The results concerning overt power among males and females that could be subjected to statistical examination in both studies exhibited an almost identical pattern. There was evidence of general existent and advocated overt male power, although such male power was mediated by other forms of female power as follows: there was evidence of existent overt female power and subtle female power in both courtship and the home. There was also evidence of advocated mutual cooperation between the sexes and extended role activities of the female. This latter trend in the direction of status and power of the women in the 1794-1825 period shows a general change over the period of the earlier study. The evidence regarding romantic love in the fictional material occurred to a greater extent than in our first study. The nonfictional discussions of romantic love and motivations were noticeably less frequent in this study than in the prior study. There were very few direct or indirect discussions of sexual standards. This contrasts with the results of our previous study. It is suggested that the family during the period 1794-1825 was influenced by ongoing internal structural changes, the consequences of the conditions of settlement in America, and humanistic and ideological beliefs which had their roots both in Europe and America. These influences are explored along with their implications for the analysis of changing family structures in historic perspective.

In recent years, investigators from several fields have raised some basic questions about the structure of the American family and the changes that have occurred through history (Wilkinson 1962, pp. 678-82; Goode 1963, chap. 1; Mott 1965, pp. 294-95; Furstenberg 1966, pp. 326-27;

Udry 1966, pp. 19–22; Greenfield 1967, pp. 312–22; Lantz et al. 1968). When the research is synthesized, the following major points emerge: the relationship between dynamic internal familial factors and external cultural structural variables, such as industrialization and urbanization, is still very much under scrutiny; there is a need to move the analysis of family change from the macroscopic to the microscopic level, and finally, to effectively measure family change and to interpret social change it is necessary to establish some base lines in history (see especially Parish and Schwartz 1972, pp. 154–73).

Most of the mass of material written about the American family lacks systematic and cumulative research. Record keeping in the early part of our history was poor, and professional historians were virtually nonexistent. A good deal of the work on the early American family is based on the writings of Calhoun (1917), but much of his analysis involves limited secondary sources, observations of judges and political leaders of the period who commented on the family. Thus, there is virtually no information to provide baselines for any important group of family patterns.

In our research, we have been attempting to design some necessary baselines in history. This research parallels a previous investigation of preindustrial American family patterns during the 1741–94 period (Lantz et al. 1968). Both investigations analyze the contents of magazines to examine selected aspects of preindustrial American family patterns during the 1794–1825 period. In both studies, we examined power patterns present in the man-woman relationship, attitudes toward and actions involved in premarital and extramarital sexual involvement, and motivations for entering the marital relationship as described in the magazines.

The boundaries of the two periods described were defined by Frank Luther Mott (1930) in his work, *A History of American Magazines: 1741–1850*, and the important magazines that he lists in each period provided our basic data. This study covers more of the post revolutionary era, a fact that becomes relevant in assessing the results.

Mott (1930) has characterized the period of 1794–1825 as one of nationalism in the literary world. There was apparently an intense desire to achieve a national literature at this time. Numerous new magazines were introduced that covered a wide variety of topics but typically did not last long. Lewis (1961, p. 17) observes that during 1800–1810 there were “thirty-four religious magazines, five comic periodicals, seven medical journals; there were also two magazines in mathematics, one for those who practiced the law, and five specifically designed for the theater-goer . . . there were nearly sixty periodicals of such a general nature that we call them miscellanies, or general magazines.”

According to Mott, the comparatively successful general magazines in the earlier years of this period were almost all weeklies, of which the most

important was *The Port Folio*, published in Philadelphia. It began in 1801 and ran until 1825 and was published as a weekly until 1809, when it became a monthly magazine. Second only to *The Port Folio* in importance during this period was the series of periodicals edited by Charles Brockden Brown. This series began with *The Monthly Magazine, and American Review* in 1799, followed by *The American Review, and Literary Journal* in 1801, and *The Monthly Anthology* in 1803.

The American Review was the first successful American quarterly. There were also some purely eclectic magazines during this period that were composed wholly or in part of extracts from British periodicals apparently because original contributions were not to be had. The period 1794-1825 was also a prolific one for the founding of religious magazines and for religious discussions in secular magazines.

Several periodicals for women began in the early years of the 19th century, but most were feeble and lasted only a short time. There were no magazines in Mott's list of influentials that were devoted exclusively to ladies, but *The New York Mirror* did contain a section that was aimed at the female population to a greater degree than were any of the other periodicals in this listing.

The magazines were generally published in the Northeast and New England; most were printed in New York, Philadelphia, and Boston. While the majority were published in the Northeast, their distribution was wider than that of the previous period in which most were published in New England. With the postal act of 1794, which enabled publishers to send magazines through the mail, circulation rose.¹ Subscribers probably constituted a rather select sample, especially since the content was aimed at a middle- and upper-class audience of readers, with many of the stories and articles about fashionable society.

LITERARY SOURCES AS DATA

The use of literary sources as data for social science is always subject to criticism. Yet, as Coser suggests, "Literature, though it may also be many other things, is social evidence and testimony. It is a continuous commentary on manners and morals. . . . While the examination of literature cannot replace systematically accumulated, certified knowledge, it can provide the social scientist with a wealth of sociologically relevant material, with manifold clues and points of departure for sociological theory and research" (1972). In spite of such positive recommendations, there are serious limitations to the use of literature as data. Content analysis is especially hazardous when literature from the past is examined. We may be

¹ For an excellent discussion of American magazines during the first decade of the 19th century, see Lewis (1961).

dealing with statements written in unfamiliar contexts, with outmoded language symbols that are not easily understood. In addition, there is the special problem of attempting to generalize from literary sources to social behavior. We do not know whether the concerns and behavior of a population, as described in literature, do in fact coincide with real behavior. While we can assume that particular themes and discussions do indeed represent the concerns and interests of a population, it is certainly hazardous to make generalizations about behavior. We cannot tell with certainty how such concerns influence the behavior of people, nor can we tell how many were influenced.

It may be that behavior patterns described in literary sources are precursors of what emerges later and do not truly represent behavior in the period in which they are described. But this problem could also apply to any type of survey data. There is always the problem of credibility. All that may be said is that insofar as literary data reflect a period in the future, they may still be examined as a forerunner of change and that they are one source of data to be examined for clues to the history of a period.

SOURCE, METHOD, AND PROCEDURE

Mott (1930, pp. 215-335) sketched 27 important magazines for the period being considered, 1794-1825.² Of these magazines, 20 were included in our analysis,³ three magazines had specialized content that was clearly not

² It should be emphasized that our populations in both studies consisted of all issues of the magazines designed to be important by Mott. The common denominator between studies is the importance of the magazines! It is only because of the work of Mott that the reasonable and very significant assumption can be made that the most influential magazines of each period have been studied. We do not regard the fact that the character and numbers of these magazines changed somewhat from 1741-94 to 1794-1825 as a contaminating factor. In fact, given that our populations consist of all of the important magazines of each period, our research design had to allow for this very possibility. For a more detailed discussion of the historical periods set off by Mott, the nature of a magazine, and the character of an important magazine, see Lantz et al. (1968, pp. 414-16).

³ These 20 magazines were: *The Monthly Magazine, and American Review*, April 1799-December 1800 (Mott [1830, pp. 218-22] also discussed several successors to this magazine. One of them was included in our analysis—*The American Review, and Literary Journal*, January-March 1801 to October-December 1802); *The Port Folio*, January 1801-December 1827; *The Boston Weekly Magazine; the Emerald*, October 1802-October 1808; *The Monthly Anthology and Boston Review*, November 1803-June 1811; *The Monthly Register and Review of the United States*, January 1805-December 1807; *Niles' Weekly Register*, September 1811-September 1849; *The American Review*, January 1811-October 1812 (Mott [1930, pp. 271-76] also discusses a successor to this magazine, *The American Quarterly Review*. This magazine was not included in our sample as it was not published until after 1825); *The General Repository and Review*, January 1812-October 1813; *The Analectic Magazine, and the Literary Gazette*, January 1813-December 1821; *Christian Disciple*, May 1813 to November-December 1823 (Mott [1930, pp. 284-92] also discusses a successor to this

reflective of family patterns;⁴ three magazines could not be located,⁵ and one magazine emphasized foreign literature.⁶ The universe consisted of all issues of the 20 magazines that were published during 1794–1825. Approximately all of these issues were examined in order to eliminate any sampling error with respect to the selection of issues.⁷ A total of 2,373 issues was considered.

The tables of contents that were cataloged in the approximately 200 volumes containing the issues were systematically examined. Article titles were then scanned to see if the article depicted man-woman relationships during 1794–1825.⁸ In contrast to the first study, the selection of articles

magazine, *The Christian Examiner*. This magazine was not included in our sample because its publication dates were January–February 1824–November 1869); *The Portico*, January 1816–April, May, and June 1818; *The American Monthly Magazine and Critical Review*, May 1817–April 1819; *The Methodist Review*, January 1818–current; *The Western Review*, August 1819–July 1821; *The Literary and Scientific Repository*, June 1820–May 1822; *The Christian Advocate*, January 1823–December 1834; *The New England Farmer*, August 1822–June 1846; *The New York Mirror*, August 1823–57 (no month); *The United States Literary Gazette*, April 1824–September 1826 (Mott [1930, pp. 331–33] also discusses a successor to this magazine, *The United States Review and Literary Gazette*. This magazine was not included in our sample because its publication did not begin until after 1825.); and *The Atlantic Magazine*, May 1824–April 1825 (Mott [1930, pp. 334–35] also discusses a successor to this magazine, *The New York Review and Atheneum Magazine*. This magazine was not included in our sample because its publication dates were June 1825–May 1826). It should be observed that the titles of these magazines were often modified over time. We have simply listed the abbreviated title employed by Mott to describe the magazine from its inception to its termination. For an exact citation of the various titles, see Mott (1930, pp. 215–335). The reader should also note that while we are concerned with the 1794–1825 period, the majority of these magazines were published between 1800 and 1825.

⁴ These magazines were: *The Medical Repository*, August 1797–1824 (no month); *The American Mineralogical Journal*, January 1810–January 1814; and *The American Journal of Science*, July 1818–current.

⁵ These magazines were: *The American Baptist Magazine*, September 1803–December 1909; *The Panoplist*, June 1805–December 1820; *The Christian Spectator*, January 1819–November 1838. (It is of interest that the religious magazines in our sample of 20 contributed very few frequencies to any of the categories of the tables.) They were not included in the *American Periodical Series*, the microfilm series utilized in this study.

⁶ This magazine was *The Eclectic Magazine* (see Mott 1930, pp. 306–9). In fact, it was published in England.

⁷ One of the important magazines listed in our 20 (Mott 1930, pp. 247–50) was *The Boston Weekly Magazine*; *The Emerald*, published from October 1802–1808 (no month). The issues from October 1802 to April 1806 could not be located. The issues published between May 1806 and October 1808 were included in our sample. Another important magazine in our set of 20 was *The Analectic Magazine*, and *The Literary Gazette*, published from January 1813 to December 1821 (Mott 1930, pp. 279–83). The one volume (the third series) for the year 1821 could not be located.

⁸ Article is used here in a broad sense. The issues did not contain numerous articles in the current sense of the term, but poems, letters to the editor, and a variety of other material were also included.

was not limited to those titles that pertained to women; if the title suggested that it might contain discussion(s) of the variables we were coding, it was included. Our experience with the magazines indicated that this slightly broader criterion was a more sensible one to employ.⁹

Although we were guided to some extent by titles of articles, we found it necessary to read the first pages of the articles because titles were not always informative enough. If this page contained information on one of the variables we were coding, the article was more carefully examined and the appropriate tabulations were made. If the themes were not found on the first page, the article was dismissed.

The unit of analysis was the explicit discussion of one of the variables. While more than one discussion of the same variable could come from the same article, this seldom happened except in the case of the romantic love variable. A schedule was used that allowed for the duplication of the five tables generated in the initial study. These tables concern power in the man-woman relationship, romantic love, motivations for marriage, and advocated and actual sanctions implemented toward individuals involved in premarital or extramarital sexual relationships. The exact character of the categories of these variables is detailed later.

Our basic analysis involved the examination of these five contingency tables. The χ^2 goodness-of-fit test with a correction for continuity was used to facilitate various comparisons within these tables.¹⁰ A two-tailed region of rejection and the .05 level of significance were used. In some instances, this test could not be employed because the size of the expected frequencies fell below five.

Reliability was approached in two ways. The criteria for classification were applied in a consistent manner.¹¹ The schedule and the specific categories of the variables depicted in our five tables facilitated this procedure. We also considered the magazine content at two points in time. The

⁹ This broader criterion was deliberately adopted in order that the probability of locating evidence of patterns not typically attributed to the American family prior to industrialization would be maximized. However, it should be noted that an estimated 95% of the articles selected in this study would have also been included if the procedure followed in the initial study had been scrupulously duplicated.

¹⁰ Our data did not represent a random sample, as the entire universe of issues was included in our sample. But, as in our first study we believed that it was legitimate to use tests of significance as this universe of important magazines could realistically be viewed as a random sample of the hypothetical universe of all important material published during the period (see Hagood [1941, chap. 17]). There is also the problem of the independence of our discussions, a recurring problem in content analysis. Some discussions did come from the same article. However, this point should not be over-emphasized, since it was only the romantic love discussions that came from the same article with any regularity. And χ^2 was only employed in one instance in regard to these data.

¹¹ For a discussion of Sebold's observations on this approach, see Lantz et al. (1968, p. 416).

tables of contents were subjected to two independent examinations, and the selected material was given appropriate consideration in both instances. The two content analyses were performed by the same coder; approximately four months elapsed between the two examinations. Only the manifest content of the material was considered.

THE RESULTS

Our findings are organized around the traditional conceptions that the early American family was basically patriarchal with essentially economic-materialistic concerns. However, our first study, along with other evidence, suggests that changes were underway, and our presentation is designed to provide data to indicate what those changes were and what their direction was. To show these changes, we established in our initial study a historical baseline for the earlier period, 1741-94. Moreover, it is necessary to point out certain patterns of family life perhaps unknown to sociohistorical investigators. This is especially the case when such patterns appear in successive historical periods. This initial period can then be compared with the period of this second report, and similarities or differences can be noted. Statistical frequencies, imperfect though they be, have their greatest meaning when different time periods are compared. Our findings take on additional meaning in this context. We want again to point out the limits and the strengths of examining these contents. We believe that writings of a period reflect the life styles of that period as well as wished for, anticipated, or exaggerated life styles imagined by the authors. But all are important!

Power in the Man-Woman Relationship

Various aspects of the power relationship between males and females during 1794-1825 were investigated. Power was defined as the ability of one individual to dominate another or others, to coerce and control them, obtain obedience, interfere with their freedom, and compel their action in particular ways. Power was coded as being exerted overtly by the male, overtly by the female, subtly by the female, or by males and females cooperatively¹² Existent and advocated discussions of power were coded in one of five categories, general discussions of power, that is, explicit discussions of power that did not pertain to a particular form of behavior; power over morality, that is, power over sexual, gambling, or drinking, behaviors:

¹² Instances of subtle male power were almost never discussed. The magazine content indicated that males were operating in a patriarchal system and could demonstrate their dominance openly. Husbands had legitimate and publicly recognized channels for expressing overt power.

power in the courtship situation; power in the handling of finances; and power in certain other situations, that is, child rearing, minor decision making, etc. The power data are presented in table 1.

Consideration was initially given to comparing those discussions in which power was exerted overtly by the male and the female. There were 33 discussions of this type in the male category and 15 in the female category for all five types of power. The resultant p of the χ^2 test was less than .02. When the male-female totals were examined in the five "exists" columns of table 1, it was found that there were significantly more discussions involving the male in the general power category ($p < .001$); there was no statistical difference at the .05 level between males and females in the courtship category, and the three remaining totals could not be tested.

Our attention next turned to the five columns containing discussions "advocating" that overt power should be held by the male or the female. A meaningful statistical comparison could not be made between the male-female totals in any of these columns. There were 12 discussions across all five columns, however, advocating that the male should have overt power and zero discussions advocating that the female should have overt power. This difference was significant at the .01 level.

Thus far, the analysis lends support to the traditional patriarchal view of the male-female power relationship prior to industrialization. It is of particular interest that the results concerning differences in the discussions reflecting overt power among males and females that could be subject to statistical examination in both studies exhibited an almost identical pattern.¹³ We now shift our attention to subtle female power.

Subtle female power, as in the initial study, refers to situations where the female is in control, but where the male may not be fully aware that he is being manipulated. Since we were basically interested in the overall power position of the woman relative to the power position of the male, the discussions that indicated either overt or subtle female power were compared with those discussions that indicated overt male power.¹⁴

There were 33 discussions of male overt power and 50 discussions of female overt and subtle power across all five power categories. The resultant p of the χ^2 test was not significant at the .05 level. When the male-female totals were examined in the five "exists" columns of table 1, it was found that there were significantly more discussions involving the male in the general power category ($p < .01$) and significantly more female discussions

¹³ This is also true of the following analysis of female overt and subtle power to male overt power. The interested reader may further explore these similarities by comparing the results of this study with those of the first study.

¹⁴ These combined totals are not presented in table 1. The reader, however, can easily obtain them by employing the data in this table.

TABLE 1
 VARIOUS DIMENSIONS OF POWER IN THE MAN-WOMAN RELATIONSHIP AND SOURCE OF POWER BY CHARACTER OF DISCUSSION (FICTION OR NONFICTION), STATE OF POWER (ADVOCATED OR EXISTENT), AND TIME PERIOD (IN FREQUENCY OF DISCUSSION)

SOURCE OF POWER	POWER IN GENERAL				POWER OVER MORALITY				POWER IN COURTSHIP				POWER OVER FINANCES				POWER IN OTHER AREAS			
	1794-1825				1794-1825				1794-1825				1794-1825				1794-1825			
	Advocated				Advocated				Advocated				Advocated				Advocated			
	F*	N†	T‡	SUM	F	N	T	SUM	F	N	T	SUM	F	N	T	SUM	F	N	T	SUM
Overly by male	2	6	8	16	0	0	0	0	1	0	1	2	0	0	0	0	1	0	3	4
Overly by female	0	0	0	0	0	0	0	0	1	1	2	4	0	4	9	13	0	0	0	0
Subtly by female	0	0	0	0	0	1	1	2	2	0	3	5	0	0	0	0	0	0	0	0
Mutual cooperation	5	7	12	24	1	2	3	6	0	1	1	2	0	0	0	0	4	9	13	26
Total	7	13	20	40	1	3	4	8	4	1	5	30	9	39	0	0	4	12	16	33

* F is fiction.
 † N is nonfiction.
 ‡ T is total

in the courtship category ($p < .001$). The remaining three categories could not be meaningfully tested.¹⁵

Consideration was also given to the male overt power totals and the female overt and subtle power totals in the five "advocates" power columns in table 1. None of these differences could be meaningfully tested. There were, however, 12 discussions indicating overt male power and four discussions depicting overt or subtle female power across all five power categories, the latter all being in the subtle female power categories. This difference was not significantly different at the .05 level.

This magazine analysis of female overt and subtle power suggests that the female was able to counter overt male power through these power forms. There was evidence of existent overt female power, especially in courtship and to some extent in the home situation. The magazine content suggests that power was especially characteristic of middle- and upper-class women. Women had the final choice of whether to accept a marriage proposal, and they could choose among their suitors. Women were also offered legal protection from would-be suitors who had no true intention of marriage. These men were often sued for breach of marriage promise, and the women were usually awarded damages. It also appears that the females' role position in the home afforded her overt power in some instances, and there was evidence of subtle female power in courtship and domestic situations, although much of this evidence came from fiction.¹⁶ The female, however, was frequently described as playing the coquette, as well as being fickle and inconsistent in her love.

The ability of the female to exert power during this period is also indicated by our data on mutual cooperation. An examination of the row totals in table 1 reveals that 71% of the total number of discussions were characterized by something other than overt male power!¹⁷ Insofar as one can draw conclusions from a content analysis of literature, power of the female may possibly have been in ascendance during the 1794-1825 period. There is always the question of the meaning of subtle female power, since it could be argued that in a patriarchal structure, whatever power the female has must be subtle. As such, the existence of subtle female power cannot be employed to indicate that a patriarchal pattern does not exist. There are several responses to be made. First, the authors do not argue that a

¹⁵ It is impossible to say anything compelling about those categories that do not have adequate discussions for analysis. Nevertheless, from a content-analysis perspective, the female "comes off as well" as the male.

¹⁶ It should be observed that 27 of the 35 discussions of subtle female power in table 1 occurred in fiction. A significant number of these discussions, however, were in fictional stories and essays rather than in poetry. But if one takes the perspective that literary discussions can be anticipators of change, the fictional "bias" takes on less significance.

¹⁷ The reader may calculate these percentages by using the row sums in table 1.

patriarchal family was not prevalent. We are suggesting only that our data may provide evidence that internal family changes regarding the reallocation of power were underway. Second, if power, subtle or overt, is defined as the ability to move others, were there some respects in which the patriarchal structure existed only in some formal sense? And that in these instances the women were basically in control? Can such a family still be identified as patriarchal? Later in our history subtle power was supplanted by more open patterns of assertion and persuasion. Was this necessarily a change in actual power, or a change in the form in which power was expressed, or both? These are questions to be raised; our study offers no answer.

A comparison of the total number of discussions occurring in the two studies by source of power affords us further insight into the possible changes that were taking place. The percentages for the first and second study, respectively, are:¹⁸ overtly by male, 42% and 29%; overtly by female, 13% and 13%; subtly by female, 33% and 31%; and mutual cooperation, 12% and 26%.

An inspection of these results indicates that the percentages in the overt and subtle female categories are almost identical, while the percentage differences in the two remaining categories are indicative of more female power during 1794–1825 than 1725–94. The p resulting from the application of the appropriate χ^2 test is significant below the .01 level. A consideration of the χ^2 calculations indicates that it is the mutual cooperation category that almost exclusively accounts for the significant χ^2 . Moreover, a closer examination of table 1 in this study shows that 29 of the 30 discussions of mutual cooperation were in the “advocated” category (19 out of the 29 advocated discussions were in nonfiction). We would have been surprised to have found a pervasive existing pattern of mutual cooperation between the sexes within a patriarchal system, yet the advocated character of these discussions suggests that there may have been a greater willingness to accept women as equal partners in the marital relationship during this period. There may have been a movement toward egalitarianism during the 1794–1825 period. Clearly, however, the joint results of these two studies portray the female as something other than a passive agent to the patriarchal system prior to industrialization.

A remark must be made at this point regarding the number of relevant power discussions found in our two investigations. There were 321 total discussions in the first study and 113 total discussions in the present study (see table 1 in both instances).¹⁹ A major reason for this decline hinges on

¹⁸ The reader may calculate these percentages by employing the row sums in table 1 of both studies.

¹⁹ It is often desirable to standardize in a comparative content analysis for number of pages. We did not do this since we had the opportunity to study the total popula-

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the character of the magazine content during this period. Magazines were more specialized and varied at this time than during 1741-94. (In fact, as indicated above, three magazines that were not at all reflective of family life were excluded from our sample for just this reason.) A significant number of the magazines in our set of 20 contributed very few discussions to any of the tables in our study. Our content analysis of the magazines revealed that there simply were not many articles concerning the family or the role of women. Literature, literary reviews, poetry, historical accounts, and letters to the editor took up most of the pages in the magazines. A contributing factor may have been a desire at this time to develop an American literature, which may have influenced magazine content. Fiction also gained as magazine material during this period. This was more true, however, of weekly miscellanies and women's magazines than the important general magazines of the period (Mott 1930, pp. 173-74).²⁰

The Romantic Love Complex

As in the first study, romantic love was defined in terms of five dimensions: (1) idealization of the loved one, (2) the one and only, (3) love at first sight, (4) love wins out over all, and (5) glorification of personal emotions. The frequency of discussions depicting each of these components is presented in table 2. The character of the discussion (poetry, other fiction, and nonfiction) is also shown in table 2.

The most interesting aspect of table 2 is the large number of discussions that exhibited the romantic love complex. There were 761 such discussions. In our original study there had been 337 discussions of romantic love for the years 1741-94. The love-at-first-sight category again contained the fewest discussions and the idealization of the loved one category once more ranked very high. The glorification of emotions, however, was the most frequently mentioned category in this study.²¹

tions of important magazine material that existed in America during 1725-94 and 1794-1825, and the issues during the second period were not as reflective of family patterns. As there were, however, more magazines and issues during the second period, an increase in the frequency of occurrence of a variable could, to varying extents, be a function of increased publication. This does not negate, however, in our circumstances, the actual increased prevalence of the variable in the second situation, nor does it seem to affect the relative comparisons of the frequency of occurrence of the categories of the variables that occurred in the two studies, for example, our discussion in the preceding paragraph of the types of power that occurred in the two studies. A significant decrease, however, in the frequency of occurrence of a variable during the second period would be particularly interesting in light of the increase in magazines and issues.

²⁰ For this reason, we should be careful to observe the number of fictional pieces involved in this study. Of the power discussions in table 1 of this study, 48% were fictional. The corresponding table in the initial study contained 28%.

²¹ The percentages in the romantic love categories for studies 1 and 2, respectively,

TABLE 2

INDICATORS OF ROMANTIC LOVE COMPLEX AND CHARACTER OF DISCUSSION (POETRY, OTHER FICTION, AND NONFICTION), 1794-1825 (IN NUMBER OF DISCUSSIONS)

Indicator	Poetry	Other Fiction	Nonfiction	Row Sum
Idealization of the loved one	137	6	1	144
The one and only	127	10	2	139
Love at first sight	11	1	0	12
Love wins out over all	25	4	0	29
Glorification of personal emotions	397	34	6	437
Total	697	55	9	761

It must be emphasized that 99% of the discussions involving romantic love in this study occurred in fiction (see table 2). The total number of discussions in poetry and other fiction differed from the number of discussions in nonfiction below the .001 level. Consequently, these discussions do not document that romantic love actually served as a basis for marriage at this time. Although it would be unusual if romantic love, prevalent as it was in certain literature of the period, did not have an influence on behavior, the data indicate that romance was very popular in the literature of the day. Especially prevalent were the works of famous English and French writers. These discussions often described love as passionate, romantic, and associated with joy. In spite of these results, as one of our readers noted, one must ask whether it is correct to assume that the increase in space devoted to romantic love represents a corresponding increase at the level of social behavior. If romantic love looms larger in this later period, does this reflect a change in social life, or is the magazine stressing it because there is in fact pressure away from free choice in the society? In response we can say that even if one wishes to grant this interpretation, the significant increase in space devoted to romantic love suggests a strong counterreaction; a counterreaction with possibly a strong base which may well have intensified the awareness and growth of romantic love. In addition, there is no suggestion in the general social history of the period to conclude significant social pressure against romantic love.

Finally, the data we have reported on romantic love may be a precursor of a trend that became widespread at a later period. The fictional and nonfictional accounts of romantic love in our previous study support the

were: (1) idealization of the loved one, 30% and 19%; (2) the one and only, 18% and 18%; (3) love at first sight, 9% and 2%; (4) love wins out over all, 25% and 4%; and (5) glorification of personal emotions, 18% and 57%.

²² There were 66 discussions of existent romantic love in our first study that occurred in nonfiction (Lantz et al. 1968, p. 420).

results reported here.²² Moreover, the research²³ and historical conclusions²⁴ of other writers lend further support to this observation.²⁵

The significant preoccupation with romantic love may have had an impact on the amount of space devoted to two related aspects of the man-woman relationship, motivations for marriage and the response to sexual deviation. Each of these areas showed a substantial drop in number of discussions.

Motivations for Marriage

As in the first study, three motivations for marriage were considered: happiness, wealth, and status. We also determined whether these motivations were held by individuals entering marriage or by their parents. The motive of personal happiness referred to a discussion where enjoyment was perceived as a major reason for marrying a person. This motivation would appear to be compatible with the romantic love complex. The meanings of status and wealth as motivating factors for marriage are self-evident.²⁶

An examination of table 3 reveals that the number of discussions describing motivations for marriage is less than in the first study. There were 164 such discussions for the 1741-94 period in the first study but only 20 in the present study. Of these discussions, 10 were in fiction and 10 in nonfiction. An examination of the row sum column in table 3 indicates that 10 discussions depicted a desire for happiness, while the remaining 10 discussions portrayed a desire for wealth or status as a motivation for marriage. The nonfictional discussions must be interpreted cautiously due to their paucity, but they demonstrate that various traits compatible with the romantic love complex were present to some extent in the important American magazine content of 1794-1825.

Sexual Standards

The magazine content was considered for evidence regarding the sexual normative structure of the period. Sanctions were employed as indexes of

²³ For a recent study, see Furstenberg (1966).

²⁴ For instance, see Stewart (1954, p. 182) and Goode (1963).

²⁵ It should be noted again that the evidence in this study regarding romantic love as a basis for marriage was largely from fiction and hence only documents that the components of romantic love were not unknown in America during the 1794-1825 period. The sources cited in nn. 22 and 23, however, support at least the limited existence of romantic love in America prior to extensive industrialization and urbanization.

²⁶ For the operational definitions of status and wealth, see our previous study (Lantz et al. 1968, p. 421).

TABLE 3

MARITAL MOTIVE AND HOLDER OF MOTIVE BY CHARACTER OF DISCUSSION (FICTION AND NONFICTION), 1794-1825 (IN NUMBER OF DISCUSSIONS)

MOTIVE	FICTION				NONFICTION				Row Sum
	Parent	Ego	Not Spec-ified	Total	Parent	Ego	Not Spec-ified	Total	
Happiness	0	3	2	5	2	2	1	5	10
Wealth	3	1	1	5	0	4	0	4	9
Status	0	0	0	0	0	1	0	1	1
Total	3	4	3	10	2	7	1	10	20

the sexual mores. The three categories of sanctions were punishment, ostracism, and sympathy. A distinction was similarly made between discussions that indicated a particular sanction had been implemented toward the person mentioned in the article (behavior) and discussions that indicated sanctions should be implemented (attitude). Hence, the three forms of sanctions were classified into one of two categories—behavior or attitude.

The most salient aspect of our analysis was that very few discussions involving sanctions toward the sexual deviant were found in the magazine content.²⁷ In the first study, there were a total of 136 discussions involving either attitude or behavior toward the sexual deviant for the 1777-94 period²⁸ (see tables 4 and 5 in Lantz et al. 1968, pp. 422-23), but in the present study there were only 12 such discussions,²⁹ and several of these discussions involved brief reports indicating that the courts had made the male pay damages to the female for "seduction" that occurred under the false promise of marriage. These discussions, though limited in number, continue to reflect some power of the female in the courtship situation during this period.³⁰

²⁷ Due to the infrequent discussions of sexual sanctions, these data are not presented in table form in this study.

²⁸ There were only three discussions of sanctions during the 1741-76 period in the first study (Lantz et al. 1968, p. 422).

²⁹ This pattern of decreasing family mentions from study 1 to study 2 has been commented on previously. It may be due to the magazine content of the period, but an alternative explanation will be presented in this section of the paper.

³⁰ Kephart (1966, p. 387) notes that "the right to bring suit for breach of promise was established in the early colonial period, and from then on the number of 'heart balm' cases tended to multiply." Our data confirm his observations. There is also evidence to suggest that some females were taking advantage of this law. The following statement from the *Niles' Weekly Register* (vol. 1, 1811-12) is of interest in this

In commenting on the marked drop in discussions dealing with motivations for marriage and sexual deviance, several possibilities arise. First, we are inclined to believe that the major literary concern in regard to the man-woman relationship during this period was with romantic love. Certainly our data support such a conclusion. Such a concern is consistent with marriage for personal-emotional reasons, rather than marriage for material wealth and position. The reduction in discussions regarding sexual deviance may indicate a decline in the concern for sexual deviance, but such a decline may bear a relationship to romantic love. Indeed, it has often been argued that the very existence of romantic love tends to place the man-woman relationship on an "elevated plane," hiding and obscuring sexual concerns. In both instances, however, the preoccupation with romantic love may have been the overriding concern detracting from a concern with the specific areas of motivation for marriage and sexual deviance. In so doing, however, the major ideological thrust is on the broader personal emotional responses and less on the traditional requirements associated with marriage. One must, of course, recognize the possibility that the increase in romantic love literature is purely fortuitous, with no relationship to preoccupation or behavior. This is a possibility, but one less convincing than the other alternatives suggested.

THE RELIABILITY CHECK

It will be recalled that our sample of magazines was subjected to a reliability check. Our basic conclusion was that the same patterns were reflected in the power, romantic love, and sexual standards categories of our two examinations. For instance, less than a dozen discussions of sexual sanctions were found; none of them were liberal in character. There was again an abundance of fictional discussions of romantic love, but very few in non-fiction;³¹ 25 discussions of motivations for marriage were located in the reliability check compared with the 20 presented in table 3; the proportions advocating marriage for love or happiness were similar in both instances.

The power results also provided consistent patterns. There were 26 discussions of overt male power. Three discussions were in fiction and 23 in nonfiction. Of the nonfictional discussions, 16 portrayed existing male overt

regard: "Breach of promise is becoming quite a fashionable law proceeding to the eastward—the willing lasses have put the unwilling lads into smart damages, in many instances."

³¹ More specifically, there were 153 discussions in fiction and five in nonfiction. This fictional frequency is considerably lower than the one presented in table 2 because poetry was not as carefully examined in the reliability check due to the extensiveness of the material being considered. Although this procedure would tend to reduce the number of fictional discussions of all the variables in the reliability check, it particularly influenced the romantic love category where so many fictional discussions came from poetry.

power and seven advocated this power. There were 24 discussions of mutual cooperation obtained in the reliability check. Of these discussions 10 were in fiction and 14 in nonfiction; all 24 discussions fell into the "advocated" category. These results are quite similar to those presented in table 1. Due to the extreme amount of time required to check these data, and because we carefully checked the two power categories that had shown the most change relative to the first study, exact frequency counts were not made for the remaining power categories. The coder, however, noted no significant departure from his other coding of the data. Parallel patterns were obtained for the remaining power categories. We are particularly impressed by these results given the large amount of data being considered.

DISCUSSION

The results of this investigation for the period 1794–1825, along with other studies noted earlier, move us closer to establishing a baseline in history with respect to the structure and organization of the American family. Specifically the findings regarding both power and romantic love suggest patterns at variance with the traditional views held of the early American family. To the extent that the results of this and other investigations are valid, it becomes necessary to ask why an incomplete and essentially oversimplified view of the family in the past has been maintained! A major difficulty may be the failure to take into account certain facets of both history and social change that were important during the early period of American history.³² An appreciation of these factors may enable us to develop a more accurate picture of the early American family, and it may enable us to see the forerunners of change prior to large-scale urbanization and industrialization. The American family during the early period of our history was influenced by at least three broad sets of factors—ongoing internal structural changes, the consequences of the conditions of settlement in America, and the humanistic and ideological beliefs concerning man that had their roots in both Europe and America. These factors may have been responsible for significant family change in the period under investigation.

Sociologists have, to a large extent, paid relatively little attention to the internal dynamics of family change in historic perspective, treating family change almost exclusively as a residual of external change. From this

³² It is of interest in this regard that Kurt Wolff (1970) has concluded that American sociology has generally disdained historical analyses and that a modification of this trend would facilitate its quest toward becoming a generalizing science, and Denzin (1970) has also recently argued for the incorporation of the historical perspective into symbolic interactionism. He notes (1970, p. 463) that "not only do social events have histories, but they are inextricably embedded in specific historical contexts that give them unique meaning and form."

perspective, the family has been viewed as a "sponge" absorbing change but not having much capacity to create change without outside intervention. While no sociologist would disagree with the proposition that there is always some change occurring in the internal structure of the family, the nature of such change continues to remain obscure. Major attention has been given to broad macroscopic variables, in anticipation that some linkage between these and internal family change agents can be made. The inability to spell out such a linkage between broad macroscopic factors and internal structural variables in the family has led to significant gaps in our knowledge of family structure and change in the past, and these gaps are especially marked in the area of social psychological processes.

Without in any sense negating the role of external events as change agents, one can visualize ongoing efforts within the family to redefine power. For example, given a family structure in which power is concentrated in the hands of a patriarch, one can hypothesize structural tensions and concomitant efforts to produce change. Under these conditions, one might anticipate that interaction between family members will contain elements in which the patriarch might maximize resources in order to control change while others might seek resources to initiate change. Power, status, and authority—never distributed equally in the family of the past—must always have been a major source of structural tension. As such, the family was probably subject to more internal conflict than has generally been recognized. Both the stress and the adaptations that were made might well be examined at the level of family dynamics and not necessarily as a residual adaptation to an outside event. The significance of an external event is to be found perhaps in the rate and direction of internal change. Thus, one could expect that external phenomena would intensify and give direction to internal family change, but it does not appear defensible to argue that some fairly basic changes in family structure would not have taken place without the intrusion of major events from the outside.

A second context in which change in the American family has to be seen are the ideologies of humanism and democracy. These may both have affected relationships within the family in profound ways. Powell (1917), for example, reports that between the years 1487–1654 there were about 200 marriage manuals published in England dealing with problems of the changing relationships between family members. While ideological changes had their roots in European society and bear some relationship to urbanization, industrialization, as well as the Protestant Reformation, the firm root which these ideologies took in America was probably to a large extent a by-product of the rural and frontier egalitarianism exemplified in the writings of men like Thomas Jefferson and Andrew Jackson.

The significance of the American Revolution must be stressed in this connection. One of the striking features of all social movements is the

opportunity for participation of new groups who formerly were not involved. Such opportunities for women were present prior to the actual Revolution, during the hostilities, and in the later period. This is reflected in the general magazine content during these periods; of special interest were topics dealing with changing women's status and role and the rights of women (Tebbel 1969, p. 21). New opportunities were available in occupational roles, and there were also opportunities for participation in political and ideological matters. The overall result was to restructure the roles of many women on a more permanent basis and to create a broader social climate that could provide change for many other women.

Finally, it is entirely possible that both humanism and democracy were intertwined with the existence and demands of the American frontier. A changing ideology regarding the role of women and the disproportionate sex ratios in favor of women placed them in favorable bargaining positions prior to marriage; that is, they were in a better position to pick and choose. They were perhaps in a better position likewise to determine some of the conditions within marriage itself. Under these circumstances, it was probably difficult for "traditional" family roles to be maintained. Insofar as these views are correct, the American family during 1784-1825 was probably undergoing structural and ideological changes, changes that ultimately resulted in new definitions of family roles permitting greater freedom from male and parental control.

LIMITATIONS

The arguments and assertions we have made should not in any sense obscure the several basic limitations of this study; we have the limitations of literacy sources discussed earlier, but a word should be said about the magazines themselves.

One reader correctly called attention to the possibility that it may be difficult to ascertain that the magazines employed in an analysis of this type did in fact reflect American society of the period. Should this assertion be correct, we might simply be reporting the editorial preferences of the journal. This is a limitation, and we cannot respond to it with complete confidence, but the following observations may be relevant.

First, 20 magazines were considered over approximately a 30-year period. Since more than 20 editors were responsible for these publications in this time span, it seems apparent there would have been a great diversity of editorial preferences. Furthermore, the content of the material covered numerous areas and was written by many authors. Not only were different types of magazines included among our 20, but each of these contained a variety of content, including poems, letters to the editor, theater reviews,

fiction, nonfiction, and a host of other material. If one is to effectively argue that editorial policy largely accounted for our results, it must be assumed that these editors attempted and were able to control the writings of the numerous and varied authors involved.

Second, there is independent evidence that during the period of the present investigation there were limited opportunities for editorial preferences. Tebbel (1969, pp. 14, 35-47), who only recently published a history of the American magazine, indicates that there were very few professional magazine writers in America prior to 1825. Indeed, payment for material was not the pattern until about 1819, and even then it was very meager. There was little financial incentive to write, and those who wrote were those who had other incomes. Thus, there was little material to choose from, and editors had to rely largely on what was submitted. To have exercised many editorial preferences would have meant running the risk of having little to publish.

Third, Tebbel (1969, p. 14) also notes that by the end of the 18th century magazines were "more and more reflecting social and political life, and they were influential well beyond their meager circulation figures, which themselves had increased 100 percent per capita on the average since 1741. Magazines were still not universally read, but those that appeared were well thumbed and read by many people other than their subscribers." While our analysis would have been enriched had we been able to include information about editors and publishers of these magazines, this information is not available for the period prior to 1825, a period in which magazine failure and editor turnover was very high. The post-1825 period is the one in which the strong publisher-editor presumably played a significant role in creating social policy (Tebbel 1969, chap. 3).³³

The class bias of the magazines must be recognized. While we are dealing with literary material of interest to the more privileged sections of the population and from particular regions of our country, we know relatively little about the reactions of other groups in regard to possible interests or behavioral changes in the area of marriage and the family.

We do know that three magazines (included in this study) existed in the western frontier in Transylvania and Lexington, Kentucky, and in Cincinnati in the period prior to 1825. To this extent, there was a broadening of the readership along with efforts to establish new magazines. Never-

³³ Another interesting dimension has been suggested by Florence Levinsohn. Had we coded our articles by the sex of the author as well as by the content, we might have learned something more, particularly with regard to power relations. It is likely, especially because there was so little economic incentive to write, that many of the authors of the materials published in the magazines we examined were women. That women might express more acceptance of changing power relations in their articles is a possibility that we did not consider.

theless, for the period of this study, Tassin (1961, pp. 200-1) has confirmed that magazine content relied rather heavily on material depicting circumstances in the eastern United States, although occasional accounts of frontier difficulties, mostly with Indians, were included.

Although content analysis of literature constitutes an important source of data, its validity will be enhanced if the results can be measured against other types of records. One of the most important areas that has been neglected as a source of potential inquiry are the legal records dealing with different aspects of the family, such as divorce, separation, inheritance, and property rights. As one of our readers has suggested, these records may provide us with the forerunners of change. People often went to court precisely because they favored new patterns at variance with the statutes. Indeed, they were contesting the law hoping for favorable court interpretations and redress. Thus, a record of complaints and court decisions in particular areas of family life could be of considerable value. These might confirm the legal pressure to maintain traditional family patterns, but they could also provide valuable data for significant movements toward family change. It would be especially valuable to compare the results of a literary content analysis of a study such as this one with the results of an analysis of court records for the same period.³⁴

There remains much to be done to overcome the many gaps in our knowledge of the American family in the past. An additional area of inquiry has to do with the examination of occupations for married women in law and commerce in the 18th and 19th centuries in America. There are numerous accounts of married women in business and of female attorneys in the colonial period, roles which carried greater authority and power than normally recognized (Dexter 1924; Cometti 1947; Bridges 1965). There is evidence to suggest that the role of women in these areas has not been fully understood and explored, and such lack of exploration is a factor contributing to our limited understanding of the role of the American woman in the past; this in turn negates our fuller understanding of the present American female. Finally, it is clear that this report is limited to selected aspects of family life. As noted in the research, there are other important dimensions of family change and stability that were not studied.

The problem remains one of seeking out innovative ways of dealing with the family in historic perspective, calling for a constant reappraisal of what

³⁴ For two representative sources on the sociology of law, see Evan (1962) and Davis et al. (1962). For a recent and excellent discussion of this subject by two members of the legal profession, see Friedman and Macaulay (1969). For two insightful anthropological studies of the relationship between law and societal patterns, see Fallers (1969) and Kay (1965). The latter study is contained in an important special issue of the *American Anthropologist* concerned with the ethnography of law.

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are obviously limited methodological techniques. Nevertheless, such research efforts enable us to fill some of the gaps in our knowledge of the family as it existed in different periods of our history, and such efforts may eventually serve to clarify the relationship between broader macroscopic influences and the internal dynamics of structural units such as the family.

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Staged Authenticity: Arrangements of Social Space in Tourist Settings¹

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The problem of false consciousness and its relationship to the social structure of tourist establishments is analyzed. Accounts of travelers are examined in terms of Erving Goffman's front versus back distinction. It is found that tourists try to enter back regions of the places they visit because these regions are associated with intimacy of relations and authenticity of experiences. It is also found that tourist settings are arranged to produce the impression that a back region has been entered even when this is not the case. In tourist settings, between the front and the back there is a series of special spaces designed to accommodate tourists and to support their beliefs in the authenticity of their experiences. Goffman's front-back dichotomy is shown to be ideal poles of a continuum, or a variable.

A theme in Talcott Parsons's work (1937, 1964) is his insistence on the necessity of integrating social scientific understanding of belief, action, and social structure. By the end of the 1950s, there was some agreement in sociological circles that Parsons's own theoretical system had not succeeded in bringing off this integration. (See the discussions in Gouldner [1970] or Friedrichs [1970].) But this is not the important point to be grasped from Parsons's total project. He left us all with a clearly defined problem to solve. During the 1960s, Berger and Luckmann (1966), Erving Goffman, Harold Garfinkel (1967), and others began constructing bridges that permit analytical passage from social structure to the structure of behavior and beliefs. Now, in the 1970s, we can, I think, begin to study the relationship of social structure and beliefs in specific social situations, using more refined conceptions than have previously been available.

This paper is part of my study of tourism. The central finding of the larger study (in preparation) is that sightseeing is a form of ritual respect for society and that tourism absorbs some of the social functions of religion in the modern world. The dimension of social life analyzed in this paper is its authenticity or, more exactly, the search for authenticity of experience that is everywhere manifest in our society. The concern of moderns for the

¹ Temple University and the Department of Rural Sociology and Center for International Studies at Cornell University have granted the author leaves of absence to gather material for his study of tourism. Some of this material is used in this paper, and the author acknowledges his gratitude. Financial support in writing this paper was provided by the Faculty Research Fund at Temple University.

shallowness of their lives and inauthenticity of their experiences parallels concerns for the sacred in primitive society. Each contributes to the structural solidarity of the society in which it is found. The solidarity of primitives depends on every individual's keeping his place, and this is guaranteed by the sacralization of functionally important aspects of individual behavior such as gift exchange and mate selection. Primitives may, but they need not, worry about the authenticity of their rituals. The very survival of their society stands as internal proof of the victory of good over evil and real over false. By contrast, individual morality is only indirectly linked to the solidarity of modern society in which functionally important relationships are among bureaucracies, communities, and other complex organizations. Under modern conditions, the place of the individual in society is preserved, in part, by newly institutionalized concerns for the authenticity of his social experiences.

I began the analysis of the problem of authenticity by starting across one of the bridges between structure and consciousness that was built by Erving Goffman, and, in the course of the study, I found it necessary to extend his conception a little to make it to the other side.

SOCIAL SPACE AND THE STRUCTURING OF BELIEFS

Paralleling a commonsense division, Goffman has described a structural division of social establishments into what he terms "front" and "back" regions. The front is the meeting place of hosts and guests or customers and service persons, and the back is the place where members of the home team retire between performances to relax and to prepare. Examples of back regions are kitchens, boiler rooms, and executive washrooms, and examples of front regions are reception offices, parlors, and the like. Although architectural arrangements are mobilized to support this division, it is primarily a social one, based on the type of social performance that is staged in a place, and on the social roles found there. In Goffman's own words: "Given a particular performance as the point of reference, we can distinguish three crucial roles on the basis of function: those who perform; those performed to; and outsiders who neither perform in the show nor observe it. . . . [T]he three crucial roles mentioned could be described on the basis of the regions to which the role-player has access: performers appear in the front and back regions; the audience appears only in the front region; and the outsiders are excluded from both regions" (1959, pp. 144-45). The apparent, taken-for-granted reality of a social performance, according to Goffman's theory, is not an unproblematical part of human behavior. Rather, it depends on structural arrangements like this division between front and back. A back region, closed to audiences and outsiders, allows concealment of props and activities that might discredit

the performance out front. In other words, sustaining a firm sense of social reality requires some mystification.

Social reality that is sustained through mystification may be a "false" reality, as occurs in conning. Equally interesting is the case wherein mystification is required to create a sense of "real" reality. Once social structure differentiates into front and back in the movement from primitive to modern arrangements, the truth can no longer speak for itself. It must always be announced and revealed.

A recent example of a mystification designed to generate a sense of real reality is the disclosure that chemical nitrates are injected into hams for cosmetic purposes to keep them more pink, appetizing, and desirable, that is, more hamlike (Minz 1971). Similarly, a respondent of mine reports that some of the go-go girls in San Francisco's North Beach have their breasts injected with silicones in order to conform their size, shape, and firmness to the characteristics of an ideal breast. Novels about novelists and television shows about fictitious television stars are examples on a cultural plane. In each of these cases, a kind of strained truthfulness is similar in most of its particulars to a little lie.

Mystification, then, can be the conscious product of an individual effort to manipulate a social appearance, as occurred in the ham and breast examples. It can also be found where there is no conscious individual-level manipulation. Social structure itself is involved in the construction of mystifications that support social reality.

Examples are found in avoidance behavior surrounding back regions. The possibility that a stranger might penetrate a back region is one major source of social concern in everyday life, as much a concern to the strangers who might do the violating as to the violated. Everyone is waiting for this kind of intrusion not to happen, which is a paradox in that the absence of social relationships between strangers makes back-region secrets unimportant to outsiders or casual and accidental intruders. Just having a back region generates the belief that there is something more than meets the eye; even where no secrets are actually kept, back regions are still the places where it is popularly believed the secrets are. Folklorists discover tales of the horror concealed in attics and cellars, attesting to this belief.

BACK REGIONS, INTIMACY, AND SOCIAL SOLIDARITY

An unexplored aspect of back regions is how their mere existence, and the possibility of their violation, functions to sustain the commonsense polarity of social life into what is taken to be intimate and "real" and what is thought to be "show." The beliefs supported by this division of society into front and back center on popular ideas of the relationship of truth to intimacy. In our society, intimacy and closeness are accorded much im-

portance: they are seen as the core of social solidarity, and they are also thought by some to be morally superior to rationality and distance in social relationships, and more "real." Being "one of them," or at one with "them," means, in part, being permitted to share back regions with "them." This is a sharing which allows one to see behind the others' mere performances, to perceive and accept the others for what they really are.

Touristic experience is circumscribed by the structural tendencies described here. Sightseers are motivated by a desire to see life as it is really lived, even to get in with the natives, and, at the same time, they are deprecated for always failing to achieve these goals. The term "tourist" is increasingly used as a derisive label for someone who seems content with his obviously inauthentic experiences.

The variety of understanding that is held out before tourists as an ideal is an authentic and demystified experience of an aspect of some society or other person. An anonymous writer in an underground periodical breathlessly describes her feelings at a women's liberation, all-female dance where she was able, she thought, to drop the front she usually maintains in the presence of men:

Finally the men moved beyond the doorway. And We Danced—All of us with all of us. In circles and lines and holding hands and arm in arm, clapping and jumping—a group of whole people. I remember so many other dances, couples, men and women, sitting watching, not even talking. How could I have consented to that hateful, possessive, jealous pairing? So much energy and life, and sensuality, we women have so rarely and ineffectively expressed. But we did, on Saturday. The women in the band were above performing and beyond competition, playing and singing together and with we who were dancing. And We Danced—expressing for and with each other. [Anon., no date, p. 33; all punctuation and capitalization as in the original]

An earlier, one-sided version of this connection among truth, intimacy, and sharing the life behind the scenes is found in descriptions of the ethnographic method of data collection. Margaret Mead has written: "The anthropologist not only records the consumption of sago in the native diet, but eats at least enough to know how heavily it lies upon the stomach; not only records verbally and by photographs the tight clasp of the baby's hands around the neck, but also carries the baby and experiences the constriction of the windpipe; hurries or lags on the way to a ceremony; kneels half-blinded by incense while the spirits of the ancestors speak, or the gods refuse to appear. The anthropologist enters the setting and he observes. . ." (Mead 1955, p. 31). These writers base their comments on an implicit distinction between false fronts and intimate reality, a distinction which is not, for them, problematical: once a person, or an observer, moves off stage, or into the "setting," the real truth begins to reveal itself more or less automatically.

Closer examination of these matters suggests that it might not be so easy to penetrate the true inner workings of other individuals or societies. What is taken to be real might, in fact, be a show that is based on the structure of reality. For example, Goffman warns that under certain conditions it is difficult to separate front from back and that these are sometimes transformed one into the other:

[W]e can observe the up-grading of domestic establishments, wherein the kitchen, which once possessed its own back regions, is now coming to be the least presentable region of the house while at the same time becoming more and more presentable. We can also trace that peculiar social movement which led some factories, ships, restaurants, and households to clean up their backstages to such an extent that, like monks, Communists, or German aldermen, their guards are always up and there is no place where their front is down, while at the same time members of the audience become sufficiently entranced with the society's id to explore the places that had been cleaned up for them. Paid attendance at symphony orchestra rehearsals is only one of the latest examples. [1959, p. 247]

Under the conditions Goffman documents here, the back-front division no longer allows one to make facile distinctions between mere acts and authentic expressions of true characteristics. In places where tourists concentrate, I am about to show, the issues are even more complex.

AUTHENTICITY IN TOURIST SETTINGS

Everett C. Hughes has suggested to me (in correspondence) that the original tours were religious pilgrimages. The connection between the two is not merely one of organizational similarities. The motive behind a pilgrimage is similar to that behind a tour: both are quests for authentic experiences. Pilgrims attempted to visit a place where an event of religious importance actually occurred. Tourists present themselves at places of social, historical, and cultural importance.

It is worth noting that not all tourists have regarded back regions as socially important places. On occasion, and for some visitors, back regions are obtrusive. Arthur Young, when he visited France in 1787 to make observations for his comparative study of agriculture, also observed the following:

Mops, brooms, and scrubbing brushes are not in the catalogue of the necessities of a French inn. Bells there are none; the *fille* must always be bawled for; and when she appears, is neither neat, well dressed, nor handsome. The kitchen is black with smoke; the master commonly the cook, and the less you see of the cooking the more likely you are to have a stomach to your dinner. The mistress rarely classes civility or attention to her guests among the requisites of her trade. We are so unaccustomed in England to live in our bedchambers that it is at first awkward in France to find that people live nowhere else. Here I find that everybody, let his rank be what it may, lives in his bed-chamber. [Young 1910, p. 332]

Among some, especially some American, tourists and sightseers of today, Young's attitude would be considered insensitive and cynical even if there was agreement that his treatment of the facts was accurate, as apparently it was. One finds in the place of Young's attitude much interest in exactly the details Young wanted not to notice.

A tourist's desire to share in the real life of the places visited, or at least to see that life as it is really lived, is reflected in the conclusion of one tourist's report from a little Spanish town: "Finally, Frigliana has no single, spectacular attraction, such as Granada's Alhambra or the cave at Nerja. Frigliana's appeal lies in its atmosphere. It is quaint without being cloying or artificial. It is a living village and not a 'restoration of an authentic Spanish town.' Here one can better see and understand the Andalusian style of life" (Pearson 1969, p. 29). There are vulgar ways of expressing this liberal sentiment, the desire "to get off the beaten path" and "in with the natives." An advertisement for an airline reads: "Take 'De tour.' Swissair's free-wheeling fifteen day Take-a-break Holiday that lets you detour to the off-beat, over-looked and unexpected corners of Switzerland for as little as \$315. . . . Including car. Take de tour. But watch out for de sheep, de goats and de chickens" (Advertisement 1970, p. 42).

Some tourists do make incursions into the life of the society they visit, or are at least actually allowed to peek into one of its back regions. In 1963 the manager of the Student Center at the University of California at Berkeley would occasionally invite visitors to the building to join him on his periodic inspection tours which was, for them, a chance to see its kitchens, the place behind the pin-setting machines in the bowling alley, the giant blowing fans on the roof, and so forth, but he was probably not a typical building manager. This kind of hospitality is the rule rather than the exception in the areas of the world that have been civilized the longest, a factor in the popularity of these areas with Anglo-Americans. A respondent of mine told me she was invited by a cloth merchant in the Damascus bazaar to visit his silk factory, and she answered "yes," whereupon he threw open a door behind his counter exposing a little dark room where two men in their underwear sat on the floor on either side of a hand loom passing a shuttle back and forth between them. "It takes a year to weave a bolt of silk like that," the owner explained as he closed the door. This kind of happening, an "experience" in the everyday sense of that term, often occurs by accident. A lady who is a relative of mine, and another lady friend of hers, walked too far into the Canadian Rockies near Banff and found themselves with too much traveling back to town to do in the daytime that was left to do it in. They were rescued by the crew of a freight train, and what they remember most from their experience was being allowed to ride with the engineer in the cab of his locomotive. A young American couple told me of being unable to find a hotel room in Zagreb,

Yugoslavia, and, while discussing their plight on the sidewalk, being approached by an old woman who led them by a circuitous route to a small apartment where they rented a blackmarket room, displacing the family of workers who slept on a couch behind a blanket hung as a curtain in the living room.

Touristic openings into society's back regions are ubiquitous. A certain amount of what is called "personal style" is a product of the way the individual relates to touristic opportunity. Some individuals are always on the lookout for a touristic opening. They are said to have an "adventuresome attitude." A report from the Caribbean suggests that a taste for adventure can be cultivated: " 'But tourists never take the mail boats,' said the hotel manager. That clinched the matter. The next afternoon, I jumped from the dock at Potter's Cay in downtown Nassau to the rusted deck of the Deborah K., swinging idly at her spring lines. . . . [The writer describes island hopping on the mail boat and ends his account with this observation.] The next day, while aloft in a Bahamas Airways plane, I spotted the Deborah K. chugging along in the sound toward Green Turtle Cay. She is no craft for the queasy of stomach and has a minimum of the amenities that most people find indispensable, but she and her sister mail boats offer a wonderfully inexpensive way to see life in the Bahamas—life as the natives live it, not the tourists" (Keller 1970, p. 24). Given the felt value of these experiences, it is not surprising to find social structural arrangements that produce them.

STAGED AUTHENTICITY IN TOURIST SETTINGS

A common reason for taking guided tours of social establishments is that the tour organizes access to areas of the establishment that are ordinarily closed to outsiders. School children's tours of firehouses, banks, newspapers, and dairies, for example, are called "educational" because the inner operations of these important places are shown and explained in the course of the tour. This kind of tour, and the experiences generated by it, provides an interesting set of analytical problems. The tour is characterized by social organization designed to reveal inner workings of the place; on tour, outsiders are allowed further in than regular patrons; children are permitted to enter bank vaults, to see a million dollars, to touch cows' udders, etc. At the same time, there is a staged quality to the proceedings that lends to them an aura of superficiality, albeit a superficiality that is not always perceived as such by the tourist, who is usually forgiving about these matters.

An account from Cape Kennedy provides illustration:

No sightseers at the Manned Spacecraft Center ever had a more dramatic visit than those who, by design or accident of time, found themselves touring the facility last month during the unforgettable mission of Apollo

13. . . . In a garden-like courtyard outside the News Bureau in Building 1, a group of tourists visiting the Manned Spacecraft Center here stared at the working correspondents through the huge plate-glass windows. The visitors, too, could hear the voice of Mission Control. A tall young man, his arm around his mini-skirted blonde girl friend, summed up the feelings of the sightseers when he said, half aloud, "Being here's like being part of it."

"Dear God," his girl whispered earnestly, "please let them come home safe." [Gordon 1970, no page]

The young man in this account is expressing his belief that he is having an almost authentic experience. This type of experience is produced through the use of a new kind of social space that is opening up everywhere in our society. It is a space for outsiders who are permitted to view details of the inner operation of a commercial, domestic, industrial, or public institution. Apparently, entry into this space allows adults to recapture virginal sensations of discovery, or childlike feelings of being half in and half out of society, their faces pressed up against the glass. Also, it can be noted that what is taken by some political radicals and conservatives to be indices of a general relaxation of society's moral standards ("swinging," "massage therapy," wide-screen cunnilingus, etc.) are only special cases of reality displays, public orgasm worked up in the interest of social solidarity.

Other basic (that is, biological process) examples of staged intimacy are provided by the tendency to make restaurants into something more than places to eat:

The newest eating place in Copenhagen is La Cuisine, strategically located on the Stroegtet, the main strolling street of the city. Everyone is flat-nosing it against the windows these days watching the four cooks.

.....
In order to get to the cozy, wood-paneled restaurant in the back of the house, the guest must pass the kitchen. If he is in a hurry he may eat in the kitchen, hamburger joint-style.

"The kitchen" bit is a come-hither, actually, admits Canadian-born, Swiss-educated Patrick McCurdy, table captain and associate manager. "A casual passer-by is fascinated by cooks at work, preparing a steak or a chicken or a salad." [Sjöby 1971, p. 5]

What is being shown to tourists is not the institutional "backstage," as Goffman defined this term. Rather, it is a staged back region, a kind of living museum for which we have no analytical terms.

THE STRUCTURE OF TOURIST SETTINGS

A student of mine has told me that a new apartment building in New York City exhibits its heating and air conditioning equipment, brightly painted in basic colors, behind a brass rail in its lobby. From the standpoint of the social institutions that are exposed in this way, the structure of their recep-

tion rooms reflects a new concern for truth and morality at the institutional level. Industry, for example, is discovering that the commercial advantages of appearing to be honest and aboveboard can outweigh the disadvantages of having to organize little shows of honesty. An interesting parallel here is with some of the young people of the industrial West who have pressed for simplicity and naturalness in their attire and have found it necessary assiduously to select clothing, jewelry, and hair styles that are especially designed to look natural. In exposing their steel hearts for all to see, and staging their true inner life, important commercial establishments of the industrial West "went hippie" a decade before hippies went hippie. Approached from this standpoint, the hippie movement is not, technically, a movement, but a basic expression of the present stage of the evolution of our industrial society.

The current structural development of industrial society is marked by the appearance everywhere of touristic space. This space can be called a stage set, a tourist setting, or simply a set depending on how purposefully worked up for tourists the display is. The New York Stock Exchange viewed from the balcony set up for sightseers is a tourist setting, as there is no evidence that the show below is for the sightseers. The exhibitions of the back regions of the world at Disneyland in Anaheim, California, are constructed only for sightseers, however, and can be called "stage sets." Characteristics of sets are: the only reason that need be given for visiting them is to see them—in this regard they are unique among social places; they are physically proximal to serious social activity, or serious activity is imitated in them; they contain objects, tools, and machines that have specialized use in specific, often esoteric, social, occupational, and industrial routines; they are open, at least during specified times, to visitation from outsiders.

Touristic consciousness is motivated by its desire for authentic experiences, and the tourist may believe that he is moving in this direction, but often it is very difficult to tell for sure if the experience is authentic in fact. It is always possible that what is taken to be entry into a back region is really entry into a front region that has been totally set up in advance for touristic visitation. In tourist settings, especially in modern society, it may be necessary to discount the importance, and even the existence, of front and back regions except as ideal poles of touristic experience.

To return to Goffman's original front-back dichotomy, tourist settings can be arranged in a continuum starting from the front and ending at the back, reproducing the natural trajectory of an individual's initial entry into a social situation. While distinct empirical indicators of each stage may be somewhat difficult to discover, it is theoretically possible to distinguish six stages to this continuum, and here is a place where the exercise of a little theoretical license might eventually prove to be worthwhile.

Stage 1: Goffman's front region; the kind of social space tourists attempt to overcome, or to get behind.

Stage 2: a touristic front region that has been decorated to appear, in some of its particulars, like a back region: a seafood restaurant with a fish net hanging on the wall; a meat counter in a supermarket with three-dimensional plastic replicas of cheeses and bolognas hanging against the wall. Functionally, this stage (two) is entirely a front region, and it always has been, but it is cosmetically decorated with reminders of back-region activities: mementos, not taken seriously, called "atmosphere."

Stage 3: a front region that is totally organized to look like a back region: simulations of moon walks for television audiences; the live shows above sex shops in Berlin where the customer can pay to watch interracial couples copulating according to his own specific instructions. This is a problematical stage because the better the simulation, the more difficult it is to distinguish it from stage 4.

Stage 4: a back region that is open to outsiders: magazine exposés of the private doings of famous personages; official revelations of the details of secret diplomatic negotiations. It is the open characteristic that distinguishes these especially touristic settings (stages 3 and 4) from other back regions; access to most nontouristic back regions is somewhat restricted.

Stage 5: a back region that may be cleaned up or altered a bit because tourists are permitted an occasional glimpse in: Erving Goffman's kitchen, factory, ship, and orchestra rehearsal cases; news leaks.

Stage 6: Goffman's back region; the kind of social space that motivates touristic consciousness.

That is theory enough. The empirical action in tourist settings is mainly confined to movement between areas decorated to look like back regions, and back regions into which tourists are allowed to peek. Insight, in the everyday, and in some ethnological senses of the term, is what is gotten from one of these peeks into a back region.

TOURISTS AND INTELLECTUALS

There is no serious or functional role in the production awaiting the tourists in the places they visit. Tourists are not made personally responsible for anything that happens in the establishments they visit, and the quality of the insight gained by touristic experience has been criticized as less than profound. David Riesman's "other directed" (1950) and Herbert Marcuse's "one-dimensional" men (1964) are products of a traditional intellectual concern for the superficiality of knowledge in mass industrial society, but the tourist setting per se is just beginning to prompt intellectual commentary. Settings are often not merely copies or replicas of real-life

situations, but copies that are presented as disclosing more about the real thing than the real thing itself discloses. Of course this cannot be the case, at least not from technical standpoints—from the standpoint of ethnography, for example. The Greyline guided tours of the Haight Ashbury when the hippies lived there cannot be substituted for the studies based on participant observation that were undertaken at the same time: the intellectual attitude is firm in this belief. The touristic experience that comes out of the tourist setting is based on inauthenticity, and as such it is superficial when compared with careful study; it is morally inferior to mere experience. A mere experience may be mystified, but a touristic experience is always mystified, and the lie contained in the touristic experience, moreover, presents itself as a truthful revelation, as the vehicle that carries the onlooker behind false fronts into reality. The idea here is that a false back is more insidious and dangerous than a false front, or an inauthentic demystification of social life is not merely a lie but a superlie, the kind that drips with sincerity.

Along these lines, Daniel Boorstin's (1961, pp. 77–117) comments on sightseeing and tourism suggest that critical writing on the subject of modern mass mentality is gaining analytical precision and is moving from the individual-centered concepts of the 1950s to a structural orientation. His concept of "pseudo-event" is a recent addition to a line of specific criticism of tourists that can be traced back to Veblen's "conspicuous leisure" (1953, pp. 41–60), or back still further to Mark Twain's ironic commentary in *The Innocents Abroad*. In his use of the term "pseudo-event" Boorstin wants his reader to understand that there is something about the tourist setting itself that is not intellectually satisfying. In his own words: "These [tourist] 'attractions' offer an elaborately contrived indirect experience, an artificial product to be consumed in the very places where the real thing is as free as air. They are ways for the traveler to remain out of contact with foreign peoples in the very act of 'sight-seeing' them. They keep the natives in quarantine while the tourist in air-conditioned comfort views them through a picture window. They are the cultural mirages now found at tourist oases everywhere" (Boorstin 1961, p. 99). This kind of commentary reminds us that tourist settings, like other areas of institutional life, are often insufficiently policed by liberal concerns for truth and beauty; they are tacky. Another way of approaching the same observations is to suggest that some touristic places overexpress their underlying structure and upset certain of their sensitive visitors thereby: restaurants are decorated like ranch kitchens; bellboys assume and use false, foreign first names; hotel rooms are made to appear like peasant cottages; primitive religious ceremonies are staged as public pageants. This kind of naked tourist setting is probably not as important in the overall picture of mass tourism as Boorstin makes it out to be in his polemic, but it is an ideal type of sorts, and many examples of it exist.

Boorstin is insightful as to the nature of touristic arrangements, but he undercuts what might have developed into a structural analysis of sight-seeing and touristic consciousness by falling back onto individual-level interpretations before analyzing fully his "pseudo-event" conception. He claims that tourists themselves cause "pseudo-events." Commenting on the restaurants along superhighways, Boorstin writes: "There people can eat without having to look out on an individualized, localized landscape. The disposable paper mat on which they are served shows no local scenes, but a map of numbered super highways with the location of other 'oases.' *They feel most at home above the highway itself, soothed by the auto stream to which they belong*" (1961, p. 114; my emphasis). None of the accounts in my collection support Boorstin's contention that tourists want superficial, contrived experiences. Rather, tourists demand authenticity, just as Boorstin does. Nevertheless, Boorstin persists in architecting an absolute separation of touristic and intellectual attitudes. On the distinction between work ("traveling") and sightseeing he writes: "The traveler, then, was working at something; the tourist was a pleasure-seeker. The traveler was active; he went strenuously in search of people, of adventure, of experience. The tourist is passive; he expects interesting things to happen to him. He goes 'sight-seeing.' . . . He expects everything to be done to him and for him" (Boorstin 1961, p. 85). As I have already suggested, the attitude Boorstin expresses is a commonplace among tourists and travel writers. It is so prevalent, in fact, that it is a part of the problem of mass tourism, not an analytical reflection on it.

In other words, we still lack adequate technical perspectives for the study of "pseudo-events." The construction of such perspectives necessarily begins with the tourists themselves and a close examination of the facts of sightseeing. The writers of the accounts cited in the first sections of this paper express Boorstin's disappointment that their experiences are sometimes fleeting and insulated and a desire to get in with the natives, but, more important here, a willingness to accept disappointment when they feel they are stopped from penetrating into the real life of the place they are visiting. In fact, some tourists are able to laugh off Boorstin's disappointment. The account of a trip to Tangier from which the following is excerpted was given by a writer who clearly expected the false backwardness she found there and is relaxed about relating it: "A young Arab pulled a chair up to our table. He had rugs to sell, but we insisted we were not interested. He unrolled his entire collection and spread them out on the ground. He wouldn't leave. I could see beneath his robes that he was wearing well-tailored navy blue slacks and a baby blue cashmere sweater" (Thompson 1970, p. 3). Similarly, the visitor to Las Vegas who wrote the following has seen through the structure of tourist settings and is laughing

about it: "[A]long with winter vacationists by the thousands, I will return to lively Las Vegas, if only to learn whether Howard Hughes, like the Mint Casino, has begun issuing free coupons entitling the visitor to a backstage tour of his moneymaking establishment" (Goodman 1970, p. 11). For these tourists, exposure of back regions is a casual part of their touristic experience. What they see in the back is only another show: it does not shock, trick, or anger them, and they do not express any feelings of having been made less pure by their discovery.

CONCLUSION

In highly developed tourist settings, such as San Francisco and Switzerland, every detail of touristic experience can take on a showy, back-region aspect, at last for fleeting moments. Tourists enter tourist areas precisely because their experiences there will not, for them, be routine. The local people in the places they visit, by contrast, have long discounted the presence of tourists and go about their business as usual, even their tourist business, as best they can, treating tourists as a part of the regional scenery. Tourists often do see routine aspects of life as it is really lived in the places they visit, although few tourists express much interest in this. In the give-and-take of urban streetlife in tourist areas, the question of who is watching whom and who is responding to whom can be as complex as it is in the give-and-take between ethnographers and their respondents. It is only when a person makes an effort to penetrate into the real life of the areas he visits that he ends up in places especially designed to generate feelings of intimacy and experiences that can be talked about as "participation." No one can "participate" in his own life, he can only participate in the lives of others.

And once tourists have entered touristic space, there is no way out for them so long as they press their search for authenticity. Near each tourist setting there are others like the last. Each one may be visited, and each one promises real and convincing shows of local life and culture. Even the infamously clean Istanbul Hilton has not excluded all aspects of Turkish culture (the cocktail waitresses wear harem pants, or did in 1968) and, for some Europeans I know, an American superhighway is an attraction of the first rank, the more barren the better, because it is more American.

Daniel Boorstin was the first to study these matters. His approach elevates to the level of analysis a nostalgia for an earlier time with more clear-cut divisions between the classes, and simpler social values based on a programmatic, back versus front view of the true and the false. This classic position is morally superior to the one presented here, but it cannot lead to the scientific study of sightseeing. Specifically, Boorstin's and other

intellectual approaches do not help us to analyze the expansion of the tourist class under industrialization, and the development on an international scale of activities and social structural arrangements for tourists, social changes Boorstin himself documents. Rather than confronting the issues he raises, Boorstin only expresses a long-standing touristic attitude, a pronounced dislike, bordering on hatred, for other tourists, an attitude that turns man against man in a they-are-the-tourists-I-am-not equation. (For an excellent discussion of this aspect of the intellectual approach, see Burgelin [1967, pp. 66-69].) Additional study of sightseeing, using analytical models that are up to the task, will show that the touristic attitude, and the structures that produce it, contributes to the destruction of interpersonal solidarity that is such a notable feature of the life of the educated masses in advanced industrial society.

Daniel Boorstin calls places like American superhighways and the Istanbul Hilton "pseudo," a hopeful appellation that suggests they are insubstantial, or transitory, which they are not, and suggests also that somewhere in tourist settings there are real events accessible to intellectual elites, and perhaps there are. In this paper I have argued that a more helpful way of approaching the same facts is in terms of a modification of Erving Goffman's front-back distinction. Specifically, I have suggested that for the study of tourist settings front and back be treated as ideal poles of a continuum, poles that are linked by a series of front regions decorated to appear as back regions, and back regions set up to accommodate outsiders. I have suggested the term "stage setting" for these intermediary types of social space, but there is no need to be rigid about the matter of the name of this place, so long as its structural features and their influences on ideas are understood.

I have claimed that the structure of this social space is intimately linked to touristic attitudes, and I want to pursue this: the touristic way of getting in with the natives is to enter into a quest for authentic experiences, perceptions, and insights. The quest is marked off in stages in the passage from front to back. Movement from stage to stage corresponds to growing touristic understanding. This continuum is sufficiently developed in some areas of the world that it appears as an infinite regression of stage sets. Once in this manifold, the tourist is trapped; his road does not end abruptly in some conversion process that transforms him into Boorstin's "traveler," "working at something" as he breaks the bounds of all that is pseudo and penetrates, finally, into a real back region. Tourists make brave sorties out from their hotels hoping, perhaps, for an authentic experience, but their paths can be traced in advance over small increments of what is for them increasingly apparent authenticity proffered by tourist settings. Adventure-some tourists progress from stage to stage, always in the public eye, and greeted everywhere by their obliging hosts.

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The Effect of Schooling on Social Contacts of Urban Women¹

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This paper examines two sets of data, one derived from a study of 571 housewives and married working women, the other of 301 widows, aged 50 and over, for the association between social relationships and formal schooling. All the women live in a metropolitan area in America. The conclusion is that urbanization and industrialization trends make formal education a major requirement for the social engagement of women. The less educated an urban woman, the more probable her social isolation. This variable is more important than any other in predicting the degree to which a woman is able to engage in society and her attitude toward her social world.

BACKGROUND

Social isolation as a consequence of urbanization, industrialization, and increasing complexity (see Winch and Blumberg 1968) demands greater sociological investigation. This paper brings together for analysis two different sources of data examining the association between the social relations of women and their educational achievement: one derived from a study of 571 housewives and married working women,² the other from a study of 301 older widows.³ Both sets of data are concerned with the

¹ Many thanks go to Eileen Markley Znaniecki for help in editing, William Bates for data presentation suggestions, Paul Zelus and Daryl Chubin for some of the statistical computations, and Florence Levinsohn from *American Journal of Sociology* for editing.

² The *Occupation: Housewife* (1972) studies consisted of several different projects lasting from 1956 to 1965. The first was limited to 299 Chicago area suburban housewives who had pre-high school children and who lived in selected areas of 12 sample suburbs, drawn with the help of the research staffs of the *Chicago Tribune* and the Bell Telephone Company to present socioeconomically divergent "new suburbanites." The instrument was a depth interview, and the work made possible with a grant from the *Chicago Tribune*. This was followed by the use of the same schedule on 700 divergent urban housewives and working women. I was assisted by a major faculty research grant from Roosevelt University and two fellowships from the Midwest Council for Social Research on Aging. Additional interviews, of a more structured nature, were used to further investigate various aspects of the different roles of married women.

³ The widowhood study was conducted with the help of an Administration on Aging grant (a division of the Department of Health, Education, and Welfare) (grant no. AA-4-67-030-01-A1) and was aided by Roosevelt University. It involved a year of exploratory work and then interviews with a modified area probability sample of

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metropolitan-area women in America. Attention is focused on the woman's educational achievement rather than her social class, which is usually determined by the occupation, education, and income of her husband. It is obvious that the amount of schooling achieved by a woman is related to that of her husband and thus to his occupation and income, but there are advantages in looking only at the wife's education.

In the first place, it is hard to compare married women and widows by the characteristics of their husbands or even by family income because of the changes that occur after the death of the husband. Second, the importance of educational achievement as a factor influencing the woman's social roles and attitudes became apparent in the first study, which consisted of several waves of interviews with samples of suburbanites, urbanites, and working women. The conclusions drawn from that research were tested in a study of a modified area-probability sample (drawn by the National Opinion Research Center) of 301 widows aged 50 and over. This paper departs from the usual procedure of reporting only the most recent research by including some data and conclusions reported earlier (Lopata 1966, 1969a, 1972). However, all tables and references to scales come from the study of widows. The analysis will proceed through each of the basic roles of urban women, first as wives, then as widows (Znaniecki 1965).⁴ Involvement in social roles will be analyzed with the help of two scales: the relations-restrictive attitude scale⁵ and the social isolation scale.⁶

301 Chicago area widows aged 50 and over, drawn and conducted by the National Opinion Research Center in 1968. I wish to thank my three consultants, Ethel Shanas, Bernice Neugarten, and Robert Winch, as well as the members of the Midwest Council for Social Research and the NORC staff for all their help. A new study of the support systems involving widows of all ages in urbanizing areas is currently being conducted with the help of a contract from the Social Security Administration. It will hopefully become comparative with the help of Dr. Adam Kurzyrowski of Warsaw, Poland, and Dr. Nada Smolic-Krkovic of Zagreb, Yugoslavia. Of special help have been Dr. Henry Brehm, director of research for the Social Security Administration, Frank Steinhart, and other members of the Center for the Comparative Study of Social Roles. Outside of these two studies there have been few research projects concentrating on widowhood. Peter Marris (1959) studied women whose husbands had died around the age of 50 or under, Berardo (1967) compared urban and rural widows and widowers in the state of Washington; Pihlblad, and Adams (1969), recently completed secondary analysis of the Pihlblad and Rosencranz (1968) data on aging in small towns of Missouri. There are several studies now in progress which should help bridge the knowledge gap.

⁴ The different roles performed by women, such as wife, mother, daughter, friend, neighbor, community member, and worker, were ranked by each respondent in relation to the other roles. References to any role pulled out of the whole interview and selected items from the relations-restrictive attitude scale were combined to obtain an overall score in each role. For example, the friend role got an extra point if the respondent explained that she got most help in her period of grief from her friend. The scores were then collapsed into three levels: high, medium, and low.

⁵ The relations-restrictive attitude scale measured the respondent's attitudes toward

I will attempt to show that educational achievement of urban women is highly related to their social life space, that is, to the degree of their involvement in a multidimensional set of social roles at various stages of the adult life cycle and to their attitudes toward people, both significant others and depersonalized members of the community. The assumption that has guided this analysis is that industrialization, urbanization, and expanding social complexity, accompanied by mobility, are decrystallizing the social groups to which women have traditionally been socialized to such a degree that skills developed through formal, abstract education are becoming increasingly necessary to help them engage in social systems.

THE ROLE OF WIFE

Interviews with housewives showed that less educated married women restrict relations with their husbands to the more physical aspects of what Rainwater, Coleman, and Handel (1959) called "servicing" and hold many negative attitudes toward them. White women tend to resent their husbands' efforts to establish and maintain patriarchal authority and define the treatment they receive from their fathers and husbands as harsh. Black women tend to feel that their husbands are irresponsible and unfaithful and resent having to insure their own and their children's survival (see also Clark 1965). Marriages of less educated women are not stable in either

various segments of her social life space, through a series of 36 statements with which different degrees of agreement were obtained. The first 15 of these were generalized attitudes, such as "Relatives are your only true friends" or "People take advantage of you when they know you are a widow." The next 21 statements were personalized as, for example, "My husband was an unusually good man," or "This time of my life is actually easier than any other time." The scale provided a total score, but its separate items were also run against each other to determine, with the help of gammas, the clustering of attitudes. Attitudes toward specific role partners were also included in the different role scores, but not more than three were ever involved in the latter scales.

⁶ The social isolation scale consisted of 45 items establishing the degree of contact a widow has with other people. Every respondent gained 1 point each time she recorded the lack of a child, sibling, grandchild, or greatgrandchild or of contact with such relatives at a frequency of less than a few times a year. Points were also given for the lack of membership in voluntary associations, a car for transportation, relatives in the neighborhood, or fellow occupants in her household. Counted were also the absence of overnight guests, of visits by her to the homes of others overnight, and of trips during the past year; the absence of help from anyone in the period after the husband's death or from in-laws in various spheres of life at any time since then. A point was also given the respondent if she did not have "acquaintance" or interactions with neighbors, hobbies, adult education courses, telephone calls during a day or letters during a month, or any activities during the day, evening, or weekend which indicated the presence of other people. Since several of these items had subunits, the highest score could be 80 and the lowest 00. Only 38 of the 301 widows scored high, having accumulated between 55 and 80 points, 211 scored between 54 and 26 points, and 52 were very low on the social isolation scale because their total of points fell between 01 and 25.

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expectation or in fact because of death, desertion, and divorce, and those that do survive are often characterized by very little affection. In general, with decreasing education, communication between husband and wife decreases in depth, variety, and frequency (see also Komarovsky 1962). Many wives, for example, do not know the educational achievement, family background, or current work experience of their husbands. They deal with their husbands concretely, when they are present, and do not "take the role of the other" in situations away from the home. Less educated women believe that they do not and should not have any influence on the husband's job, or they limit it to their actions in the role of wife in the home. They see their influence as creating a home free from conflict and providing good meals, perhaps pushing the man to take a better job, or on the contrary, as simply refraining from nagging him. Better educated women become much more involved in the work of their husbands, indirectly by listening and showing an understanding of his problems or directly through the performance of specific services, such as bookkeeping or entertaining.

The attitudes which less educated wives have toward their husbands are carried into widowhood (Lopata 1973). In the study of widows, a scale combining all references to the husbands indicates that the less educated woman is much less positive about her late spouse and much less involved with him in retrospect than the more educated woman (table 1). The

TABLE 1
HIGH, MEDIUM, AND LOW SCORES ON THE ROLE-OF-WIFE
SCALE BY EDUCATION OF RESPONDENT
(%)

ROLE-OF-WIFE SCALE	EDUCATION (YEARS)		
	Less than 8	8-12	13+
High	22	35	41
Medium	45	48	50
Low	33	17	9
Total	100	100	100

NOTE.— χ^2 with 4 df = 12.42, $P < .02$.

death of the husband disrupts the latter's life to a greater extent than it does the former's, since her role as wife is more multifaceted, and the husband is likely to have been an important member of her social circles in other roles (Lopata 1969b). At the same time, the higher the education, the more skills the woman has to reengage in society in new ways, by recombining, transforming, or developing new social relations after the period of grief; thus, she ends up less socially isolated. The less educated woman

is minimally engaged in social relations prior to the death of her husband, and for her that event simply means another step in the disengagement process (Cumming and Henry 1961).

THE ROLE OF MOTHER

The less educated urbanites, both black and white, and the white suburbanites, who contributed to the *Occupation: Housewife* study rear their small children traditionally, concentrating on physical maintenance and believing that their personality is inborn or, anyway, that mothers are pretty helpless in developing it. Living usually in neighborhoods with high criminal and delinquency rates, they worry about "bad influences," sometimes to such an extent as to experience constant anxiety. These mothers with teenage children express greater concern for the physical and social safety of their children and trust them less than previous generations seem to have done. The main satisfaction that the mothers of older children express about life is: "the children turned out all right." As the children, especially the males, get older, the less educated mothers tend to become increasingly passive in their attitudes and relations to them, feeling that males cannot be controlled. They are often faced with offspring whose lifestyle in adulthood makes them uncomfortable, especially for those children who are upwardly mobile. Black children, especially the boys, move out of their mother's home earlier than white children, even before marriage (see also Lopata 1972). Eventually, most children leave home; almost half of the widows live alone, and only 22 out of the 301 live with married children and their spouses (Lopata 1972; see also Shanas et al. 1968).

Contrary to many cultural assumptions, less educated women do not use their role of mother to guarantee social contact, in spite of the paucity of their involvement in other social roles. Because of geographical scatter and status decrystallization of their families, most older widows develop asymmetrical relations with different children, seeing, helping, or being helped by one child more than by others. At all educational levels, widows tend to have a low average frequency of contact with all living children. The less educated often see one offspring more often than the others, and they are apt to have very rare contact with children living at a greater distance. The more highly schooled try to develop systematic rhythms of contact with all children, although it also does not provide them with complete involvement in such interaction. In addition, the less educated tend to hold more negative attitudes toward adult offspring: they are more likely than the better educated to feel that they are imposed upon when they are asked to perform services which they would not voluntarily offer (table 2, statement 1).

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TABLE 2

AGREEMENT WITH SELECTED ITEMS FROM A RELATIONS-RESTRICTING ATTITUDES
SCALE BY EDUCATION OF RESPONDENT

STATEMENT	EDUCATION (YEARS)		
	Less than 8	8-12	13 Plus
1. Agreement with, "One problem with adult children is that they always want you to do favors for them—baby sit, or sew or things like that," by education of respondent.	50 (34)	39 (61)	11 (3)
2. Agreement with, "My brothers and sisters became much more important to me after I became a widow," by education of respondent.	29 (18)	42 (67)	36 (9)
3. Agreement with, "Relatives are your only true friends," by education of respondent.	39 (33)	21 (37)	7 (2)
4. Agreement with, "Other women are jealous of a widow when their husbands are around," by education of respondent. ...	57 (37)	39 (65)	16 (10)
5. Agreement with, "My present income makes it impossible for me to maintain old friendships," by education of respondent.	36 (25)	21 (36)	10 (3)
6. Not belonging to any organization before becoming a widow.	57 (43)	48 (92)	25 (8)
For all respondents	(76)	(193)	(32)

NOTE.—Figures in parentheses = *N*. 1. = χ^2 6 df = 15.03; $P < .02$. 2. = χ^2 2 df = 2.96; N.S. 3. = χ^2 6 df = 30.14; $P < .001$. 4. = χ^2 6 df = 12.35; $P < .10$. 5. = χ^2 with 2 df = 9.61, or $P < .01$. 6. = χ^2 with 4 df = 35.8, or $P < .001$.

THE ROLE OF KIN MEMBER

For both the housewives and the widows studied, siblings have not been easily or regularly available for social contact. For the less educated woman, married or widowed, they serve mostly as emergency-contact relations; for the more educated, the patterns of contact are more systematized, particularly around holidays, but relatively infrequent. The role of sibling, or for that matter of daughter or grandmother, was seldom mentioned by the married housewives and working women in response to the many open-ended questions exploring their social relations. The widows did not vary greatly, by level of education, in their scores on the role of sibling (χ^2 with 2 df, $P < .25$), and only 12 women scored high in this role when all references to such relatives were combined. Nor was the association any different when education was cross-tabulated with agreement with the following

statement: "My brothers and/or sisters became much more important to me after I became a widow" (table 2, statement 2).

Mobility and mortality probably account most often for the fact that the sibling is not as important in the lives of older widows as might be expected from reports of help in the extended family (Sussman 1963; Litwak 1968). Particularly among the less educated, many are first-generation city residents who have left some siblings behind in "the old country" or in another state, and these relatives are seen sporadically and rarely. Contact with siblings is limited to those who are concretely and easily available. Financial limitations and the unplanned nature of their life-style prevent regularized interaction with those at an inconvenient time-distance. Average frequency of contact is low, lower than among the better schooled, but emergency help patterns are retained, particularly in time of illness, death, or family dissolution. The frequency of such events is high in the lower-class community, where the less educated tend to be located, and kin members pitch in to share housing, health care, or child rearing. However, such occasions are interspersed with long periods of social isolation and cannot be built up into a picture of constant companionship in accordance with the myths about lower-class family solidarity.

For the older widow, the role of grandmother also tends to be complicated by the class gap and by geographical dispersal. Grandchildren who live at a time-distance beyond convenience are not seen often and do not form a major source of social integration. Feelings of closeness to grandchildren are dependent on the frequency and the nature of contact, but particularly on the absence of interference from the intervening generation.

THE ROLE OF FRIEND

The *Occupation: Housewife* studies concluded that couple-companionate friendships are replacing sex-segregated relations in the American society and that these are most prevalent in the upper and middle classes (see also Babchuk and Bates 1963, 1965). Increase in the number of reported friends accompanied an increase in education. The people who reported that they lack friends were the most minimally educated, married to minimally educated husbands, having experienced so much geographical mobility in recent years that they no longer live in "the old neighborhood." The widowhood study expanded the sources of data on friendship, and educational variations among these older women proved important in differentiating their attitudes toward this role. In the first place, the more educated women reported more strain in interaction with friends after the husband's death than the less educated ones did. The basic reason is the couple-companionate nature of their prior relations, which became asymmetrical

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with the death of a term partner. At the same time, these women, accustomed to having friendships outside of the family and equipped with skills in building new social relations out of a large pool of acquaintances, are able to enter and execute the complicated series of steps necessary to develop new or modify old interactions.

The less educated widows lack both the attitude toward friendship as a relationship which can be purposely developed and the knowledge that some steps can lead to an intensification of casual interaction (unlike Carnegie's 1937 readers). They never were involved in couple-companionate relations and, as the old sex-segregated chums of childhood days become unavailable for contact, they increasingly report no friends. They see the world as containing people who are already close and trustworthy or strangers. These are the very people who social gerontologists find to be dependent for social interaction upon location in a dense population having the same characteristics as themselves (Rosow 1967). The rather rare members who are educated do not need such an environment; and the very uneducated cannot take advantage even of such contacts because of their self-isolating attitudes. Even in dense areas of older people a great deal of social isolation is found (Blau 1961).

The relation between education and attitudes toward friendship can be seen in the responses to several statements contained in the widowhood schedule. For example, the less educated women are more prone than the more educated ones to agree that "relatives are your only true friends" (table 2, statement 3). Anyone really believing this statement shuts out the possibility of becoming intimate with nonkin members and can become isolated if the relatives are not available for contact. Another significant association is between the level of education and the assumption that "other women are jealous of a widow when their husbands are around" (see table 2, statement 4). The dependence upon physical proximity for relational maintenance on the part of the less educated widow is further shown by the relation between educational achievement and agreement with the statement: "My present income makes it impossible for me to maintain old friendships" (table 2, statement 5). Of course it is likely that widows who had reached higher levels of education and married men who became successful in life are not apt to be constricted in their finances at the present time. However, many widows living on very restricted incomes disagreed with the statement (in fact, the association between income and agreement is not significant). The combination of attitudes indicates that the less educated women find it difficult to retain friendships with childhood associates without opportunities for spontaneous contact and fail to build new relations of such intimacy from the resources at hand, in married life or widowhood.

THE ROLE OF NEIGHBOR

The studies of housewives and working women came to two basic conclusions as to the relation between neighboring and education: first, that the complexity and level of neighboring increased with education; and second, that the school finishers at any level had a higher combined score than the nonfinishers.⁷ The latter result suggests the presence of different types of neighborhoods in which finishers are the elite for their area. Thus, wives with a high school diploma seem to have more of the abilities and attitudes which help them integrate into the neighborhoods in which they are living than the high school dropouts, and even somewhat more than the college dropouts. Not only did the overall score increase with an increase in educational level in spite of these fluctuations, but the component forms of neighboring showed dramatic variations. The least educated women scored higher in the exchange of services with neighbors, such as borrowing or lending, than their more highly educated counterparts, but they obtained a lower total neighboring score because they did not interact frequently with neighbors in sex-segregated formal or planned interaction, had husbands with low scores, and did not engage in couple-companionate activities. Women in the middle ranges of schooling interacted spontaneously, even frequently, when they lived in certain suburbs; they were very surprised and pleased by the "friendliness" of the neighborhoods and they exchanged services. The higher the education, the more the total score was influenced by the husband's level of neighboring and by couple-companionate interaction. Services decreased in importance, and the personal interaction among neighbors changed from the very informal, seasonally influenced, and casual contacts away from the residence to the more formal, "event-organized" contacts not dependent on chance. The suburbanites neighbored more often than the urban housewives and the urban working women.

The findings concerning married women help explain the fact that widows are not highly involved in neighboring and do not vary much by education in their total scores, lacking the dip pattern entirely. In the first place, few of them continue to live in the suburbs after the death of the husband. Second, their general educational achievement is lower than that of the married women, who are on the average much younger and better educated, in accordance with the modern trends in

⁷ Neighboring was investigated extensively in the *Occupation: Housewife* studies and, finally, a complex model was developed which combined noninteraction, low, medium, and high levels of interaction with several different "forms of neighboring." The forms included: neighboring among women, borrowing and exchanging services, husband interaction, couple-companionate interaction, and definition of the neighborhood in terms of friendliness. The different social classes and different levels of educated women proved to have divergent neighboring patterns, both in level and in component forms.

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American society. Third, none can now build a score from the husband's or the couple's interaction. Thus the better educated widows cannot be involved in the multiform type of neighboring which raised the scores of their married counterparts. The least educated widows are lower than the wives, but not very dramatically, nor do the forms of neighboring vary. If they neighbor at all now, the higher achievers are limited to sex-segregated "event socializing," while the lower achievers depend on sporadic, chance encounters. In general, few widows (27 out of 301) are highly involved in the role of neighbor, while 83 fall in the medium levels of involvement, and twice as many of them are low.

PARTICIPATION IN VOLUNTARY ASSOCIATIONS

The relation between education and membership in voluntary associations which was found or implied by class variables in other sociological studies (Bell 1956; Komarovsky 1962; and Bell and Force 1956; Bell and Boat 1957; to name just a few) proved particularly strong in the case of the suburban housewives, less so with urban housewives, and still less with urban working women in the *Occupation: Housewife* study. One set of data which threw off the anticipated membership total for urban women was the presence in the sample of relatively large numbers of ethnically affiliated and black women who belonged to church groups, in spite of a low level of educational achievement.

Education proved to be a highly relevant factor in voluntary association memberships in the past of older widows (table 2, statement 6). Only a fourth of the less educated ever belonged to one or two such groups, and less than that now hold three or more memberships. Only about half of the widows aged 50 and over who once belonged to social groups are now able to continue such memberships, and most of the ones who dropped out are the less educated. One reason is the lack of transportation; most urban widows do not drive or maintain a car. Those living in deteriorating neighborhoods are afraid or at least hesitate to go out alone. To be picked up by friends inconveniences the latter, and mobility after widowhood tends to increase the distance to meetings. The less educated women are also likely to be suffering from failing health and financial limitations. This combination of factors makes involvement in voluntary associations simply too difficult, even for those who had at some time found a group with which they could identify.

There is an interesting difference between the black and the white respondents when past memberships (prior to widowhood) are compared. The black women with less than eight years of schooling are more apt than their white counterparts to belong to voluntary associations, mostly

churches or block clubs.⁸ However, they are the ones most likely to drop such memberships in widowhood and to list health and financial problems as major reasons. Thus, although more active than the whites in the prime of life, they are most affected by hardships and end up restricting their lives so much that there is no significant difference between the races when current participation is examined. On the other hand, two-thirds of the respondents listed going to church as the only or main activity of a weekend, compared with only 16% of the whites who concentrate on this activity. Also, even those who do not now belong to groups (two-thirds) or mention church attendance find their religion important to them. Of the blacks, 81% as compared with only 57% of the white widows, claimed their religion to be "extremely important."

SOCIAL ISOLATION AND RELATIONS-RESTRICTIVE ATTITUDES

The conclusions concerning the relationships between educational achievement of the widow and her involvement in various social roles are borne out when her total social life space is measured. A score on a social isolation scale was obtained for each respondent by adding a point each time she recorded a lack of involvement in any category of social relations, ranging from children, siblings, and grandchildren, to neighboring and membership in voluntary associations. The connection between education and social isolation, as thus measured in many spheres of life, is a very strong one (table 3). The less educated the women, the more socially isolated she is in widowhood. Her education is, in fact, even more influential than is the education of her husband, although that association is also significant (χ^2 with 18 df = 35.27 or $P < .01$). It is not surprising that the relationship of husband's schooling to his wife's social isolation is similar to her own, since women tend to marry men with similar schooling. White women tend to marry slightly up, or men with more years of formal education, while black women tend to marry slightly down. However, it is the amount of schooling found in her background that seems to determine the woman's ability to cope with the urban environment. The less schooling she has, the less able is she to create a satisfying environment for herself. Even when her husband is alive, the wife's

⁸ The association between education and social isolation is stronger for white than for black women in the total scores and in component parts. One sixth (52 out of 301) of the sample of Chicago area widows are black, and 82% of them had never gone beyond grade school as compared with 42% of the whites. However, the possibility that the relation between education and isolation was inflated by their presence in the sample is negated by the fact that the gamma is only $-.068$ between these variables for the blacks, while the gamma is $.489$ among the whites. The same holds true, for example, for the association between education and membership in voluntary associations (.061 gamma for the blacks and .381 gamma for the whites).

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TABLE 3

HIGH, MEDIUM, AND LOW SCORES ON THE SOCIAL ISOLATION SCALE
BY EDUCATION OF RESPONDENT
(%)

EDUCATION	SOCIAL ISOLATION SCORE			Total N = 301
	High (55-80) N = 38	Medium (26-54) N = 211	Low (01-25) N = 52	
No school	9.0	90.9	0.0*	3.6
1-4 grades	12.5	87.5	0.0*	7.9
5-7 grades	31.7	65.8	2.4	13.6
8th grade	17.1	65.7	17.1	23.2
Some high school	6.5	80.3	13.1	20.2
Completed high school	6.4	66.1	27.4	20.5
Some college	5.5	55.5	38.8	5.9
College graduate	0.0	50.0	50.0	2.6
Graduate or professional training	0.0	50.0	50.0	1.9
Total	12.6	70.0	17.2	99.4

NOTE.— χ^2 with df = 45.22 or $P < .001$.

* In order to compute χ^2 the number 1 was substituted for 0 in blank cells. The additions are not sufficient to warp results.

schooling seems to have a significant influence on her role involvement. In spite of the many sociological assumptions, it might be useful to examine in greater detail the influence of the wife's schooling on family life-styles, instead of always taking her husband's as the basic variable.

The widowhood study tried to determine not only the woman's involvement in various social roles, but also her attitudes toward the people in her social life space. The relations-restrictive attitudes scale consisted of 36 statements from which the respondent's degree of agreement was obtained. A high score on this scale indicates a very negative attitude toward others. Some items on that scale were designed to measure involvement in particular roles, but the overlap consisted of at most three items out of 36 or more. In fact, although the relation between the isolation scale and the relations-restrictive attitudes scale is significant (χ^2 with 4 df = 10.40, or $P < .05$), it is much less so than the relation between either scale and schooling (see table 4). This means that, although they hold negative attitudes toward at least one set of their social relations, some widows are involved in social contacts, at least on a medium level. Others holding no grudges or hostile feelings toward the world are nevertheless social isolates. However, less educated women tend to be located at the high end of both scales at a rate much greater than the more educated (table 4). There is no doubt that the less educated widow is more hostile to her environment and is more socially isolated than her better trained counterpart. Furthermore, the profile of the isolated and hostile woman shows her to be not only uneducated,

TABLE 4
PERCENTAGES OF WIDOWS EXPRESSING AGREEMENT WITH SELECTED ITEMS FROM
RELATIONS-RESTRICTING ATTITUDE SCALE BY THEIR SCORE ON SOCIAL ISOLATION
SCALE WITH γ AND χ^2 TESTS OF ASSOCIATION

ATTITUDE	N	SOCIAL ISOLATION SCORE			γ	χ^2
		High (%)	Medium (%)	Low (%)		
Total population	301	13	70	17
I have trouble being nice to people who did not help during my period of grief:						
Agree	29	28	69	3	.65	13.91§
Disagree	240	8	72	20
Not applicable	32	34	56	9
People take advantage of you when they know you are a widow:						
Agree	117	20	69	11	.41	12.38‡
Disagree	167	8	70	22
Not applicable	17	12	76	12
Widows are sexually propo- sitioned, even by husbands of their friends						
Agree	58	21	67	12	.39	9.65†
Disagree	195	7	73	20
Not applicable	48	25	62	12
Many widows who remarry are very unhappy in that marriage:						
Agree	128	19	70	12	.39	9.49†
Disagree	111	7	71	22
Not applicable	62	10	69	21
My present income makes it impossible for me to maintain old friendships:						
Agree	64	17	73	9	.38	6.86*
Disagree	209	9	71	20
Not applicable	28	32	57	11
I wish people wouldn't try to get me to go out and do things all the time:						
Agree	67	19	70	10	.38	8.37**
Disagree	209	8	72	20
Not applicable	25	32	52	16
I wish I had more friends:						
Agree	101	17	70	13	.31	7.06**
Disagree	173	8	72	20
Not applicable	27	30	56	15
My husband was an unusually good man:						
Agree	262	11	70	19	.54	6.58*
Disagree	24	25	71	4
Not applicable	15	27	73
Of the men I have dated I have most in common with widowers:						
Agree	43	2	79	19	.38	3.98*
Disagree	92	12	76	12
Not applicable	166

NOTE.—df = 2. †P < .01.
*P < .05. ‡P < .005.
**P < .025. §P < .001.

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but lacking other facilities for social involvement. She is likely to have been married to a blue-collar husband with a limited education, to be now living on a very restricted income, never to have had an income above average, and to be suffering from physical ailments which bother her so much that she does not make any effort to engage actively in society. Her personality tends to be passive vis-à-vis the world; she waits for people to come to her and is disappointed when they do not. She is black and unemployed or working only sporadically, or of Italian or a mixed European ancestry, and minimally connected or completely unaffiliated with a church. She is not apt to be Jewish. She lives in an apartment building containing 10 or more units or in a traditional Chicago "two-flat," but not in her own house. She feels financially restricted, and the memories of her past are not very pleasant. This profile negates the conclusion of some social gerontologists that the socially isolated are happily disengaging from a prior active life in anticipation of death (Cumming and Henry 1961).

SUMMARY AND CONCLUSIONS

One of the conclusions drawn from the studies of suburban and metropolitan housewives, working women, and widows is that educational achievement of the woman influences the direction, the type, and the level of her social engagement, and should be studied more carefully in future research of life-styles. Second, those widows who are socially isolated hold negative feelings toward the world and should by no means be considered as positively disengaging from a full and satisfactory set of social relations.

The evidence of these studies points to the need women have for the skills which are obtained through formal schooling. The most isolated are the women who have not achieved any level of education above grade school. Their relations with their husbands and with adult children tend to be restricted in depth and scope; they have few social roles which provide a steady and multifaceted involvement, and they lack the attitudes and skills necessary for social reengagement at a broader level when previous life patterns are disturbed by mobility, death, or other crises. They face frequent disruptions and spend much of their life in relative social isolation, waiting until someone comes along to relieve it temporarily. Modern urban life is increasingly breaking up the previously stable ethnic communities, and people are migrating from villages and small towns to heterogeneous areas of the metropolis. The rate of social change, especially the change experienced in the lives of women, is increasing the potential for social isolation on the part of those members of this sex who were trained into passive stances and who hold negative

sentiments about themselves, about strangers, and about new social roles. Even many upwardly mobile women, newly deserting the culture into which they were socialized, often experience social isolation as status decrystallization breaks up old relations. Thus, there are socially isolated people in the city who lack the finances, health, or personality necessary to maintain a high level of social involvement or to reengage in new forms once old ones are dissolved. They tend to be the less educated women of a historical generation which socialized all but the elite females into a restricted social life space and a limited use of the resources of the modern urban scene. The same concentration of the socially isolated may be expected at any place in the world where the social structure has changed and a mismatch has developed between the ways people were taught to approach the world in social situations and the cultural reality of a modern, complex, urbanized, industrialized, and voluntaristic society.

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Mayoral Influence in Urban Policy Making¹

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Political behavior of big-city mayors in community policy making was examined to explore its impact on decision making. The argument that mayors are weak and act only as brokers among local interests was tested with data collected from 93 northern American cities. A comparison of community programs that were adopted with those that were rejected revealed that the probability of adoption was directly related to how actively the mayor campaigned on behalf of the program. The city government in general plays an important role in initiating community programs, particularly when the city provides a strong organization for the mayor. Consequently, the result is interpreted as not supporting the weak-mayor hypothesis.

The uniqueness of the role of the mayor in the American urban community long has been recognized. Twenty-five years ago, a large-city mayor was ranked sixth nationally in occupational prestige, immediately after Supreme Court justice, physician, state governor, member of the president's cabinet, and diplomat. Since then, the prestige of the occupation has fallen behind some professions—such as lawyer, scientist, and college professor—but its rating has been relatively stable (Hodge, Siegel, and Rossi 1964). Greer (1963) found that the mayor is one of the very few political leaders whom community residents can correctly identify by name and by stand on issues. He is the most visible politician, constantly in local newspapers and on local television. In fact, his constituents seem to have high expectations concerning the power of the big-city mayor. Sayre and Kaufman (1960, p. 657) write that the mayor's office "is the most perceptible, the most impressive; it is taken for granted that he has the most power and thus the capacity to act vigorously in the solution of the city's problems great and small." Despite the unique and crucial role that the mayor is believed to play, comparative studies of mayoral political behavior have been disappointingly scanty. Most of the literature exists in the form of case studies, and often the level of generality of their findings is uncertain, particularly when applied to different situations.

The existing studies, with a few exceptions,² tend to present the mayor as a political actor of limited powers; he is not regarded as a commanding

¹ I am grateful to Robert Crain, Matthew Crenson, Laura Morlock, and David Kirby for their comments on an earlier version of this paper. Thanks also to two anonymous readers who helped me improve the paper.

² See particularly Salisbury (1964), Rosenthal and Crain (1966), and Vanecko (1970).

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political figure. In some studies, he appears as a mere vassal of the local business community (Hunter 1953; Rossi 1965). Hunter describes the mayor of Atlanta as looking constantly to local businessmen for advice before making a move on any project concerning the whole community. Instead of deciding to "move out front" by himself, the mayor ordinarily follows their lead. Other studies describe the mayor as only reflecting the political decisions which have been reached in the community at large, ratifying and announcing decisions that have already been made by powerful groups or by municipal bureaucrats. He contributes to major substantive policy by implementing these agreements already arrived at by groups more powerful than himself. The mayor, in short, does not take the initiative in formulating urban policy. Although he is charged with the responsibility for his city's problems, he is not the one who designs solutions for them (Banfield 1961).

The severely limited power of the mayor is thought to contribute to the particular style of policy making that prevails in cities. Policy decisions are made on political grounds, and the ultimate criterion of political feasibility dictates politics of a particular sort—piecemeal departures from the status quo as these are demanded by powerful urban interest groups. According to this view, there is no room in the urban political system for successful long-range, comprehensive planning, and there is no room in the mayor's office for a far-sighted regional activist who devises and promotes programs on behalf of the "public interest." Only power brokers and bargainers are thought to flourish in the office.

To be sure, the phenomenon of political brokering is inevitably a characteristic of any mayor. The difference between one mayor and another is a matter of degree in this respect. Almost all communities need more programs than their resources can provide. Furthermore, no matter how active a mayor may be, he can concentrate only on certain key areas where he has the most to gain politically. Ordinarily, local interests are encouraged to take the initiative on projects that are either of less priority or of the kind that can be undertaken more effectively and with less opposition by the private sector. In any case, the mayor is prone to act as a power broker and to pursue a bandwagon strategy. It is the basic assumption of this study, however, that the pressure of the city's problems does not always allow him to act only in these ways. At times he must attempt to cope with these problems and must himself initiate policy. One of the purposes of the present study is to identify those political conditions under which the mayor is most likely to innovate policy.

Hunter argues that the mayor is a second-rate political leader and that businessmen are at the top of the community power structure, even though his own study ranks the mayor fourth among the top 40 community leaders. Hunter arrives at his conclusion by insisting that the source of the mayor's

presumed power is his close ties to businessmen, that, indeed, these ties are so close that a dual leadership system in the local community is not even possible. Public officials respond to the demands of powerful particular sectors. The mayor does not initiate policy, in Hunter's view, but responds to the initiatives of the business community. It is a commonplace observation that the mayor has to work with local interest groups, especially active business groups. There is no study that does not document this kind of political transaction. But this does not constitute evidence that the mayor does not initiate policy. The question of whether the mayor is active and influential in making policy is a separate one and can be empirically tested.

In the normal situation, it can be predicted that the mayor will act differently in regard to different policy proposals. If a policy proposal in which the mayor invests considerable energy is adopted while another proposal in which he invests little energy is rejected, we can term the mayor's political behavior influential. For if the amount of influence exerted by the mayor is proportionally related to the success of a project, it shows that the mayor's political behavior has considerable impact on the outcome of the community policy no matter what other interest groups participated in the program.

THE DATA

The data included in this paper were obtained from a larger study dealing with the politics of school desegregation. All 93 sampled cities have the following characteristics: a population size of at least 50,000, a location outside the South, and 3,000 or more Negro residents in 1960. The last limitation was used to exclude the cities where the black population was not sufficient to create a de facto segregated school. In fact, the final sample has another special characteristic as well: it is the subsample of the Permanent Community Sample selected by the National Opinion Research Center (Rossi and Crain 1968). The Permanent Community Sample consists of 200 cities with populations of 50,000 or more. It is a probability sample stratified by city size. The present research sample represents 95% of the population in northern U.S. cities with a population of 250,000 or more and 44% of the population of all the northern cities of 50,000 or more which contain at least 3,000 Negroes.

In each of the cities in the sample, the NORC interviewers conducted a panel of 19 interviews. In the present paper the data were taken from the interviews conducted with four respondents:³ (1) the city editor of a

³ Information collected from such a limited number of informants may possibly cause problems of reliability. While this problem may be resolved by increasing the number of interviews in each city, research costs may become unreasonable when comparison of samples in several cities is involved. The present investigation should be viewed as

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major local newspaper, (2) the mayor or his administrative assistant, (3) the chief political opponent of the mayor in the party or faction other than the mayor's, and (4) a major civic leader in the community.

The community programs discussed in this study are designated the major *adopted* programs and the major *rejected* programs. The editor provided the basic information concerning their content and participants. The identity of the major adopted program was obtained from this questionnaire item: "You say that (a problem area)⁴ has been the most important problem in this city since 1960. There have probably been a number of programs proposed that were designed to deal with this problem. We are interested in the program you consider to be the most important one adopted in this area. Would you please describe this program?" Similarly, the name and content of the major rejected program were learned from this question: "So far, we have been talking about a major new program that was actually adopted by the city. However, we are also interested in the reasons why some programs are not adopted. Considering the area of (the same problem area as above question), would you describe a program you think would have the most importance to the city but for some reason was never adopted?"⁵

Table 1 reports the various types of the major adopted and rejected programs. Nineteen cities in the sample reported no rejected program; therefore, this group of cities was excluded from the calculation in the percentages of the major rejected program. Table 1 shows certain distinct trends. First, those programs that were designed to improve race relations tended to have a higher rate of rejection if they posed a threat to established social conditions. Among them were civil rights legislation (i.e., open-housing ordinances) and new school programs (i.e., proposals for integrating de facto segregated schools). Each of these was mentioned 15% of the time on the list of the major rejected programs. Programs involving race relations but posing no immediate threat to the ongoing system tended to have a better chance of adoption. These were poverty programs, community action programs, and employment programs, such as

an initial effort at comparing mayoral powers, and the conclusions drawn from this study are subject to modification when evidence is accumulated in the future.

⁴ The interviewer was instructed to skip the education area if that was judged the most serious problem. The school programs that appear among the major adopted and rejected programs were named as solutions to the race problem.

⁵ My use of retrospective data deserves some comment here. Depending on the informant's memory can present problems, especially when the program in question was initiated some years prior to the interview. Often the respondent is unable to provide complete information. One way to deal with the problem is to check the official documents; another is to ask the same question of several informants. The latter solution was used in this study, and the answers received from different informants constituted the final information which we count as most reliable.

TABLE 1
PROBLEMS INCLUDED IN THE MAJOR ADOPTED PROGRAMS AND THE
MAJOR REJECTED PROGRAMS

	Major Rejected Programs	Major Adopted Programs
Urban renewal program	35% (32)	26% (19)
Employment program	12% (11)	5% (4)
Attracting new industry	10% (9)	5% (4)
Poverty (community action program)	9% (8)	0% (0)
Public improvement	9% (8)	5% (4)
Civil rights legislation	8% (7)	15% (11)
Human relations commission	5% (5)	3% (2)
Tax increase	3% (3)	4% (3)
Welfare measures	2% (2)	4% (3)
School program	1% (1)	15% (11)
Other programs	7% (6)	17% (13)
Total	101% (92)*	99% (74)†

SOURCE.—Editor of major newspaper.

* One case blank.

† Nineteen cities reported no major rejected programs.

proposals to create jobs in firms, factories, or public agencies. Second, proposals for urban renewal or the introduction of new industry generally had a higher adoption rate in the cities. However, when we look at their rate of rejection (26% for urban renewal programs and 5% for new-industry programs), we also see that together they constitute one-third of the major rejected programs. Since prominent bankers, developers, and industrialists frequently initiated and participated in these two kinds of programs, it is interesting to learn that there exists a considerable discrepancy between the widely publicized power of business and their ability to implement their will.

ACTIONS TAKEN BY THE MAYOR

The process of making decisions about community programs goes through several phases (Agger, Goldrich, and Swanson 1964). The major concern here is the mayor's behavior during the stages of program initiation and of interest articulation. Specifically, I will attempt to answer the questions of whether the big-city mayor initiated urban policies and whether the mayor's activities promoted or inhibited passage of the urban policies he supported.

Past studies seem to have been interested in the issue of the mayor's motivation for initiating urban policies. Dahl (1961), for example, indicates that Mayor Lee of New Haven, Connecticut, selected issues with a view toward their effect on his chances for reelection. According to this view, then, the mayor is likely to initiate a particular policy when he anticipates

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a strong demand by his constituency. Salisbury (1964), however, argues that the mayor may actively seek out solutions to certain problems he regards as critical to the city's growth. "Pressure from the constituency would not be the usual way for policy to be initiated," he writes. "The bulk of the city's working agenda is made up of proposals drawn up by the city's own technicians to meet problems identified by them or by their allies in the problem-oriented sectors of the community. The need for new revenue sources, for example, is perceived by the mayor and his staff long before public pressure is exerted, and it is the mayoral coalition which seeks a viable solution to the problem" (Salisbury 1964). The data here are not sufficient to test the validity of these two arguments, but we can test the validity of the power-broker model, which suggests that the mayor tends to wait for civic leaders or community groups to initiate policies.

The data in table 2 show who the initiators for the major adopted programs and the major rejected programs were.⁶ Apparently, if a community

TABLE 2
INITIATORS OF THE MAJOR ADOPTED PROGRAM AND THE MAJOR REJECTED PROGRAM

Initiators	Major Adopted Program	Major Rejected Program
Private sector alone	27%	56%
City government alone	24%	25%
Mixed: city government and private sector ...	49%	19%
Total	100% (92)	100% (68)*

SOURCE.—Editor and the mayor (or assistant).

* Six cases do not know.

project was to be adopted, it was indispensable to have the city government as a sponsor. Among those programs adopted by the community, about three-fourths (24% — 49%) received support from the city government. By contrast, of the programs rejected by the community, less than half were sponsored by the city government (25% + 19%). In other words, these proposals lost because they failed to win support from the city government. We learn from this evidence, then, that the city government played an important role in enhancing the chances of success of a com-

⁶ Caution has to be taken when summing across different issues. One possible criticism may be that the issue decides who will participate in the policy making. To moderate this effect, I have stressed the most important issue in the city and assumed that, in this case, motivation of various political actors for becoming involved will be more evenly distributed. Consequently, variation of political participation caused by the issue is reduced.

munity program. Without the cooperation and support of the city government, the proposed program faced a higher risk of being rejected.

When the respondents (editor and mayor or his assistant) were asked to identify the city officials who initiated the program when the city government was involved, the mayor was most often singled out as one of the principal initiators. The big-city mayors, acting either alone or together with other city officials, initiated 48% of the major adopted programs and 18% of the major rejected programs. This finding demonstrates why Banfield's argument must be rejected if the concept of brokership is taken to mean that community issues are raised and defined by private interest groups and elected officials "are slow to take up an issue presented to them by the 'civic leaders'" (Banfield 1961, p. 270). What the evidence shows here is that the mayor actually was active in initiating programs which were later adopted and was relatively inactive in the programs which were later rejected.

The basic issue goes beyond the question of whether or not the mayor initiated the program. A more important question is the circumstances under which the mayor is likely to assume initiatory leadership. Two factors seem to have facilitated the action of the political actors or groups pursuing a common political goal, assuming that they were not engaged in a zero-sum political "game" and that this coalition became an influential block in the city. Specifically, the first factor depends on whether the mayor's politics coincide with those of the dominant political group in the city, and the second depends on whether his bloc has a high degree of centralization of power. It is hypothesized here that the greater the consistency of the mayor's politics with those of the political group to which he belongs and the greater the centralization of power within the group, the greater the likelihood that the mayor will initiate important community programs.

The centralization of power can be determined by measuring the strength of party organization in the city. The reasoning is that, when the party is strongly organized, there is a better chance that the mayor and the majority of the city council will come from the same party. In American big cities, consequently, the strength of the Democratic party needs to be assessed, because Democratic voters make up the majority of the urban electorate. A second possible measure of the centralization of power is the strength of the local business community, an idea long advocated by adherents of the reputational approach to community power structure. Therefore, using these two measurements, a mayor with congruent status was defined as a Democratic mayor situated in a strong Democratic city or a Republican mayor located in a strong business-dominated city.

The index of the strength of the Democratic party in each city has been constructed by applying a four-point scale to two issues. The mayor

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(or his assistant) and the major politician were asked: "In most mayoral elections since 1960 has the support of (the Democratic party) been essential, important, not essential, not too important, or not important at all?" The editor of the major newspaper in town was asked whether the Democratic party would be "essential, important but not essential, not too important, or not important at all in deciding the fate of a school bond or tax referendum held in city?" The answers provided by the three informants were assigned a score in the scale from 1 to 4 (essential to not important at all). To obtain the final index of the importance of the Democratic party in the city, we first summed up the party's total score from the two issues and then averaged it by the number of respondents. The index is related positively to the partisan election ($r = .34$) and was checked against another measure of party strength—the patronage rate.⁷ The latter associates with party strength a gamma of $+.37$.

The strength of the business community was assessed in a similar fashion. The same three informants and the same set of questions were used, except that this time the rating was made on the degree of importance of the business community. By business community we indicated three groups in the city: bankers, industrialists, and downtown merchants. They were rated separately by the three informants and then rated together. Thus, the overall "influence of the business community" is the average importance of the three groups—bankers, industrialists, and downtown businessmen—for the same two issues mentioned above. Not surprisingly, the index of business strength is inversely related (gamma = $-.42$) to the index of Democratic party strength and positively related to nonpartisan election ($r = .22$). If the Democratic party is strong in the city, then business influence in the city tends to be weak, or vice versa. This relationship, in fact, is further substantiated by taking other measures of the strength of the business community. Consistently it was found that the frequency of meetings between businessmen and city officials when new projects were being developed increases when the influence of the Democratic party decreases (gamma = $-.44$). Furthermore, the extent to which projects were initially conceived by the business groups is a negative function of Democratic party strength in the city (gamma = $-.29$).⁸

⁷ The politician was asked to report the number of district leaders in the Democratic party and the number of them holding appointed city, county, or state offices. By taking the percentage of the latter over the former, I obtained the index of patronage rate in the Democratic party.

⁸ Data source of this information: civic leader. These questions were used: "Would you say that a number of city government's projects were initially conceived by business and civic leaders, only a few, or almost none?" and "When a city project is being developed, do government officials usually meet with business and civic leaders to get suggestions a number of times, only once or twice, or not at all?" (see Mueller et al. 1970 for the meaning of gamma. I present gamma when the index of party strength or business strength is recategorized).

TABLE 3
PERCENTAGE OF PROGRAMS INITIATED, BY CITY TYPES AND BY
MAYOR'S PARTY AFFILIATION

	DEMOCRATIC MAYORS		REPUBLICAN MAYORS	
	In Strong Democratic Cities	In Weak Democratic Cities	In Strong Business Cities	In Weak Business Cities
Major adopted program*	54% (39)	29% (14)	53% (17)	39% (19)
Major rejected program†	15% (26)	17% (12)	38% (13)	7% (14)

SOURCE.—Editor and the mayor (or his assistant).

* Four blanks in mayor's party affiliation.

† Total of nine blanks and do not knows.

The data in table 3 confirm the proposed hypothesis: the mayor who was situated in a congruent political context tended to have been active in supporting community programs that won adoption. Democratic mayors in the cities with strong Democratic parties were found initiating 54% of the major adopted programs, while Democratic mayors in cities with weak Democratic parties initiated only 29% of the major adopted programs. Republican mayors in the cities where the business community is strong initiated 53% of the major adopted programs, while Republican mayors in cities with weak business communities initiated 39% of the major adopted programs.⁹

As I indicated earlier, the mayor was less likely to initiate programs with a high risk of rejection. The second row of table 3 shows that the percentages of the major rejected program initiated by mayors are, in general, lower than those of the major adopted program. The congruent Republican mayors still maintained a comparatively higher level of activism, but the same did not hold true for the Democratic side: the congruent Democratic mayors did not initiate major rejected programs more often than the incongruent mayors.

If we call the initiation of a major rejected program a bad political investment, and if we examine the mayoral behavior of political miscalculation, we see that Democratic mayors in the strong Democratic cities committed only slightly fewer (17% — 15%) mistakes than their colleagues in weak Democratic cities. Moreover, they added to their political gains by initiating major adopted programs, outperforming their colleagues in the weak Democratic cities (54% — 29%). The same situation, how-

⁹ One could argue that, insofar as Democratic mayors cooperate with their business communities, they might also play active roles in initiating policies. My data, however, do not confirm this contention; rather, the data demonstrate a 14 percentage point difference with respect to initiation of the major adopted programs when Democratic mayors in strong Democratic and strong business communities are compared (54% — 40%).

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ever, does not exist with respect to Republican mayors in strong business cities. They committed more (38% — 7%) mistakes than did their colleagues in weak business cities, nor did they do any better (53% — 39% = 14%) in initiating adopted programs. What the reasons are for this difference between Democratic and Republican mayors is not totally clear. I suspect that effective party organization may reduce the chance of political miscalculation, because the mayor can control more actors in the community. Moreover, the cooperation of the business community with Republican mayors might be less stable than the cooperation of Democratic party politicians with Democratic mayors. Banfield and Wilson (1963, p. 266) indicated that "the respectable (established) businessman is likely to use political influence for purely private purposes only in what may be called defensive actions." It follows, then, that business influence can be more effective only in the proposals in which they have an interest.

When we present the correlates of our major variable, the mayoral initiation in the major adopted program in table 4, we see that the mayors in

TABLE 4
CORRELATIONS OF CITY CHARACTERISTICS WITH MAYORAL INITIATION OF THE
MAJOR ADOPTED PROGRAM AND WITH MAYORAL CONGRUENT STATUS

	Mayoral Initiation	Mayoral Congruent Status
Mayor-council form of government	+.05	+.27**
Partisan election	+.03	+.12
Population rank of city	-.21**	-.23**
Percentage of nonwhite	-.13*	-.23**
Percentage of foreign born	+.03	+.12
Median education	+.00	-.21**
Percentage of white collar	+.01	-.23**
Median income	+.00	-.08
Mayoral congruent status	+.19**	...

* Significant at .10 level.

** Significant at .05 level.

the larger cities, or in cities where the percentage of nonwhites is low, had a tendency to initiate more adopted programs. Nevertheless, the best correlate seems to be mayoral congruent status ($r = .19$, significant at .05 level). The second column of table 4 further displays the relationship between mayoral congruent status and other city characteristics. It shows that this variable varies positively with the mayor-council form of government, the size of the city, and the percentage of nonwhite residents, and negatively with the city's median education and the percentage of white-collar residents.

The purpose of the above discussion is to qualify the idea of the power-broker leadership system by showing that the mayor did initiate a certain

proportion of key community programs, particularly the mayor with strong organizational support. In the following discussion, we shall examine the mayor's behavior during the promotional stage of policy making to discover what effect it had on the fate of a program.

Hunter's argument is based on the assumption that consultation usually takes place between the mayor and the private sector. The first question, then, is how probable it is that the mayor has been consulted if he has not been one of the initiators of the program. The data in table 5 show us that

TABLE 5
PERCENTAGE OF ADOPTED AND REJECTED PROGRAMS BY MAYORAL ACTIVITY

	Initiators Who Consulted with the Mayor (If He Did Not Initiate Program)	Mayors Who Reacted Favorably to the Program (If He Did Not Initiate Program)	Mayors Who Had High Score of Promotional Activity
Major adopted program	61% (47)	83% (47)	68% (92)
Major rejected program	66% (56)	54% (56)	40% (68)

SOURCE.—Editor and the mayor (or his assistant).

in this respect Hunter's theory fails to predict the actual situation. Sixty-one percent of the mayors were consulted in regard to major adopted programs and 66% in regard to the major rejected programs. In other words, the private sector tended to consult the mayor as often as the mayor consulted the private sector. There are reasons why initiators of programs might have found it necessary to consult the mayor: they may have needed technical and legal advice from the mayor and his office, or the mayor is often the person who knows most about who needs to be included in the planning and where the opposition is likely to appear. But most important of all, by consulting with the mayor, the private sector discovered his opinions and forestalled possible governmental hostility. This last point is partially inferred from the data. Table 5 shows that the mayor's initial reaction to a program often revealed something about its ultimate fate. At the consultation stage, 83% of the mayors reportedly favored programs that later would be adopted; by contrast, 54% of the mayors favored programs that were later defeated.

A second finding that contradicts Hunter's theory indicates that the mayor's activity tends to affect decision making. The mayor actively promoted programs that were later adopted; he was less likely to have taken action to build majority support for programs that were rejected. The city editor and the mayor (or his assistant) were asked, "What did the mayor do to make sure that the proposed program was understood by various groups in the city such as business, labor, racial groups, and nationality

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groups?" Informants were asked to indicate whether the mayor talked privately to leaders of these groups, issued public statements, held public meetings, or did nothing. The data showed that the mayor was much more likely to do nothing about rejected programs (25%) than about adopted programs (15%). An index was constructed to measure the level of the mayor's activity in regard to both programs. For each type of activity, I assigned the mayor a score from low to high (private conversation, 1 point; private meeting, 2 points; public statement, 3 points; public meeting, 4 points; and public hearing, 5 points). The final score, which is the total score each mayor accumulated, measures the extent to which the mayor was active in the program. Table 5 shows that about 68% of the mayors had high action scores on adopted programs but that only 40% of the mayors scored high on rejected programs. The data suggest that when the mayor was only marginally involved it was simply because he had little interest in the program. He might have anticipated negative public reaction, perceived too much opposition to a proposal, or anticipated too much controversy about it. In any case, a noncommittal stance by the mayor can be expected to discourage the proponents of the program.

A third aspect of the mayor's influence can be seen in his relationship with the city council. When he was the chief proponent of the program, he usually met no opposition within the city government. Furthermore, even when there was opposition from the city council, he often succeeded in overcoming it. This finding is drawn from the answers provided by the politicians to the question of who were the major proponents and opponents within the city government of the major adopted programs and the major rejected programs separately. Table 6 shows the data. They indicate

TABLE 6
POLITICAL OPPOSITION WITHIN CITY HALL TO MAYOR'S POSITION ON PROGRAMS

	PROGRAMS	
	None	Councilman
Mayor supported the adopted program	57% (51)	18% (51)
Mayor opposed the rejected program	44% (16)	31% (16)
Mayor opposed the adopted program	*	*
Mayor supported the rejected program	53% (19)	26% (19)

SOURCE.—Politician.

* Inconclusive ($N = 2$).

that when the mayors were the winners—the chief proponent of a major adopted program, or the chief opponent of a major rejected program—57% of the cases of adopted programs and 44% of the cases of rejected programs, they were not opposed by other local government officials. For 18%

of the cases of adopted programs and 31% of the cases of rejected programs, when their most important opponents were councilmen, they were still able to overcome this opposition and emerge as winners.¹⁰ One of the possible reasons for a mayor's success is that he built a majority coalition with businessmen and the press. The data further show that the mayor was rarely the opponent of an adopted program. We can examine what occurred when he was a political loser by looking at those cases in which he supported a program that was later rejected. These cases are reported in the fourth row of table 6. It shows that, within the city government, even when the mayor supported a losing program, he was not likely to meet opposition from local government officials. His position in these cases was about the same as when he chose to work on the winning side. In a word, about 53% of the cities reported no opposition to the mayor from within the city government when he was the most important actor in city hall. It is true that the mayor sometimes encountered opposition in the city council and subsequently found himself on the losing side of the issue, but, in general, the mayor seems to have had much more political weight than the city council.

ACTIONS TAKEN BY THE PRIVATE SECTOR

So far, the data have indicated that the mayor played an important role in urban policy making. A further question, raised by the arguments of power-broker leadership and Hunter's approach to community structure, pertains to the kinds of influence business groups in the community have. Instead of posing the unsolvable, muddy question of who governs American cities, this study sought to discover what local interest groups have done in order to make a program pass or fail, what the styles of their influence were. Besides local businessmen, groups such as local labor unions, the Democratic party, the Republican party, Negroes, nationality groups, newspapers, civic organizations, college teachers and students, neighborhood groups, and reformed political groups were included in the investigation to analyze their actions during the stage of interest articulation of the two types of community programs.

Table 7 indicates the proportion of support and opposition of each interest group in regard to the two types of community programs. The data show three things. First, for each type of interest group, the likelihood

¹⁰ Some might argue that the mayor might control the city council and, consequently, single opposition could not pose a threat to his power. In some cities, this could be the case, but I am reluctant to accept this assumption even though it does not contradict our idea of a strong mayor. The mayors actually had to fight with their city councils. Los Angeles is a typical example of this kind of city (see Ruchelman 1969, pp. 215-22).

TABLE 7

STANDS OF INTEREST GROUPS IN THE MAJOR ADOPTED PROGRAM (A)
AND THE MAJOR REJECTED PROGRAM (R)

	Support (%)	Oppose (%)	No Stand (%)	Don't Know (%)
Labor unions	57 (A) 30 (R)	6 (R) 4 (A)	42 (R) 30 (A)	22 (R) 9 (A)
	+27	+2	+12	+13
Democratic party	55 (A) 27 (R)	6 (R) 2 (A)	51 (R) 39 (A)	16 (R) 4 (A)
	+28	+4	+12	+12
Republican party	36 (A) 19 (R)	7 (R) 9 (A)	55 (R) 48 (A)	18 (R) 8 (A)
	+17	-2	+7	+10
Merchants	58 (A) 25 (R)	6 (R) 8 (A)	43 (R) 27 (A)	24 (R) 7 (A)
	+33	-2	+16	+17
Industrialists	63 (A) 25 (R)	4 (R) 8 (A)	48 (R) 23 (A)	22 (R) 7 (A)
	+38	-4	+25	+15
Bankers	66 (A) 31 (R)	3 (R) 4 (A)	46 (R) 22 (A)	19 (R) 8 (A)
	+35	-1	+24	+11
Negroes	48 (A) 40 (R)	12 (R) 7 (A)	32 (R) 37 (A)	17 (R) 7 (A)
	+8	+5	-5	+10
Nationality groups	18 (A) 14 (R)	4 (R) 10 (A)	55 (R) 59 (A)	27 (R) 14 (A)
	+4	-6	-4	+13
Newspaper	82 (A) 42 (R)	11 (R) 1 (A)	27 (R) 11 (A)	20 (R) 6 (A)
	+40	+10	+16	+14
Civic organizations	65 (A) 36 (R)	2 (R) 3 (A)	44 (R) 25 (A)	19 (R) 7 (A)
	+29	-1	+19	+12
Reform political groups	26 (A) 18 (R)	13 (R) 18 (A)	49 (R) 46 (A)	20 (R) 11 (A)
	+8	-5	+3	+9
Neighborhood groups	34 (A) 20 (R)	23 (R) 17 (A)	32 (R) 44 (A)	25 (R) 6 (A)
	+14	+6	-12	+19

TABLE 7 (*Continued*)

	Support (%)	Oppose (%)	No Stand (%)	Don't Know (%)
College students and teachers . .	33 (A) 25 (R)	2 (R) 0 (A)	50 (R) 46 (A)	23 (R) 20 (A)
	+8	+2	+4	+3
Clubs and associations	38 (A) 18 (R)	6 (R) 5 (A)	53 (R) 47 (A)	23 (R) 10 (A)
	+20	+1	+6	+13

SOURCE.—Politician.

of supporting the adopted programs was in general much higher than the likelihood of supporting the rejected programs. Second, although there was an overall tendency for groups to oppose the rejected programs rather than the adopted programs, the percentage difference between opposition to the adopted programs and opposition to the rejected programs is not very large. Third, the percentage reported who took no stand or whose stand was unknown is generally higher for the rejected programs than for the adopted programs. An important implication of the findings is that the issues failed because the groups hesitated to support them or could not come up with a unified position on them. In other words, programs failed not because they were opposed but because they were not supported. I shall further point out, however, that different groups may have had different incentives for becoming involved directly in the interest articulation. Usually if a mayor acted as a representative of a particular interest group, it was unnecessary for that group to take a stand on the two programs.¹¹

Perhaps the most important findings of this table are the varying degrees of effectiveness of support and the veto power of the interest groups studied. Effectiveness of support was defined as the frequency with which an interest group supported programs that were finally adopted. Ineffectiveness, therefore, means the frequency with which an interest group supported community programs which were subsequently rejected. This measure of effectiveness is the difference between lines 1 and 2 in each column of column 1 of table 7. The figures in the bottom line of each cell serve as an index of the relative effectiveness of an interest group—the balance between support for successful programs and support for rejected programs. The findings show that newspapers, bankers, and industrialists all score very high in the relative effectiveness of their support. After them come local merchants, the Democratic party, civic organizations, and local labor unions. Two of the interest groups which score lowest in the effectiveness

¹¹ I am grateful to one of the anonymous readers for this point.

Second, the business sector of the city tended not to have high veto power. Their opposition to a program sometimes did not pay off in the expected direction. The fact that ethnic groups had the lowest veto power is also interesting, for, as mentioned earlier, while Negroes as well as ethnic groups were relatively ineffective supporters of community programs, Negroes were found to be a relatively powerful veto group.

The findings concerning the style of influence of various interest groups reflect, on one hand, some general impressions on the effects of the civil rights movement and, on the other hand, some theoretical considerations about the influence of business groups in the city. The civil rights movement, with its continual use of demonstrations and other protest techniques, increased the ability of blacks to block some proposed programs they believed to be potentially damaging to them. Because of their general lack of financial resources and prestige, however, they still did not have enough status or influence to be included in the early stages of policy making. They were able to take action only on program proposals which had already appeared on the community agenda. As for the business groups, the findings substantiate an argument advanced by Peter Clark (1961): "In sum, and with occasional important exceptions, [their] influence is seldom exercised in the form of overt acts of opposition." The endorsement of the businessman took the form of his lending his authority to civic and public decisions because "it is his mere presence and assent to a proposal that others usually seek. His approval can strengthen it by lending legitimacy; this will reduce the chance that others will oppose. His disapproval will probably not lead him to oppose it actively, but his refusal to support will weaken the proposal" (Clark 1961).

More than any group in the community, newspapers seem to have enjoyed the ability to choose the "right" side in any issue; they supported the major adopted programs and opposed the major rejected programs. The frequent appearance of newspapers on the winning side can be explained in part by their "public regarding" mission in the community. The newspaper is one of the very few actors in a city with a catholicity of interests. As Banfield and Wilson (1963) point out, the press "takes a comprehensive view of the public interest and . . . exercise[s] a powerful influence upon all other actors." In other words, one reason newspapers appeared so frequently on the "right" side is simply that they took sides more frequently than other interest groups. Aside from the sheer breadth of activism, the power of the press is derived from its influential editorial comment on community projects and from its ability to report and interpret information concerning program proposals. It needs to be pointed out, however, that the findings indicating the outstanding political record of newspapers on both the major adopted and rejected programs might be contaminated by the fact that the city editors were the respondents who

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selected both programs. It is possible that they would be likely to remember and mention those programs in which their newspapers played a role.

CONCLUSION

The behavior of the mayor in urban policy making tended to influence the fate of a proposed program. Whenever he failed to act in behalf of a program, either in its initiation or in its promotion, the chances that the program would be rejected increased. The public sector in the cities studied took a very important part in policy making. Although joint sponsorship by the public and the private sectors tended to enhance a program's chance of success, the political weight seems to have been on the side of the public sector. The data show that programs initiated by the private sector had a higher risk of rejection than those initiated by the public sector. There is no doubt that in the public sector, the mayor was the official who initiated most of the programs, particularly the mayor located in a city with a strong party organization or a strong business community, depending on his party affiliation, to support him. The notion that mayors are weak in the sense that they do not initiate policy directly contradicts the data; in addition, the argument that the mayors use a power-broker system to exercise leadership must be modified, because this study shows that mayors actually initiated a large proportion of the major urban programs. Furthermore, this study shows that the mayor was an effective supporter of his own programs.

This investigation of the influence on policy of local interest groups shows that groups differ in their ability to block or push through programs; their effectiveness depends on their political importance within the community.

This study has shown, then, the necessity of further research on our firmly held but hitherto unexamined preconceptions about politics in urban America.

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Power Distribution, Integration, and Conformity in Small Groups¹

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Utilizing data from 61 children's groups, this study examines interrelationships among peer power distribution, conformity behavior, and three modes of social integration discussed in early sociological writings, namely, interpersonal integration, functional integration, and normative integration. With requisite mathematical adjustments for interaction effects and for the effects of subjects' sex, age, camp, and group size, interpersonal integration and functional integration are found to explain substantial proportions of the variance in peer power distribution, whereas normative integration explains relatively little variance. The relationship between peer power distribution and group members' conformity behavior in an experimental situation does not attain accepted levels of statistical significance.

Social power is a pervasive social phenomenon that has attracted the theoretical and experimental interest of social scientists for many years (cf. Cartwright 1959, 1965; Gamson 1968). Even though social power usually is considered a relational concept and, moreover, one that is an attribute of a system as well as its subparts (Hawley 1963), most small-group studies have focused only upon the social power of certain individuals located within given group structures rather than upon the overall distribution of power within groups. Consequently, it has been difficult, if not impossible, to systematically examine the relationship between power distributions and various modes of integration in small groups. This state of affairs is of concern, since a basic sociological perspective would posit that power relations in small groups, as well as in other social systems, necessarily are related integrally to structural features of the group, such as members' friendship relations, the distribution of resources within the group, the members' capacities to perform important group functions, and the extent of shared norms in the group. Empirical examination of interrelationships among the foregoing phenomena is all the more important, since the available theoretical and experimental literature regarding their interaction tends to be both contradictory and fragmented.

In the present discussion peer power distribution will be conceptually

¹ This study was supported by research grants from the National Institute of Mental Health (4F1MH-23,327-02, MH18813-03), and the National Science Foundation (GS-790).

defined as the extent to which social power, or the ability to influence other group members, is dispersed throughout the group. The central focus of the definition will be upon the mean level of distributed power in groups. Three considerations are of importance with reference to the foregoing conception of peer power distribution. First, unlike many formulations to be found in the small-group literature (cf. Emerson 1962; Schelling 1960), a zero-sum model of social power is not posited. Instead, as suggested in an excellent essay by Parsons (1960), power will be regarded as the ability of system members to utilize and mobilize resources for the achievement of individual or collective goals. Consequently, it is possible to visualize increases in the power of group members without assuming corresponding decreases in power for some member, or members. Granting that power has a distributive aspect, "it also has to be produced and it has collective as well as distributive functions" (Parsons 1960, p. 221). Similarly, the above formulation seeks to avoid the zero-sum model associated with most studies of small-group coalitions and, as noted below, will endeavor to avoid the arbitrary and contrived assignment of power values to members that has characterized many laboratory studies of small-group power relations.

Second, the foregoing interpretation sets forth a rather global definition of social power and, consequently, of power distribution. The data will not permit the categorization of social power into the five various bases, or modalities, of power defined in the classic discussion set forth by French and Raven (1959), the three modes of influence discussed by Etzioni (1961), or any of the other more differentiated typologies to be noted in the literature. Third, the examination of power distributions within groups necessarily will focus upon social power as an intragroup phenomenon. This does not obviate the realization that social power oftentimes is manifested as an intergroup phenomenon. However, greater empirical rigor is afforded when the referents of power are located within the particular group(s) being studied rather than in one or more relatively less defined interacting groups. This is especially true when the data are derived from a field study, as is the present case.

MODES OF SOCIAL INTEGRATION

Most sociologists posit that all social systems, including small groups, must attain and maintain an adequate, although as yet ill-defined, degree of social integration so as to constitute a persistent order or to undergo an orderly process of developmental change. To date, however, there has been little theoretical consensus regarding definitions of "social integration." Moreover, efforts to operationalize the concept have been distinctly limited. Two widely accepted definitions clearly are unsuitable for the present discussion. Thus, for example, Bogardus (1958) has defined social integration

as "the uniting of separate entities into a cohesive whole which is something different from the parts" (p. 207). Although this formulation takes account of the existence of group properties distinguishable from those of its component parts, it is hardly a sufficient one unless "cohesion" is defined independently of "social integration," which does not appear to be the case in Bogardus's presentation. Furthermore, should one accept a definition of social integration in terms of group cohesion, the small-group literature reveals that the latter concept usually is equated solely with the mutual attraction of members of a social system (Lott and Lott 1965). An interpretation of social integration in terms of mutual interpersonal attraction may represent a necessary, but far from sufficient, step toward refinement of the concept.

Bogardus also cites a common definition of social integration as "that social process which tends to harmonize and unify diverse and conflicting units, whether those units be elements of personality, individuals, groups, or larger social aggregations" (1958, p. 207). (For a similar definition at the societal level, see Wilensky and Lebeaux 1958, p. 340.) This definition of social integration is inadequate insofar as it suggests that social integration is achieved solely through the elimination of conflict within social systems. As Coser (1956, 1967) and others have trenchantly noted, conflict frequently can contribute to the integration, as well as to the disintegration, of the social systems in which it is manifested.

In the present discussion, "social integration" will refer merely to the regularity and coordination of behavior among the members of a social system. Consequently, emphasis is placed upon the patterning of social interaction among the group's members. In order to facilitate clarification of the interacting, and sometimes countervailing, nature of the social forces contributing to the integration of small groups, efforts should be made to differentiate and examine the functional components, or bases, of social integration. Such an endeavor is most desirable in conjunction with any effort to determine the relationship between social integration and other sociological variables, including social power. Consequently, three important modes of group integration will be examined in the present study. These are normative integration, functional integration, and interpersonal integration.

Each of the foregoing modes of integration has numerous precursors, not only in the early sociological literature,² but also in the "presociological" writings of the ancient classicists (cf. Gouldner 1965). In briefly tracing

² See, for example, the writings of Comte, Spencer, Schaffle, Fouillée, Worms, Lilienfeld, Simmel, and Sumner (Barnes and Becker 1938; Barnes 1948; Hofstadter 1955; Parsons 1961; Stark 1961; Wolff 1950). Of similar relevance are the early writings of Cooley (1911), McDougall (1928), MacIver (1920), Giddings (1932), Redfield (1947), and Sorokin (1962).

their background, Angell (1968) has referred to the concepts, respectively, as "the common orientation theory of social integration," "the interdependence theory of social integration," and "the interpersonal theory of social integration." At the small-group level Newcomb, Turner, and Converse (1965, pp. 384-486) similarly have defined the most important bases of group cohesiveness as shared attitudes concerning group-relevant matters (which they label "normativeness"), coordinated behavioral interdependence (which they label "structural integration"), and mutual attraction.

Several contemporary sociologists have endeavored to operationalize the foregoing concepts and to analyze them, or their correlates, in empirical studies focused at the group, community, and societal levels. Angell (1951), for example, analyzed the consequences of "moral integration" and "interpersonal integration" in a study of American cities. Moral integration refers to "the degree to which there is a set of common ends and values toward which all the members are oriented and in terms of which the life of the group is organized." Stated differently, "moral integration is the degree to which the areas of possible friction or conflict within the group are covered by a set of moral norms that are accepted and implemented by all" (Angell 1951, p. 115). Our formulation of "normative integration" is quite similar to Angell's interpretation of "moral integration." Likewise, his operational definition of "interpersonal integration" is analogous to the one utilized in the following discussion.³

Viewing the concept from an ecological perspective, and using the community as his unit of analysis, Hawley further refined the notion of functional integration. From his perspective, the collective life of man "revolves simultaneously about two axes, one of which is symbiotic, the other commensalistic" (1950, p. 209). Symbiosis pertains to the interdependence of unlike forms, that is, units of dissimilar functions. Although a parallel can be noted between Hawley's concepts and the earlier ones of Durkheim and Toennies, symbiosis and commensalism refer directly to functions and take little or no account of such social phenomena as norms and reciprocal attraction.

More recently, normative integration and functional integration have been conceptualized as basic modes of integration at the national level. Withey and Katz (1965) posited four major modes for binding people into a national system: (1) ideological integration about a common objective, (2) normative integration based upon beliefs about the demands of specific practices, (3) functional interdependence through actually assuming roles

³ In his study of cities, Angell operationalized interpersonal integration by asking respondents, "Is this a friendly city to live in?" (1951, p. 88). In a later publication he defined interpersonal integration as "the degree to which members of any group feel at home with one another and enjoy mutual contacts" (1958, p. 9). The latter is virtually identical with the interpretation of interpersonal integration set forth below.

in a system, and (4) dependence upon a focused integrative role in which an individual or small group is required to assume the roles, concerns, and interests of large social units. Concepts 2 and 3 are directly analogous to the ones set forth here.⁴

At the small-group level, Moreno (1941) frequently is credited with providing the major impetus for a continuous series of empirical studies aimed at determining the nature and consequences of interpersonal attraction, or interpersonal integration. This mode of integration has been the subject of countless small-group empirical investigations during the past two decades (cf. Lott and Lott 1965). However, the other two modes of integration have received only a modicum of theoretical attention (cf. Newcomb et al. 1965) and relatively little empirical attention in small-group studies, particularly those situated beyond the experimental laboratory.

CONCEPTUAL DEFINITIONS

In this section conceptual definitions will be set forth for the three basic modes of group integration. Following the elaboration of testable hypotheses the requisite operational definitions will be set forth.

Normative Integration

Thibaut and Kelley (1959, p. 129) have defined a norm as a "behavioral rule that is accepted by all or most of the members of a group." In the present discussion, group normative integration will refer to the degree of consensus among group members concerning a number of group-relevant behaviors. Normative integration, as one mode of group integration, does not necessarily presuppose the existence of strong affective ties among group members. To the contrary, it sometimes has been suggested that the creation or invocation of norms represents an effort to evade affectional relations.

Functional Integration

A function will be defined as a regularly performed specialized activity which serves one or more requirements of a group (Freedman et al. 1956). It will be assumed that the functional imperatives of any group can be

⁴ When further defined by the authors, the similarity becomes even more apparent. Normative integration refers to "the more specific enforceable codes about given types of behavior," whereas functional integration "has to do with the reciprocal patterns of behavior based upon the assumption of mutual dependency and expectation and is related more to the task requirements than to the moral demands" (Withey and Katz 1965, p. 67).

reduced to three: (1) goal attainment, (2) pattern maintenance and tension management, and (3) external relations.⁵ The goal attainment function refers to the capacity of a group to progress toward whatever goals have been explicitly or implicitly selected by the group members. The function of pattern maintenance refers to the capacity of a group to maintain harmonious and consistent intragroup relations. The external relations function refers to the capacity of a group to maintain viable relationships with other groups. Functional integration of a group will refer to the degree of complementary specialization among group members and to their effectiveness in performing the three foregoing functions.

Interpersonal Integration

Interpersonal integration will refer to that mode of group integration based upon the reciprocal liking of group members for one another. In addition to treating interpersonal integration as only one mode of group integration, the present formulation differs from some others since it focuses upon the reciprocal nature of liking and, therefore, is based upon the assumption that an individual's interpersonal integration into a group depends upon both the extent to which he likes his fellow group members and is liked by them.

HYPOTHESES

A review of the relevant empirical literature indicates little direct evidence concerning interrelationships among the major variables denoted above. Nonetheless, a number of testable hypotheses may be derived from related studies.

Functional Integration and Power Distribution

By definition, a group's effectiveness at attaining various goals depends partially upon the particular goal, or goals, selected by and/or for the group. For certain tasks a centralized power structure may be highly effective, whereas for others a decentralized, or democratic, power structure may be most conducive to goal attainment (White and Lippitt 1960; Thibaut and Kelley 1959). However, in daily living groups where members of a given social unit are confronted by a myriad of changing and essential tasks, it seems plausible that a broad range of skills and role performances would be necessary for effective goal attainment. As suggested by the

⁵ The concepts of "goal attainment" and "pattern maintenance" are derived, in part, from Parsons (1951). The notion of "external relations" is similar to that set forth by Homans (1950).

situational, interactional, and functional theories of leadership (Cartwright and Zander 1968), it appears rather unlikely that a single individual, or subgroup of individuals, would be capable of performing, or facilitating the performance of, all such role behaviors for any given group. Consequently, groups in which power is broadly dispersed among all the members would seem likely to achieve high degrees of functional integration and, similarly, of goal attainment. Unlike groups constituted for a single, short-term task, this supposition should hold true, especially, for daily living groups or other social systems where the members engage in social interaction over a long period of time, within a myriad of social contexts, and around a large variety of tasks.

The above supposition would appear to be particularly valid, also, if the group members were to expect generalized satisfactions other than those associated with a particular task, or set of tasks. In residential camp groups, for instance, and in those marital relationships where the primary objective is to "have fun" or to "enjoy the relationship" per se, satisfaction would appear to be consonant with an egalitarian distribution of social power among the participants. This hypothesis has been somewhat supported by studies of family power structures (Blood and Hamblin 1958; Hallenbeck 1966; Heer 1962; King 1969; Olson 1969; Wolfe 1959), laboratory investigations concerning communication and decision-making structures (Bavelas 1960; Heise and Miller 1951; Leavitt 1951), and studies concerning the differential effects of lecture and seminar teaching arrangements (Karlins, Kaplan, and Stuart 1969). The two latter types of studies are especially relevant, since they frequently indicate that members experience great "satisfaction" with a given structural arrangement, particularly one involving an egalitarian distribution of social power, even though the group's goal attainment efforts for the assigned task(s) may be inferior to those for less democratic arrangements. Nonetheless, if "satisfaction" is a central objective for the group's members and, moreover, one that is important for member cooperation and for the attainment of other task objectives, a broad distribution of social power would appear to be consonant with generally high levels of functional integration. The foregoing considerations, then, permit the first of a series of testable hypotheses to be generated: there is a positive correlation between functional integration and the distribution of power in small groups.

Interpersonal Integration and Power Distribution

Groups with high levels of interpersonal integration are characterized by relatively great interpersonal attraction, or liking, among most or all of the members. As such, the members can be expected to possess substantial degrees of referent, or attraction, power (cf. French and Raven 1959),

and, moreover, such power is likely to be broadly dispersed within the group. Conversely, as suggested by the classic White and Lippitt (1960) studies concerning democratic and autocratic groups, groups with great power dispersion are likely to be characterized by high degrees of interpersonal liking. Innumerable small-group studies have reported a positive relationship between group cohesiveness, defined in terms of members' interpersonal attraction, and members' conformity to perceived social pressures from within the group (cf. Schachter et al. 1951; Berkowitz 1954; Deutsch 1960; Festinger, Schachter, and Back 1950). Some investigators (cf. Collins and Guetzkow 1964, p. 130) have assumed that the occurrence of high frequencies of conforming behavior in highly cohesive groups betokens great power dispersion. Presumably within such groups a situation exists in which the members exert a great deal of social influence over one another. However, such an assumption may be unwarranted. Even though members' conforming behavior may reflect strong conformity pressures within the group, it does not necessarily follow that power is highly dispersed among the members. To the contrary, members' conforming behavior may reflect their responses to the differential influence exerted by one or two extraordinarily powerful individuals. As in the present study, the two phenomena should be defined and operationalized independently. Nonetheless, the foregoing studies suggest a second hypothesis to be tested: there is a positive correlation between interpersonal integration and the distribution of power in small groups.

Normative Integration and Power Distribution

Thibaut and Kelley (1959, p. 115) contend that the greater the power group members exert over one another, the greater the tendency toward similarity of values and attitudes. To the degree that one assumes such similarities to be associated with shared behavioral norms, their contention suggests a positive correlation between the latter variable and power dispersion in groups. However, aside from studies concerning alienation and anomie, wherein member powerlessness is sometimes deemed to be associated with normlessness, there exists little or no empirical evidence regarding the validity of the foregoing supposition. Moreover, at the individual level of analysis a number of investigators posit that high-power group members are permitted considerable leeway to engage in deviant, idiosyncratic, or nonnormative behavior (Hollander 1958; Homans 1950; Torrance 1955). The latter considerations pose a basic empirical question, namely, in groups with large proportions of high-power members, is idiosyncratic behavior frequently and widely tolerated? Or, conversely, do the members seek to exert control over the possible ambiguities and conflicts arising from such circumstances through the elaboration of

relevant social norms? To examine these questions a third basic hypothesis is set forth: there is a positive correlation between normative integration and the distribution of power in small groups.

Conformity Behavior and Power Distribution

An additional empirical question is of central concern in the present study, to wit, the relationship between peer power distribution and the conformity behavior of group members. Although, as posited above, a widespread dispersion of power may be associated with the emergence of generalized social norms, the latter need not presuppose either that members' social power will be significantly compromised or that they will behave conformingly in all social situations. To the contrary, it is possible that emergent group norms will serve to sustain and protect members' respective spheres of power, to prescribe normative behaviors concerning potentially conflictual and dysfunctional social situations, and, at the same time, to permit highly individualistic, competitive, or idiosyncratic behavior in nonprescribed and/or noncritical areas. Behavior observed in typical small-group conformity experiments is apropos of the latter. In essence, then, a basic question is raised when high levels of social power are broadly distributed within groups, namely, are the members of such groups susceptible to conformity pressures perceived as emanating from their peers, or are they more capable of resisting such pressures than are the members of other groups? Once again, the classic studies of White and Lippitt (1960) suggest that members' conforming behavior may be frequent in routinized, normatively prescribed, and closely monitored social situations, but that the members may behave in more creative and idiosyncratic ways when power is broadly dispersed. The foregoing considerations, then, suggest a fourth testable hypothesis: there is an inverse correlation between group members' conformity behavior, as manifested during a brief experimental situation, and power distribution in small groups.

METHODS

Subjects

The data reported in the present study were collected from a sample of 538 boys and girls, ranging in age from nine to 16 years, who attended four camps for Jewish children located in the midwestern United States. Two of the camps, to be designated as Camps Fair and Center, were conducted by community-sponsored organizations, were coeducational, and served predominantly lower-middle-class children, as determined from reports of parents' income, occupation, and education. The other two camps were conducted under private auspices and served predominantly upper-middle-

class and lower-upper-class children. One private camp was coeducational and the other served boys only. The former will be designated as Camp Silver and the latter as Camp Pioneer.

The 538 subjects constituted 61 cabin groups. The cabin group is the basic unit of analysis for the present study. At all four camps members of a cabin group roomed together, took their meals together, and frequently participated in recreational and work activities as a unit. There were 3 boys' groups, composed of 288 subjects, and 27 girls' groups, composed of 250 subjects. Group size varied from six to 13 members. The average number of members per cabin was 8.8 and the mode was 10.

Indices

As part of a larger study (Feldman 1968) from which the following descriptions are drawn, the subjects were given questionnaires designed to measure power distribution and the normative integration, functional integration, and interpersonal integration of their groups. The indices were based upon pretest questionnaires administered to 231 children, comprising 29 groups, who ranged in age from eight to 16 years.

Normative integration.—The normative integration index was designed to measure the extent to which group members generally share a given set of norms. From a pretest pool of 120 items, 20 questions, such as "Should it be alright for the guys (girls) in this cabin to smoke cigarettes?" were selected for inclusion in the normative integration index. Identical questionnaires were administered to boys and girls except for references to gender. For each normative item, respondents were instructed to select one of five scale positions. The average score for each of the 20 items was calculated for each separate group. The extent to which the individual group member varied from the group average for each item was then determined and the member's total deviation, summed for all 20 items, was figured. The total deviation for each member was then divided by 20, the total number of items, to ascertain his average normative deviation. This figure was then subtracted from 4.00, since responses were coded from 0 to 4, so that higher scores would reflect higher normative integration into the group. The resultant score can be considered to represent an individual's "normative integration into the group." The "normative integration of the group" was then determined by calculating the average of the individual normative integration scores for each group. Groups with high scores are considered to have greater normative integration than groups with low scores.

Functional integration.—The functional integration index was designed to measure two factors: the extent to which the three earlier cited functions are performed by the members of a group, and the extent to which specialized roles are developed by, and distributed among, the group member

as they perform those functions. Subjects were asked to rate each of their fellow group members according to his perceived ability to perform certain functions for the group. Hence, although the functional integration index refers to intragroup behavior, the measure is more directly a reflection of group members' perceptions. As with the normative integration index, a five-point scale, coded from 0 to 4, was utilized. From the pretest pool of 31 questionnaire items, nine were selected for inclusion in the index, three representing each of the key group functions examined.⁶

For each of the nine items, the average rating received by every group member was figured. The member's perceived capacity to perform each of the three functions was calculated by obtaining the average of his received scores for the appropriate cluster of three questions. A score representing each member's "functional integration into the group" was calculated by obtaining the total of the three above scores, that is, scores for goal attainment, pattern maintenance, and external relations. Scores can range from 0.00 to 12.00, the higher scores representing higher degrees of functional integration into the group. To calculate the "functional integration of the group," a somewhat more complex procedure was devised in order to adhere to the conceptual definition set forth earlier. Construction of a coding table (see fig. 1) facilitated the calculations for each group. For each of

<u>Effectiveness</u>		<u>Scoring</u>
Goal Attainment	Pattern Maintenance	External Relations
Superior = 3.01 [†] (four points)		
Adequate = 2.01 [†] (two points)		
Duplications (minus two points)		
		<u>Total</u>

FIG. 1.—Functional integration table

⁶ Goal attainment, pattern maintenance, and external relations were indicated, respectively, by questionnaire items such as the following: "How often does he help the cabin to get things that it needs"; "How good is he at stopping arguments between people in this cabin"; and "Suppose your cabin needed someone to meet with other cabins. Would you choose him?"

the three key functions posited, the code number of that group member with the highest score was inserted into the appropriate "superior effectiveness" cell of the table if his score was 3.01 or above. If no group member attained a score of 3.01 or higher, the cell was left vacant. Each time one of the three cells in the "superior effectiveness" row was filled with a member's code number, a score of 4 points was recorded for the group.

In order to fill the "adequate effectiveness" cells, one of two options was possible. If the "superior effectiveness" cell for a given function was filled, one inserted into the "adequate effectiveness" cell the code number of that group member with the second highest score for the function if his score was 2.01 or higher. If, however, the "superior effectiveness" cell for the function had not been filled, one merely recorded in the "adequate effectiveness" cell the code number of that group member who scored highest on the given function if his score was 2.01 or above. Each time one of the three cells in the "adequate effectiveness" row was filled, a score of 2 points was recorded for the group. If, on the basis of the foregoing criteria, no group members were qualified to fill the "adequate effectiveness" cell, it was left vacant.

Functional integration of a group, it will be recalled, was defined partially in terms of the degree of complementary role specialization among group members. Whenever a given group member is considered especially valuable in helping the group to perform more than one of the three key functions, it is observed that the complementary distribution of functions among group members tends to become skewed. In order to account for this tendency, 2 points are subtracted from a group's score each time a member's code number appears more than once in the code table. Hence, a group's functional integration score is equal to the total of occupied "superior effectiveness" and "adequate effectiveness" cells minus duplications. Group functional integration scores can range from 0 to 18, the higher scores representing higher degrees of functional integration.⁷

Interpersonal integration.—A score representing an individual's "interpersonal integration into the group" was calculated by averaging the member's "sent" and "received" liking scores, thus accounting for the reciprocal nature of the phenomenon. A "sent" score equals the average of a group member's ratings for all of his peers, whereas a "received" score is equal to the average rating accorded to him. In order to calculate the inter-

⁷ In the case of ties within a given cell, adjustments depend upon the extent of duplications in the coding table. For example, if two group members are tied for "superior effectiveness" regarding goal attainment but their code numbers do not appear elsewhere in the coding table, the group still receives exactly four points. If, however, in the case of ties for a given cell, one finds the code numbers of persons who are represented elsewhere in the table, the duplication is accounted for by subtracting from the group's score a sum equal to two (the duplication penalty) divided by the number of persons whose code numbers appear within the cell.

personal integration of the group, one merely tabulates the mean of the individual members' scores. Scores can range from 0 to 4, the higher scores representing higher degrees of interpersonal integration.

Peer power distribution.—In order to obtain a measure of each subject's social power, every group member rated each of his peers according to the following question. "How good is he (she) at getting the guys (girls) in this cabin to do what he (she) wants?" A five-point scale was provided, ranging from "very poor" to "very good." Scores were coded from 0 to 4, with higher scores indicating greater social power, in order to provide a zero base point for the measure. It was then possible to calculate a social power score for each group member by averaging the scores received from his peers. As can be readily noted, this definition of social power is virtually identical with the formal definition set forth by Lippitt et al., to wit, "social power is the potentiality for inducing forces in other persons toward acting or changing in a given direction" (1960, p. 746). It is also similar to one of several operational definitions used by them to measure social power.⁸

Similarly, a measure of peer power distribution was obtained by calculating the average social power score for all of the group's members. As noted earlier, the above measure focuses upon social power solely as an intragroup phenomenon. Although the resultant scores are likely to comprise adequate indicators of the mean level of social power within each group, they cannot clearly indicate the ranges for scores that fall within high, medium, or low mean categories. Whereas scores for the high and low categories are likely to cluster within a narrow range, this need not be the case for groups with moderate scores on the index. Groups within the latter category could represent a variety of situations, including those in which scores are distributed unimodally at the moderate level, bimodally at the high and low extremes, or relatively equally at all three levels.

Conformity behavior.—In order to measure group members' conformity behavior, a brief experimental situation was devised. Although fully described elsewhere (Feldman 1972, 1973), the major features of the conformity experiment were as follows. A trained experimenter met separately with the members of each group and announced that all groups in the unit were to compete for a prize. The subjects were shown a drawing of an American Indian symbol and were asked to select, from a list of 11 possible answers, the single object that they thought was represented by the symbol. Each subject was given an answer sheet and was asked to circle his choice. Following selection of their answers the experimenter informed the subjects that he would tabulate their responses, report the two "leading choices," and offer everyone a second opportunity to select an answer. Subjects were

⁸ Lippitt et al. (1960, p. 747) asked their subjects, also children at summer camps, to indicate who "is best at getting the others to do what he wants them to do." However, each subject rated only two children in the group.

informed, also, that they were expected to report their second answers to the group following completion of the experiment and that the group with the highest proportion of correct answers would be awarded the prize. Following selection of the subjects' initial answers, the tabulations were reported by the experimenter in such a manner as to lead each member to believe that everyone in the group except himself had selected one of the two leading choices. In reality, however, none of the group members had selected the two answers reported as "leading choices." Conformity behavior was then measured by determining whether or not the subject, following his second choice, shifted his selection to one of the announced leading choices. In order to assure a more conservative measure of conformity behavior, the experiment was immediately repeated, utilizing a different symbol and a different list of 11 possible answers. Only those group members who conformed on both tests were classified as "conformers." The two symbols previously had been used for different purposes in a study by Bachrach, Candland, and Gibson (1961). In order to control for the effect of expertise, the true answers were omitted from the list of 11 choices. Following termination of the experiment, the subjects were informed that it was unnecessary to divulge their answers and that they need not do so. All the cabin groups were then brought together, the true purpose of the experiment was explained and, if permitted by camp policy, each participant was awarded a prize. Utilizing this design it was possible to determine the proportion of "conformers" and "nonconformers" in each group.

RESULTS

Portions of the data presented below were analyzed by means of a computer technique of regression analysis called Multiple Classification Analysis (cf. Andrews 1963; Freedman and Coombs 1966). In addition to the calculation of true mean scores for any category of subjects, this mode of analysis permits the derivation of (1) adjusted (that is, predicted, or mathematically probable) mean scores for any given category of subjects after mathematical adjustments have been made for the effects of other predictor variables, (2) η^2 scores, which represent the proportion of variance explained in group power distribution scores by a given predictor variable when adjustments are not made for the effects of any other predictors, and (3) data that represent the proportion of variance explained in group power distribution scores by a given predictor when adjustments are made for the effects of other predictor variables. For lack of a more accurate label, the latter will be referred to as scores representing additional proportions explained variance. Adjusting for the effects of other variables represents a partialling operation which is analogous to, but not identical with, mathematically controlling for their effects after data collection (cf. Coble 1965).

TABLE 1
RELATIONSHIPS BETWEEN GROUP POWER DISTRIBUTION SCORES AND
SEVEN PREDICTOR VARIABLES ($N = 61$)

Predictor	η^2	Additional Proportion of Variance Explained	Percentage Explained of the Explained Variance
Sex09	.09	13.4
Age04	.06	9.0
Camp11	.08	11.9
Group size16	.14	20.9
Normative integration08	.00	0.0
Functional integration18	.11	16.4
Interpersonal integration37	.19	28.4
Total67	100.0

Review of table 1 shows two of the three basic types of group integration to be highly significant predictors of peer power distribution. Examination of η^2 for each predictor shows that interpersonal integration explains more than twice as much of the variance (37%) in power distribution scores as any other variable. Functional integration, explaining 18% of the variance, is the next most important predictor. The requisite η^2 for normative integration (8%) suggests that it, too, may be a relevant predictor of peer power distribution.

A more accurate appraisal of the predictive power of each variable can be obtained if adjustments, or "controls," are made for their differential effects through the utilization of Multiple Classification Analysis. The requisite data are reported in the center column of table 1. More specifically, those data refer to the additional proportion of variance explained in power distribution scores by each predictor when mathematical adjustments are made for their differential effects. With such adjustments, interpersonal integration retains its status as the single most important predictor variable. In addition to the effects of sex, age, camp, group size, normative integration, and functional integration, it is seen that interpersonal integration explains 19% of the variance in power distribution scores, suggesting that it is an important correlate of power dispersion in groups such as those studied. It may be noted that the reduction in the proportion of explained variance, from 37% to 19%, most probably can be attributed to adjustments for interaction effects between interpersonal integration and functional integration ($r = .51$, $p < .001$, t -test, two-tailed). The latter variable also retains its predictive importance, following group size in the additional proportion of variance explained (11%) in power distribution scores. In contrast, given adjustments for the effects of the other variables, the

additional proportion of variance explained by normative integration is reduced to zero. Hence, it appears that the supposed significance of this variable is an artifact of its relationship with one or more of the other predictors, most probably interpersonal integration ($r = .24$, $p < .10$, t -test, two-tailed).

Perhaps one of the most important findings illustrated in table 1 is the observation that the seven predictors together explain 67% of the variance in power distribution scores. Even when adjustments are made for degrees of freedom, R^2 (that is, the total proportion of variance explained in power distribution scores by the combination of all seven predictors) diminishes only to .46, thus indicating that the foregoing cluster of variables serves as a relatively effective predictor of power distribution within groups. Furthermore, the particular explanatory significance of functional integration and interpersonal integration is further emphasized by reviewing the third column in table 1, where it is noted that the latter explains 28.4% of the explained variance in power distribution scores, while the former explains 16.4% of the explained variance in such scores. Having found that many of the above predictors are integrally related to peer power distribution, we now examine the specific nature of those relationships in light of the particular hypotheses set forth earlier.

Hypothesis 1.—There is a positive correlation between functional inte-

TABLE 2
PRODUCT-MOMENT CORRELATIONS FOR RELATIONSHIPS BETWEEN GROUP
POWER DISTRIBUTION SCORES AND THREE MODES OF
GROUP INTEGRATION ($N = 61$)

Mode of Group Integration	r	p Values ^a
Functional integration52	<.001
Interpersonal integration64	<.001
Normative integration28	<.02

^a t -test, two-tailed.

gration and the distribution of power in small groups. A brief review of tables 2 and 3 indicates that the hypothesis is supported. Groups with low functional integration are characterized by low power distribution scores, whereas those with high functional integration have the highest scores. The requisite product-moment correlation for the relationship between the two variables is .52. Utilizing a two-tailed t -test, which must be regarded primarily as a heuristic device, since the assumption of random sampling is not a technically valid one, the relevant probability level is less than .001.

The above findings are not unexpected in view of the various rationales leading to formulation of the foregoing hypothesis. However, the data

TABLE 3
TRUE AND ADJUSTED MEAN GROUP POWER DISTRIBUTION SCORES BY SEX,
AGE, CAMP, GROUP SIZE, AND THREE MODES OF
GROUP INTEGRATION ($N = 61$)

	True Mean	Adjusted Mean	N
Sex:			
Male	2.02	2.08	34
Female	2.17	2.10	27
Age (modal, in years)			
10	2.15	2.27	10
11	2.08	1.99	19
12	2.03	2.07	13
13	2.11	2.09	13
14	1.98	2.06	3
15	2.20	2.21	3
Camp:			
Fair	2.07	2.14	18
Center	2.20	2.15	19
Pioneer	1.98	1.98	15
Silver	2.05	2.02	9
Group size:			
6	1.90	1.90	6
7	1.97	2.07	10
8	2.16	2.09	7
9	2.13	2.19	14
10	2.14	2.09	19
11	1.89	1.84	1
12	2.22	2.18	3
13	1.88	1.80	1
Normative integration: ^a			
Low	1.99	2.11	21
Medium	2.13	2.09	20
High	2.15	2.06	20
Functional integration: ^a			
Low	1.90	1.97	12
Medium	2.10	2.11	36
High	2.23	2.14	13
Interpersonal integration: ^a			
Low	1.89	1.90	21
Medium	2.13	2.13	20
High	2.25	2.23	20

^a Low, medium, and high categories were obtained by trichotomization of the total sample upon the basis of scores obtained for each index.

indicate that functional integration is neither the sole nor, indeed, the most potent predictor of power distribution in small groups. Instead, interpersonal integration is the most potent predictor. This finding underscores a crucial observation, namely, that power dispersion within the groups studied is not uniquely associated with members' capacities to exert control over key group functions—capacities which, to utilize the framework of French and Raven (1959), would most probably find their bases in

"expert" and "legitimate" power, and perhaps to a lesser extent in "reward" and "coercive" power. As will be demonstrated below, interpersonal integration scores indicate, for the groups studied, that the group's network of "referent" power probably is the most potent predictor of its generalized distribution of social power. This finding may reflect the interplay of several factors. Of greatest plausibility, perhaps, is the possibility that the particular subjects studied have been socialized to view power more in terms of its noncoercive aspects than its coercive ones.

Hypothesis 2.—There is a positive correlation between interpersonal integration and power distribution in small groups. Examination of tables 2 and 3 indicates that the hypothesis is strongly supported. With requisite adjustments for all other predictor variables, interpersonal integration still explains 19% of the variance in power distribution scores. The product-moment correlation for the relationship between the two variables is .64 ($p < .001$). This finding may be taken as striking confirmation of White and Lippitt's (1960) earlier conclusion that members of democratic groups develop greater interpersonal attraction than members of autocratic groups. As noted earlier, interpersonal integration explains 28.4% of the explained variance in group power distribution scores and, therefore, is the single most important predictor variable for such scores. From these data it is clear that the extent of a group's interpersonal integration is closely associated with its internal dispersion of social power.

Hypothesis 3.—There is a positive correlation between normative integration and power distribution in small groups. Inspection of tables 2 and 3 suggests the existence of a positive relationship between normative integration and group power distribution scores ($r = .28$, $p < .02$). However, when adjustments for the effects of other predictors are introduced through Multiple Classification Analysis, the relationship is found to be neutralized (see tables 1 and 3). Normative integration explains none of the variance in power distribution, thus suggesting that the previously supposed relationship is an artifact of its interaction with one or more of the other predictors, most probably interpersonal integration ($r = .24$, $p < .10$) or sex. Unlike the other two modes of group integration, normative integration tends to be differentially associated with the sexual composition of the groups studied. Girls' groups are characterized by significantly higher levels of normative integration than are boys' groups (table 4). Consequently, with requisite adjustments for interaction effects the foregoing hypothesis is rejected. Analogous to the manner in which Coser (1956, 1967) posited conflict as functional for certain social systems, the present findings suggest that norm dissensus is integrally associated with the prevention of power centralization in small groups. Further support for this interpretation is gained from the observation that the relationship between normative

TABLE 4
PERCENTAGE DISTRIBUTION OF THREE MODES OF GROUP INTEGRATION
BY SEX OF GROUP MEMBERS ($N = 61$)

EXTENT OF GROUP INTEGRATION ^a	MODE OF GROUP INTEGRATION					
	Normative		Functional		Interpersonal	
	Boys	Girls	Boys	Girls	Boys	Girls
Low	50.0	14.8	20.6	18.5	38.2	29.6
Medium	26.5	40.7	64.7	51.8	41.2	22.2
High	23.5	44.4	14.7	29.6	20.6	48.1
Total	100.0	99.9	100.0	99.9	100.0	99.9
N	34	27	34	27	34	27
χ^2 ($df = 2$)	16.812		1.943		5.496	
p value	<.001		N.S.		<.10	

^a Low, medium, and high categories were obtained by trichotomization of the total sample upon the basis of scores obtained for each index.

integration and functional integration similarly does not attain an accepted level of statistical significance ($r = .14$, $p < .30$).

Effects of Other Variables

Sex.—The data reveal that girls' groups are characterized by somewhat higher power distribution scores than are boys' groups (table 3). However, when adjusted power distribution scores are examined, the differences between boys' and girls' groups are negligible, thus suggesting that girls' higher unadjusted scores were biased by their tendency to obtain higher levels of normative integration and interpersonal integration (table 4).

Age.—For the 61 groups studied there appears to be no consistent variation between power distribution scores and the modal age of group members ($r = -.02$).

Camp.—As noted in table 3, groups at Camps Fair and Center exhibit higher power distribution scores than those at Camps Pioneer and Silver. Following adjustments for other predictors, the differences are even more pronounced. The former two camps are the community center camps in which children were tested after living together for only 16 days. The latter two are the private camps at which children were tested after living together for 47 days. Consequently, there may be a tendency for social power in groups to become centralized over time or in accord with the higher socioeconomic status of group members. Or, similarly, the findings may indicate that subjects become relatively more aware of actual power differentiations in their groups after living together for a longer period of

time. The latter possibility appears to be relatively implausible, however, since subjects' ratings for related social phenomena (e.g., leadership scores) were not observed to decrease over time. Likewise, it is possible that the observed variations are attributable to other differences associated with the respective camp environments, such as the professional backgrounds and value orientations of the supervisory staff and other socializing agents.⁹

The data reported in table 5 are of additional interest in demonstrating

TABLE 5
PRODUCT-MOMENT CORRELATIONS FOR THE RELATIONSHIPS BETWEEN
THREE MODES OF GROUP INTEGRATION AND GROUP POWER
DISTRIBUTION SCORES, BY CAMP (N = 61)

CAMP	N	MODE OF GROUP INTEGRATION		
		Normative	Functional	Interpersonal
Fair	18	.24***	.37***	.78**
Center	19	.39***	.55*	.70**
Pioneer	15	-.01***	.78**	.48***
Silver	9	.50***	.76*	.67*

* = <.05.
** = <.001.
*** = N.S.

the varying effects of camp environment upon group power distribution. If it is assumed that a group's power distribution is in part determined by its normative integration, functional integration, and interpersonal integration, we see that the relevant relationships vary somewhat from camp to camp. In view of the lack of statistical significance for observations at all four camps, one earlier conclusion is even further supported, that is, regarding the inadequacy of normative integration as a predictor of group power distribution. For the two community center camps, interpersonal integration is found to be most strongly correlated with group power distribution, whereas for the two private camps functional integration is most strongly correlated. Once again the differences may reflect social class variations, differences in length of socialization, or the varying orientations of camp staff.

Group size.—Examination of table 1 shows that group size explains a

⁹ Professional staff at the community center camps primarily were social workers, whereas those at the other camps usually were public school teachers. In a most germane study, McLeod and Meyer (1967) have shown that social workers espouse values such as group responsibility and societal interdependence, whereas public school teachers favor the contrasting values of individual responsibility and individual autonomy. Should such value orientations be transmitted to the children, either directly or through indirect means, such as variations in camp activities and programming, the immediate social environment can be considered an important determinant of differences in group power dispersion.

substantial proportion of the variance (14%) above and beyond the effects of sex, age, and camp. In fact, this variable explains more than one-fifth of the total explained variance in power distribution scores. The product-moment correlation for the relationship between group size and peer power distribution is .25 ($p < .05$, t -test, two-tailed). Consequently, it appears that within the size range studied (all but five groups were composed of six to 10 members), increasing group size affords relatively greater opportunities for the dispersion of power among members and, moreover, for each member to exert relatively greater influence over his peers. It is most improbable, however, that this relationship would hold for groups of considerably larger size, such as 20 or more members, in which coordination problems are likely to entail a more pronounced centralization of power.

Hypothesis 4.—There is an inverse correlation between power distribution and group members' conformity behavior. The results of the conformity experiment show that 40% of the subjects ($N = 216$) shifted their second choices to the perceived group response on both tests. The requisite product-moment correlation for the relationship between group power distribution and the proportion of conformers per group is $-.18$ ($p < .08$, t -test, one tailed). Consequently, although there may be a discernible trend toward an inverse relationship between the two variables, the null hypothesis must be rejected. Further examination of the relationship between the foregoing variables is afforded through the review of data presented in tables 3 and 6. Data in the latter table indicate that the lowest proportions of

TABLE 6
TRUE AND ADJUSTED MEAN PROPORTIONS OF CONFORMERS
PER GROUP, BY CAMP ($N = 61$)

Camp	True Mean (%)	Adjusted Mean (%)	N
Fair	29.7	30.4	18
Center	33.9	31.4	19
Pioneer	56.1	60.2	5
Silver	51.8	48.7	9

conformers are found at Camps Fair and Center. Indeed, the camp variable appears to be the best single predictor of the proportion of conformers in groups, explaining 26% of the variance in such scores and 50% of the total explained variance. Perusal of table 3 reveals that the same two camps also have considerably higher mean power distribution scores than those with high proportions of conformers. Therefore, as concluded earlier, the data suggest that the immediate social environment, namely, camp, mediates the effects of a variety of intervening variables, including the distribution of power within groups. These, in turn, influence phenomena

such as members' susceptibility to experimentally induced conformity pressures.

DISCUSSION AND SUMMARY

The foregoing data suggest that at least two basic modes of group integration are potent and independent predictors of power distribution in small groups. With adjustments for the effects of all other predictor variables (sex, age, camp, group size, and normative integration), interpersonal integration was found to explain 19% of the variance in power distribution scores, and functional integration was found to explain 11%. While all seven predictors explain a total of 67% of the variance in group power distribution, the two foregoing modes of group integration explain nearly 45% of that total. For the groups studied, then, members' reciprocal liking appears to be an important correlate of power distribution ($r = .64$). The product-moment correlation for the relationship between functional integration and power distribution ($r = .52$) suggests that the latter phenomenon is integrally associated with the extent to which groups perform key functions such as goal attainment, pattern maintenance, and external relations, and, moreover, with the extent to which role performances for those functions are distributed among the group's members. Functional integration, however, clearly is of less predictive potency than interpersonal integration.

Contrary to expectation, the relationship between normative integration and power distribution ($r = .28$) appears to be a spurious one. Following adjustments for the effects of other predictors, normative integration explains none of the variance in power distribution scores. The data suggest that the presumed relationship between the two variables is an artifact of the association between normative integration and interpersonal integration ($r = .24$) and, also, of the tendency for girls' groups to attain higher levels of normative integration than boys' groups. The above findings clearly suggest that the reputed effects of group norms may be attributable in part to their interaction with other social variables. Consequently, they point to the desirability of requisite controls and comparisons in future studies of such phenomena.

Although the results of a brief conformity experiment indicate a weak inverse relationship between power distribution and the proportion of conforming members in each group, the requisite product-moment correlation ($r = -.18$) falls short of statistical significance. Nonetheless, the data suggest that a widespread dispersion of power in small groups enables members to withstand conformity pressures perceived as emanating from within the group. Additional analysis points to the significance of the

immediate camp environment in mediating the relationships among each variables.

Both group power distribution and conformity behavior are differentially associated with camp environment. Groups at the two community-sponsored camps were characterized by high power distribution and low conforming behavior, whereas the converse typified groups at the two private camps. Although it is possible that the observed variations are attributable to the differing professional and value orientations of staff at the four camps, the data do not permit the rejection of two alternative interpretations, namely, the effects of social class variations and differing amounts of shared experience prior to testing.

Within the size range studied, power distribution is positively associated with group size ($r = .25$). Although this datum cannot be extrapolated to groups of considerably larger size, it suggests that within the range studied, increasing size affords greater opportunities for social power to be distributed among the group's members. Such a tendency may be attributable to the emergence of additional group functions, including coordinative ones, and the increasing necessity for requisite role performances to be dispersed among the group's members. The latter interpretation gains additional support, it should be noted, in view of the positive product-moment correlation observed for the relationship between functional integration and group size ($r = .24$, $p < .07$, t -test, two-tailed). Since interpersonal integration bears no clear-cut relationship to group size ($r = .13$), functional integration may serve as the predominant mode of integration in larger groups, whereas interpersonal integration may be relatively more important in smaller groups. Within the size range studied, however, both modes of integration appear to be significant, especially as predictors of group power distribution. Finally, after requisite adjustments for the effects of other predictor variables, it was found that neither sex nor age composition of the groups bears any discernible relationship to peer power distribution.

In summary, then, this study represents an effort to operationalize and analyze in a field setting the relationships among basic social variables that have been of continuing interest to sociologists and social psychologists. As might have been expected, the findings tend to confirm the notion that antecedent theoretical formulations have focused upon critical social variables. Although earlier operational efforts were rather limited and disparate, the relevant theoretical constructs nonetheless are amenable to empirical study within the canons of current research methodology. And, as might have been further expected, the relationships among such constructs and varying social phenomena of contemporary interest are considerably more complex than might have been originally posited. Though sociological wisdom may advance with age, it is nonetheless contingent upon the wisdom of earlier ages.

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Neglected Characteristics of Collective Behavior¹

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Current development of a more sociological analysis of collective behavior has concentrated on the social characteristics of its conditions and its consequences. The behavior itself is less clearly conceived of in social terms. Concentration on models of the individual participant in collective behavior has drawn attention away from the fact that the behavior is enacted by collectivities with characteristics comparable to, but not identical with, those enacting institutionalized behavior. Closer and more fruitful relationships between theories of institutionalized behavior and theories of collective behavior depend in part on the development of a social-level conception of collective behavior. To this end the two forms of social organization are contrasted along the dimensions of social norms and social relationships. On these dimensions, a definition and a typology of collective behavior are suggested.

It is widely agreed that sociological thought does not adequately deal with the dynamic aspects of social life (Swanson 1971; Tiryakian 1967; Buckley 1967). Sociological theories are at their weakest in the analysis of some of the most interesting and challenging aspects of contemporary societies, including the social movements and episodic outbursts so salient in recent America. James S. Coleman has expressed the extent of these shortcomings well. "Sociology is no longer an infant discipline. . . . Yet sociologists have astonishingly little to say, either from evidence or from theory or from both, on some of the most important sociological problems in the world. . . . For example, sociologists appear to be able to say little about the genesis of the recent riots . . . [and] research in the sociology of conflict, and in collective behavior generally, languishes" (1969, p. 105). He goes on to observe:

The current neglect leads one to suspect that the whole discipline of sociology (and not just certain theoretical positions, such as Parson's and functional analysis . . .) has evolved toward the study of social statics and

¹ Some of the research reported in this paper was done at the Disaster Research Center at Ohio State University under PHS Grant 5 RO1 MH 15399-03 from the Center for Studies of Mental Health and Social Problems, Applied Research Branch, National Institute of Mental Health. Earlier drafts of this paper were read by Gary A. Kreps and Dennis E. Wenger. We thank them for their several helpful comments.

becomes impotent in the face of change. Whether this is the case, or whether it is merely that the study of social change, social movements, conflict, collective behavior, and some other transient states is simply more difficult, the end result is the same. These are the underdeveloped areas of social research. They are not only backward at present; they are not catching up. [1969, p. 112]

Also, middle-range theories of collective behavior are widely regarded as weak. Choosing only some recent examples, we find criticism of collective behavior studies of a variety of topics, including scapegoating after disasters (Drabek and Quarantelli 1967), expressive fads and fashions (Klapp 1969), the development of adolescent crowds (Smith 1968), mass hysteria (Kerckhoff 1970), radical right and left political movements (Rush and Denisoff 1971), and collective violence (Snyder and Tilly 1972). Often these criticisms, at least implicitly, show that collective behavior phenomena are analyzed in terms of ad hoc theories.

On the side of general sociological theory, there is the failure to handle adequately dynamics and ephemeral patterns of social action. On the side of collective behavior theory, there is the failure to break away from special explanations. These weaknesses are related. Both are perpetuated by the lack of theoretical ties between collective behavior and general sociological perspectives. Following Blumer (1939), sociologists generally define collective behavior as noninstitutionalized. Thus, contrasted with the institutionalized model of behavior, which implicitly underlies most sociological thought, collective behavior theory has been somewhat "encapsulated and immune to the theoretical developments in other aspects of sociological thinking" (Janowitz 1964, p. 113). This separation means not only a failure to draw from the core of sociology to construct theories of collective behavior, but also a corresponding failure of general sociological thought to profit from collective behavior studies of social dynamics.

One of us recently suggested that this gap exists because few sociologists are faced with research situations where both institutionalized and non-institutionalized behavior are equally crucial. Thus, "organizational theorists study their governmental and industrial bureaucracies while collective behavior theorists study their crowds and cults" (Dynes and Quarantelli 1968, p. 417). Brissett (1968, pp. 71-72), on the other hand, attributes the gap to early sociology's distinction between group and individual actions which equated the latter with nonnormative behavior. Whatever the reasons, the theoretical hiatus between collective behavior and other areas of sociology is artificial, unnecessary, and unfruitful. The importance of bridging it has led Turner (1964a, p. 384) to hope that a "search will ultimately undermine all of the traditional distinctions between collective behavior and organizational behavior and suggest that no special set of principles is required to deal with this subject matter."

From these premises, we deduce that it is important to relate collective behavior more intimately with other areas of sociological theory. General sociological theories and middle-range theories of collective behavior are increasingly important to each other's development. To this end we illustrate, discuss, and show implications of overcoming one of the central barriers to this needed cross-fertilization, namely, the lack of a social-organizational conception of collective behavior (Weller 1971). The argument is presented in four related steps. First, we present the points of departure that reveal our general perspective. Next, by analysis of several major theories, we illustrate that a social-level conception of collective behavior has yet to be fully realized. Third, we suggest two generic social-organizational differences that may obtain between institutionalized and collective behavior. Finally, from these generic contrasts, we draw out some implications for collective behavior theory, including a rudimentary typology.

POINTS OF DEPARTURE

Our discussion begins by developing three basic ideas. First, we identify an omission in recent efforts in the literature to develop a social-level theory of collective behavior. Next, we express the opinion, contrary to some recent critics, that important developments in collective behavior can be built upon contemporary work in the area. Third, we note the type of social phenomena that have been the empirical pattern against which we have assessed both collective behavior theory and our own thoughts.

The early Italian and French writers (LeBon 1887; Tarde 1903; Sighele 1895; Rossi 1907) described collective behavior in individualistic and psychological terms. Early scholars writing in English maintained a similar orientation (see the discussions of Ross, McDougall, Conway, Trotter, Sidis, and Martin in Manning 1970). Even the "Americanization" of the study of collective behavior (Bramson 1961, pp. 47-72) and its establishment as a sociological specialty did not alter this basic stance. Individual liberation rather than alienation was the theme of Park and his University of Chicago students (see Elsner 1972), but they still located the essence of collective behavior in the characteristics of its participants or their atypical reactions to each other.

In the last 15 years, several writers have contributed to the development of a more distinctly sociological approach to collective behavior. Central aspects of the analyses of such writers as Turner (1964*a*, 1964*b*), Killian (1964), and Smelser (1963, 1968) are explicitly expressed in social terms. However, the advantages of a fully sociological analysis of collective behavior have not been exploited. Smelser (1963) does indicate conditions at a social level which explain collective behavior; so do Gusfield (1968),

Tiryakian (1967), and Turner (1968). Consequences of collective behavior are carefully depicted by Turner and Killian (1972), as well as by Killian's (1964) discussion of social movements' contribution to social change. But, the social nature of collective behavior itself has not been adequately conceptualized. This behavior is still described in terms of psychological states of participants or atypical forms of interaction among them.

Failure to identify the social properties of collective behavior is one of the major barriers to development of a fully sociological theory of collective behavior and, consequently, is a primary factor in promoting the gap between collective behavior and general sociological perspectives. We suggest that not only the search for social conditions and social consequences of collective behavior, but also all problems of its analysis be defined at a social level. Once the distinctive social characteristics of collective behavior have been specified, it will be possible to view the behavior itself as enacted by distinctive types of collectivities, with some significant social-level differences from those collectivities that enact institutionalized patterns of behavior.² Such a definite social-level conception will provide symmetry to the analysis of collective behavior: the conditions explaining collective behavior, the collectivities enacting it, and its consequences for participating and encompassing social systems can all be conceptualized and related at a social level.

Some recent critics call for a rather sharp break with contemporary efforts of collective behavior theorists (for example, see Skolnick 1969, and Currie and Skolnick 1970). Without dismissing some worthwhile criticisms, we take exception to these conclusions. This is no time to begin again in the analysis of collective behavior. Both classic and contemporary writers have laid a foundation that can bear many useful theoretical and empirical extensions. According to Brissett (1968), the theoretical tradition of collective behavior has already played an important, if negative, role by challenging, and thereby leading to modifications in theories of, social psychology. Recent developments in the specialty also point the way to equally fruitful (and positive) contributions to social-level theories.

This tradition points to the sociological problem we wish to define and emphasize. The problem, while neglected, has not gone unmentioned (Blumer 1957; Tilly 1963). Furthermore, the materials from which we

² Heberle's (1951, p. 8) term "social collectivities" captures the same point but is used only in connection with social movements. A social collectivity consists of a core group which is the center and locus of social organization and action, as well as an amorphous extension which is comprised of a congeries of other organized and quasi-organized groups as well as of unattached individuals. Young's (1970) term "reactive subsystems," while dealing to an extent with similar phenomena as encompassed by the label "collectivity," suffers from its assumption that only conflict groups are involved and also implies more of a complex pattern than seems warranted in much collective behavior.

construct an initial step in developing the question are implicit in that tradition. For example, Durkheim in his treatment of the emergence of collective representations (1915, pp. 87-88 and 205-39) touches on matters which we emphasize. So does Sumner's (1906) sketchy analysis of the creation of folkways. Our conviction is that the tradition of collective behavior theorists is a fruitful one, kept from even more fruitful cross-fertilization with other sociological perspectives by the neglect of a central problem: what are the social properties of the collectivities that engage in collective behavior, and how do they conceptually compare and contrast with the complexes of social action engaged in institutionalized behavior?

Our third point of departure provides an empirical reference point for our conceptual enterprise. The arguments we detail later were not developed systematically from empirical studies. However, when examining existing views about collective behavior, we did use a series of research observations and a body of empirical data seldom discussed so far in the collective behavior literature. For, at the same time we were developing these critiques and extensions of collective behavior theory, we were engaged in the study of organizational and community responses to natural disasters and civil disturbances. The diverse social responses that emerge to cope with the exigencies of such community crises provide a much more fruitful prototype for the area of collective behavior than the commonly accepted prototype of the crowd with which LeBon launched the specialty three-quarters of a century ago.

Crowds are amorphous and fleeting. Systematic observations of them are difficult. Because of this and the fact that they have been relatively little studied, a number of incorrect views about their nature and behavior have developed leading to a serious question about using them as a prototype for all collective behavior. Couch (1968) points out several erroneous stereotypes of collective behavior that can be seen to be misconceptions of the crowd that have been projected upon the entire class of phenomena. Furthermore, from what we do know about the crowd, we can see that it has some features less likely to appear in other types of collective behavior. For example, the rapid turnover in leadership, the continuous but short-lived patterns of participation, and the tendency of crowds to draw the direct attention of social control agencies are important crowd characteristics not equally shared with many other forms of collective behavior.

Collectivities that can be observed in sudden and unexpected community crises provide an alternative prototype of collective behavior. We have in mind the heterogeneous array of organized responses that are elicited by two different kinds of crises, natural disasters and civil disturbances (Quarantelli 1970a). This behavior ranges from the emergence of entirely new groups to the adjustments of existing groups in a suddenly changed context. Furthermore, these social responses reflect the conflict stance of a

civil disturbance crisis or the cooperative orientation of the consensus context of natural disasters. Empirically, the range of groups involved is broad. In natural disasters, for example, there is often the concurrent appearance of many new or reorganized collectivities such as search and rescue groups, informal communication and coordination centers, committees of distant outsiders organized to assist victims, expanded welfare groups, highly elaborate and unplanned systems for handling intense medical problems, and the switching of organizations from predisaster to disaster-relevant domains of action. In civil disturbances, not only groups of looters appear, but also rioting police units, informal youth patrols, ad hoc committees of community leaders, counterrioter groups, and extensive adjustments on the part of social control and public safety organizations.³ Many of these examples do not fit the pattern of collective behavior extrapolated from stereotypes of the crowd. Nor, for that matter, do most fit within other standard types of collective behavior such as hostile outbursts, panics, crazes, social movements, and cults. But they are undeniably not institutionalized behavior. Furthermore, they do exhibit the dynamic and ephemeral qualities associated with almost all types of collective behavior. They take a variety of forms and follow several patterns of development. This variety presents conceptual and explanatory problems much more representative of the range of collective behavior than does the typical example of the crowd. These organized social patterns require a social level of conceptualization. We want to suggest such a conception of collective behavior, but first the next section illustrates the neglect of such social characteristics by influential writers on collective behavior.

NEGLECT OF SOCIAL CHARACTERISTICS

For a long time collective behavior was regarded as psychologically abnormal. Even now, it is so regarded by many sociologists who are not specialists in the area (for example, see Broom and Selznick 1968, pp. 221-54). However, viewpoints that find collective behavior irrational, essentially emotional, or nonsocial have been almost totally rejected by collective behavior specialists. For example, Turner and Killian (1972), Turner (1964a), Smelser (1963), Skolnick (1969), Marx (1970), and especially Couch (1968) have pointed out various ways these conceptions neither adequately distinguish collective behavior from other behavior, nor capture its essential characteristics. Nevertheless, a more subtle but similar ten-

³ Our data are taken from the field research of the Disaster Research Center at Ohio State University, where more than 250 events, primarily natural disasters, have been studied in recent years. For summaries and detailed references to Center studies of relevance to collective behavior, see Dynes and Quarantelli (1973) and Quarantelli and Dynes (1970a).

dency remains. Many writers still locate the essence of collective behavior in the makeup of participants or their reactions to each other. Sociologists seem concerned with providing a psychological account of the nature of collective behavior.⁴

This can be seen in the writings of such distinguished contributors to the field as Blumer, the Langs, and Smelser. In many respects their works differ significantly. However, they do share and exemplify a tendency to view collective behavior in terms of individual participants. Although they refer to existing patterns of social organization in defining what collective behavior is not, and rely in varying degrees on social factors to explain collective behavior, their conceptions of the behavior itself are couched in social-psychological terms. In discussing the behavior, they attempt to depict what is happening to and within individual participants.

Consistent with Park from whom he drew his substantive focus, Herbert Blumer eschewed earlier pejorative views of collective behavior. Yet, in work spanning three decades, Blumer (1939, 1969a) uses Mead's interaction framework to describe the nature of collective behavior. He suggests that participants in the behavior respond to others by circular reaction rather than symbolic interpretation (1951, pp. 170-77). As Smelser notes (1963, p. 10; 1964, p. 117), Blumer locates the distinguishing characteristic of collective behavior in the interaction of participants. In fact, the distinguishing characteristic ultimately reduces to the individual's mode of response to others' gestures. Such a view invites us to conceptualize collective behavior as a characteristic of participants' responses to social cues and to search for its explanation in conditions that influence individuals to abandon symbolic interpretation. In other work in the area, Blumer (see especially 1957) does not so restrict his analysis. His classic discussions of publics (1948) and fashion (1969b), for example, are not totally restricted to this level of analysis. However, because the crowd is offered as a prototype, the implication is often taken that this conception applies to all collective behavior.

A rough parallel is found in Lang and Lang (1961, 1968). They suggest that participants in collective behavior act without guidance of social structure. In their terms this means that participants act without the benefit of patterned generalized expectations associated with social statuses. Thus, defining the nature of collective behavior, they focus in detail upon collective dynamics by which individuals acquire the social definitions usually supplied by traditional patterns. As Turner (1964b, p. 128) sug-

⁴ To see this as a problem does not mean that certain aspects of collective behavior cannot be profitably described in strictly social psychological terms (see Milgram and Toch 1969). Rather, we claim that a social psychological approach neither exhausts the possibilities nor is it the most fruitful vantage point for a more sociological analysis.

gests, the resulting theory uses a sophisticated contagion approach. The problem for analysis is to discover how individuals are caught up in collective episodes, rather than the forms collective behavior takes (Manning 1970, p. 150).

Neil Smelser (1963) presents the most consistently sociological analysis of the range of social conditions that must be examined to explain collective behavior episodes. But, even though his early work (1963) is an avowedly sociological theory of collective behavior, it provides a third major example of an individualistic conception of collective behavior. He views the behavior itself as based upon participants commonly holding a "generalized belief." Despite his sociological conception of many of the determinants of collective behavior, these conditions are ultimately reduced to the beliefs of the participants. That generalized beliefs are central to Smelser's conception of the nature of collective behavior can be seen in two aspects of his analysis. First, it is the major way he distinguishes between collective behavior and institutionalized behavior and, second, it is the only way he distinguishes subtypes of collective behavior. Smelser (1970, p. 48), of course, explicitly claims this conception is quite sociological. However, as Milgram and Toch note, "Smelser's theory employs as its central postulate the notion of generalized belief, implying a unitary outlook at the root of collective action" (1969, p. 561; see also Quarantelli and Hundley 1969 and Currie and Skolnick 1970, although the latter critique is inaccurate in several respects). Smelser's more recent work (1972) seems to move even more explicitly in a psychological direction.

Each of these major theorists and others that could be mentioned rely on psychological and social-psychological terms to discuss the nature of collective behavior. This tendency is partially a consequence of the idea that individuals "normally behave according to institutionalized patterns (for explicit statements on this, see Blumer 1957, p. 130; Lang and Lang 1961, p. 6; and Smelser 1964, p. 117). Because collective behavior is not routine, recurring activity for its participants, theorists have attempted to explain the mechanisms of individual behavior during collective behavior episodes. They ask how the individual is different in these situations as opposed to those in which he engages in institutionalized behavior. This concern with having an adequate model of the individual has absorbed much of the effort expended to develop conceptions of collective behavior. Most sociological treatments of collective behavior have consequently been sidetracked from proceeding to define their initial problem as one requiring conceptual elaboration at a social level.

The consequences of this concentration of theoretical attention can be illustrated by comparing treatments of individual social actors by institutional and collective behavior theories. In accounting for the social characteristics of institutional behavior, individual behavior is taken as

unproblematic. That is, for most purposes of studying institutional behavior, a simple model of the individual has been assumed. Distorted for brevity, this model holds that individuals come to act according to social norms through a variety of social mechanisms, for example, socialization, anticipation of sanctions, exchanges of valued commodities, and asymmetrical power relations. The benefits of this model are not in its "realism." It can easily be criticized as an inadequate representation of the reality of individual behavior (see, for example, the criticisms of Wrong 1961 and Blumer 1969c). However, the assumptions of this model of the individual allow us to suspend the consideration of individuals in favor of social patterns. The model does not solve the problem of individual behavior. It merely expedites a focus upon other interesting sociological problems, for example: how do patterns of action in different group contexts compare? What are the effects of the interplay between environments and the groups they encompass? How do the characteristics of groups change? The model permits an uncomplicated view of the relationship between individual and social behavior. It frees sociologists to concentrate on the social characteristics of the social action they study.

Unfortunately, approaches to collective behavior are not based on an equally uncomplicated model of the individual that also allows concentration on problems defined at a social level. Instead, development of a model of the individual engaging in behavior so unusual for him and so unsupported by the norms that guide his routine behavior is taken as the problem to be solved by a theory of collective behavior. Models as widely varying as Lang and Lang's (1961) and Smelser's (1963) share the fault of not emphatically dealing with the social characteristics of the collectivities engaging in collective behavior. Collective behavior is identified or conceptualized in terms that can most clearly be seen as properties of individuals, not collectivities. In many cases, the social actions of these collectivities are regarded as the aggregate responses of individuals converging along similar lines or spreading through contagion-like processes (see Klapp 1972). Thus, collective behavior is assumed to be described and explained adequately by accounting for how a single individual in a given situation could pursue the course of action observed of the collectivity as a whole (see Turner 1964a, pp. 384-97).

Turner's (1964a) development of the emergent norm perspective (from its inception in Turner and Killian 1957) responds to this problem. He provides an improved view of the social psychology of collective behavior by suggesting that it should be analyzed with the same model of individual and interactive behavior as institutionalized behavior. This is a far more satisfactory perspective on collective behavior participants than those posited by other theories. At the same time, however, the distinction between institutionalized and collective behavior is obliterated. The strength

of the emergent norm perspective is that it rules out looking at atypical characteristics of individuals and their reactions to each other to identify and explain collective behavior.⁵ But it does not then provide alternative criteria for conceptualizing the differences this behavior has from institutionalized behavior. While we are eager to accept the argument that there are no fundamental psychological differences between participants in institutionalized and collective behavior, this does not mean that there are no differences at all. There are obvious differences, and if they are not social psychological, then we are almost forced to look for them in the social properties of the collectivities that enact them. Couch makes a similar point when he observes that it is not enough to point to the lesser importance of cultural factors in crowd behavior. He feels that attention also needs to be directed to the crowd as a social system "with social processes and social relationships" (1968, p. 321). Elsewhere he adds, "relatively few studies have attended to structures of social units engaged in collective behavior" (1970, p. 457). It is to questions at this level that the social-psychological postulates of the emergent norm perspective lead us.

SOCIAL-ORGANIZATIONAL DIMENSIONS

Tendencies toward psychological conceptions of collective behavior maintain the gap between those theories and the rest of sociology, but this gap can be bridged by following the implications of the emergent norm perspective. We need to set about specifying the social-organizational nature of collective behavior collectivities. This manner of problem definition calls for the parsimonious isolation of generic differences between these collec-

⁵ This model of individual behavior following the guidelines of group norms avoids some of the explanatory questions answered by the convergence and contagion theories. Heterogeneity of individual behavior regulated by a single emergent norm is postulated but unexplained. Heterogeneity has to be accounted for by factors other than the norms—perhaps even by mechanisms similar to the discredited contagion and convergence theories—namely, differential exposure and susceptibility. But, the very strength of Turner's position is his neglect of these questions. It allows suspension of attention at an individual level in favor of posing questions in social terms. On the other hand, the convergence and contagion approaches provide explanations of collective behavior, but the emergent norm approach does not. It provides only a set of assumptions by which to view the behavior. In Turner's words, contagion and convergence theories "explain collective behavior . . . [while] emergent norm theories see collective behavior as regulated by a social norm which arises in a special situation" (Turner 1964a; emphasis added). As for explanation, the assumption of homogeneity of individual orientations is replaced by one of apparent unanimity of opinion. The collectivity's course of action is thus predicated on a single emergent norm—one that the observer finds consistent with the behavior. As for explanation, the content of the norm determines the concrete course of action. Differences in behavior can be attributed to different norms. Whatever concrete behavior is observed, we can imagine a normative prescription which it can be assumed to fulfill. We endorse the advances in conceptualization made possible by the emergent norm perspective, but it has yet to show equal strength in improving explanations of collective behavior.

tivities and those carrying out institutionalized behavior. While it is standard to contrast collective behavior with institutionalized behavior, almost always definitions of collective behavior are made in negative terms. That is, definitions of collective behavior tell us merely what it is not. This negative assertion is insufficient. A more positive identification of collective behavior's distinctive qualities depends upon specifying its social-organizational alternatives to the collectivities enacting institutionalized behavior. We take the approach of first asking what social dimensions of any collectivity provide a basis for or predicate the group's social action. Then we ask how the dynamic and ephemeral collectivities enacting collective behavior are different.

The foundations of the social action of any collectivity are social norms and social relationships. Sometimes the latter is seen as subordinate to the former. Too often, the social bases of social action are merely equated with a system of social norms. Blake and Davis persuasively state that at times it seems sociologists "adopt the extreme view by treating normative systems as the sole object of analysis or as the sole determinant of social phenomena" (1964, p. 461). However, many sociologists have noted that social relationships are conceptually distinct from norms (for especially effective discussions of this distinction, see Burns 1958 and Blau and Scott 1962, pp. 4-6). As a basis for behavior either may predominate. For example, the relationship between mother and child is a social basis of their behavior through a variety of changing normative contexts. Both social relationships and social norms are necessary elements of collectivities. The conceptual dimensions of social relationships and social norms both can be fruitfully imposed to organize the study of any acting collectivity. It is along these two dimensions that we can make a positive, generic identification of collective behavior that parallels standard sociological descriptions of institutionalized behavior.

Blumer has said that "social organization is a framework inside of which acting units develop their actions" (1969c, p. 87). Social organization not only "contains" social action, it also provides a foundation of social norms and relationships upon which action is built. Both institutionalized and collective behavior are contained within and predicated upon social organization. However, for institutionalized behavior, social organization bears a relatively permanent or traditional relationship to the social setting in which the behavior takes place, while in collective behavior the substructure of social organization is at least in part new to that setting. That is, the collectivities engaging in collective behavior are in some sense new. Collectivities make possible the interdependent and coordinated interaction of people. In collective behavior they are new in that some major aspect of this social foundation is created at the time the social behavior is enacted. Institutionalized behavior is based on enduring systems of social norms and

social relationships, but collective behavior upon similar systems in part created concurrently with their implementation as social action.

Furthermore, phenomena need not involve the emergence of both social norms and relationships to be usefully regarded as collective behavior. In fact, some examples of collective behavior appear not to involve the emergence of norms or relationships at all. We can see at least four types of collectivities, each of which differs from an ideal type conception of those enacting institutionalized behavior. Each of these four has an aspect of its social-organizational foundation for behavior created concurrently with the behavior it enacts, and each seems to fit the social-organizational nature of phenomena usually regarded as collective behavior.

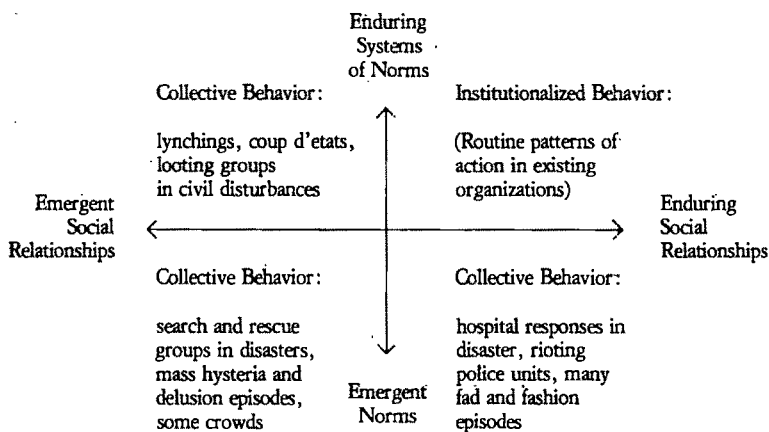


FIG. 1.—Institutional and collective behavior collectivities

TYPES OF NEW COLLECTIVITIES

Figure 1 represents three types of collectivities that contrast with those engaged in institutionalized behavior. These ideal types are obtained by dichotomizing and cross-classifying variations in the collectivity's source of social norms and social relationships as to whether they are enduring or emergent with respect to the social setting in which behavior takes place. (This dichotomization is illustrative. Actually both of these dimensions are quite likely continuous. For example, a range of mixtures between completely emergent and completely enduring social norms is likely.) The collectivities of institutionalized behavior have both enduring systems of norms and relationships. Three types of collectivities engaging in collective behavior are either entirely emergent or emergent on only one of these dimensions. A fourth type (not depicted in the figure) involves emergence in neither dimension, but occurs when previously dissociated systems of

norms and relationships are integrated with each other into a collectivity.

First, we want to stress that social organizational conceptions of collective behavior should not be confined to collectivities with both emergent norms and relationships. Existing systems of social relationships are often combined with emergent norms as a basis of social action that is not institutionalized and that can be fruitfully classified as collective behavior. For example, adjustments in a disaster situation may lead to the assumption of entirely new tasks by an ostensibly old, preexisting community agency—a formal group responding to the crisis with its preemergency identity, membership, and authority relationships, while nevertheless operating often with a substantially different set of norms. Hospitals in the immediate aftermath of a sudden major disaster impact frequently exhibit this pattern (Quarantelli 1970*b*). The same situation can be seen when changes in song fads or dress fashion are examined. Well-established formal social entities such as fashion houses, large circulation magazines, department store chains (Lang and Lang 1961, pp. 465–88), music companies, radio stations, and music stores (Hirsch 1970) maintain their names, personnel, internal leadership roles, and communication channels at the same time that new normative patterns emerge. Perhaps even more important, the innovations are typically taken over and then discarded by group members in relatively fixed peer relationships to one another (see, for example, Johnstone and Katz 1957).

In both examples, new norms emerge while social relationships endure. Any strict limitations of the scope of collective behavior to totally new collectivities with both new systems of norms and social relationships misses such cases that cannot be treated by an institutionalized model of behavior. Social organization involves two major dimensions, and emergence in any one of the dimensions means that the collectivity is enacting collective behavior.

This view may seem to stretch the bounds of phenomena classified as collective behavior beyond previous definitions. If this is so, it merely responds to a need clearly demonstrated by those with dynamic conceptions of social organization. Thus, Strauss and his co-workers (1963) have for some time been pointing out that normative guides to behavior are constantly emerging within established groups. They particularly illustrate this in a study (Bucher and Strauss 1961) in which developing segments in medical professions are analyzed as having the character of social movements within institutionalized arrangements. The emergence of a partial new normative pattern in a public works department during a flood threat illustrates the same point (Brouillette and Quarantelli 1971).

On the other hand, collective behavior cannot be defined solely in terms of the emergence of norms. In fact, to do so would only duplicate the

emergent norm formulations already developed by Turner (1964a).⁶ Social relationships are an integral and necessary aspect of collectivities, and they too emerge in many cases of social action that can be adequately captured neither by an institutionalized model of behavior nor by the emergent norm model of collective behavior. In the agency response to disaster and fashion examples noted above, it is social relationships that endure, while norms emerge. But the converse can also be the case.

Norms guiding collective behavior may be rather firmly institutionalized. For example, the recent urban disorders in the United States have shown consistency in some of their behavior patterns. Looting, for example, appears to be normative (Quarantelli and Dynes 1970b). In such cases, the norms guiding the behavior can be regarded as institutionalized. The occurrence of collective behavior depends on the emergence of a system of social relationships before the norms can be enacted rather than on the emergence of new norms. The problem of distinguishing collective and institutionalized behavior is more difficult in some other cases. For example, transfer of political power in some countries follows an institutionalized pattern of military overthrow of governments. Huntington in fact observes that reform coups d'état should be viewed as mechanisms of change, "the non-constitutional equivalent of periodic changes in party control through the electoral process" (1962, p. 40). This could be regarded as dissimilar to collective behavior. Nevertheless, even though the norm is institutionalized, the set of social relationships among the participants may be emergent. A clearer example of this type of collective behavior is lynchings. In the United States they have followed the same normative pattern time after time. In the social settings of the early American West and South, the norms of lynching were institutionalized, and the question as to whether the social action would occur depended on the emergence of a system of social relationships, including a division of labor in the lynch mob itself (Raper 1933). This seems to be particularly true for Western vigilante groups (Brown 1969, pp. 154-225).

Of course, there are situations where there are both emergent norms and relationships. For research, there are the most easily identifiable because in a sense they are the purest forms of collective behavior. Collectivities engaged in crowd behavior, for instance, clearly involve in many cases, al-

⁶ Turner and Killian's second edition of their text (1972) appeared while this article was being edited for publication and a year after the initial draft had been completed. They, too, far more explicitly than they ever did in the first edition (1957), also state that "two of the major sources of" stability in behavior are "the normative order and social structure" (1972, p. 59). The former is equated with values, goals, and norms; the latter with a division of labor or a "structure of interdependent roles" (1972, p. 59). Nevertheless, even in the second edition (1972), there is a tendency to single out emergent norms as the most important of the two features (for example, pp. 5, 22, 29, 32, 198, and 259) with the concept of social structure seldom being explicitly used after its introduction.

though not all, the emergence of new social relationships as well as norms. This is why there is much less confusion in the literature as to the placement of crowds in collective behavior taxonomies compared with the location of fashion (see, for example, Turner and Killian 1972, pp. 152-54) or publics (see Smelser 1963, p. 172) where only one social organizational dimension may often be involved. We, however, are not equating crowds with collectivities having both emergent norms and relationships, since, for example, an established audience assembled for traditional purposes may maintain its prior set of relationships as its normative behavior develops new forms. (A classic case is described in Douglas [1939], where a funeral procession became a very expressive social collectivity.) Established social groups maintaining some of their old relationships as their members develop mob actions can also be seen in some police behavior in recent urban disturbances. Marx, for instance, notes that in some cases the same application of a collective behavior perspective to the rioters "might equally be applied to the police" (1970, pp. 48-50). Grimshaw (1963) documents the same for earlier historic periods.

A liberal interpretation of the scope of collective behavior can thus include at least three types of collectivities that are alternatives to established groups. New collectivities can exhibit emergent norms, or emergent social relationships, or both. To single out only emergence on one dimension, as implied by the emergent norm formulation, leaves out two other possibilities for the organization of new collectivities. Similarly, to characterize, as Swanson (1971, pp. 75-110) does, crowds and social movements as "transitional organizations"—while perhaps an improvement over the term "collectivity"—seems to exclude emergent phenomena in enduring groups that seem best conceptualized as collective behavior.

A fourth type of collectivity does not seem well conceived of as institutionalized behavior, nor does it seem to involve emergence, but it also might be fruitfully conceived of as collective behavior. So far we have looked at collectivities that create some aspect of their social organization. In other cases, it seems borrowing is more salient than creation. There are cases where it is very difficult to understand the behavior as institutionalized for the collectivity engaging in it, even though it is quite traditional for other groups. For example, when an existing peer group becomes a cult by copying norms of witches' behavior (Quarantelli and Wenger 1973), or a long-established teachers' professional association becomes a militant social movement by adopting the norms of collective bargaining, we have no emergence, but instead the confluence of previously dissociated norms and relationships. What appears to have been created in this type of collectivity is the integration of these dissociated dimensions of social organization.

We feel that this typology and the arguments on which it is based can

be useful for examining several problems. It is derived in a systematic fashion and works toward describing collective behavior in terms that are comparable with those of a social-organizational perspective on institutionalized behavior. In addition to this contribution, the formulation has both theoretical and research implications for the specialty of collective behavior.

First, the typology serves as a definition of collective behavior. In its terms, collective behavior is social action engaged in by one of four types of collectivities. The identification, furthermore, is a positive one. The types and collective behavior as a whole are defined in terms of characteristics they do possess, rather than by the absence of certain qualities or features. This positive approach to definition may also be helpful in identifying empirical instances of collective behavior, especially more marginal ones such as emergent gangs (Yablonski 1959; Pfautz 1961), "conventional" assemblages (Concepcion 1962), and nonviolent accommodation groups (Quarantelli 1970a; Marx and Archer 1971). This may overcome what appears to be a common tendency to label research cases as collective behavior because of their surface similarity to past cases reported in the literature, rather than because they exhibit required characteristics. Also, the use of positive characteristics to identify collective behavior improves its conceptual separation from institutionalized behavior.

Second, the typology avoids collapsing of dimensions that delineate types with factors that are used to explain the behavior or record its consequences. For example, Smelser (1963) uses his five types of generalized beliefs both to delineate a typology and as a major explanatory factor in his value-added scheme. Also, it is parsimonious in the number of dimensions it employs. In contrast, as Smelser notes (1963, p. 7), Blumer uses at least four disparate criteria to distinguish four elementary collective behavior groupings.

Third, our arguments undermine the place of the crowd as a prototype of collective behavior (for example, as is typified in the uncritical view of Fisher [1972, p. 187]). Collectivities traditionally classified as crowds fall into at least the first three types of our typology. Seen from this vantage point, the crowd has no special place as a preeminent example of collective behavior. Instead, we should recognize that the processes and problems of forging out new social relationships might be quite different from those in the emergence of norms. Furthermore, how previously dissociated norms became part of the social organization of a group differs significantly from the processes by which norms and social relationships emerge together (for examples, see Forrest 1968). Instead of using the crowd as a prototype of all collective behavior, a view that emphasizes the emergence or borrowing of both norms and relationships should be more useful.

Finally, while the major purpose of this paper is to account neither for

conditions nor consequences of collective behavior, but rather to delineate its characteristics, the typology suggested could contribute to these other two aspects. For example, including emergent social relationships as a quality of some collectivities indicates that collective behavior may occur in situations other than where traditional norms are absent or inoperative. Where norms are clear but the social relationships necessary to implement them are lacking, collective behavior may occur to create them. An implication here is that institutionalized social relationships are not always one end product of the crystallization of norms. The reverse is true in some cases: established relationships may help crystallize norms. This is consistent with observations such as by Turner (1964*b*, p. 132), Couch (1968, p. 320), Quarantelli and Hundley (1969, p. 547), and others about crowd development. They note that it is not the supposed anonymity of participants that is crucial in crowd formation, but that crowd norms emerge from the nuclear cores provided by preexisting social relationships among members. A similar point is made by Freeman (1973) in her analysis of the more successful emergent groups involved in the women's liberation movement.

At a more fundamental level, since the typologies allow for partial institutionalized patterns intermixed with emergent patterns, there is an encouragement of the convergence of the two perspectives involved. The earlier cited quotation from Turner is but a more explicit and forceful statement of a view increasingly being expressed. Thus, Zald and Ash (1966, p. 328) state in their discussion of social movements that "the sociological approach to organization utilized . . . helps to bridge the gap between organizational analysis and collective behavior." Merton, in his preface, favorably observes that Barton's (1969, p. xxii) book on community disasters attempts to make a connection between collective behavior and the behavior of organizations. Other recent statements (Dynes and Quarantelli 1968; Denzin 1968; Lammers 1969; Landsberger 1969, pp. 1-62; McPhail 1969, p. 443; Oberschall 1973) on the historical gap between the sociology of organizations and collective behavior consistently note the handicap it creates for theory and research in both areas.

CONCLUSION

We have stressed the need to identify collective behavior in terms of the presence of certain social organizational characteristics and not just by the absence of particular features. As Berk (1972) has noted of studies of crowds, attention to conditions for and consequences of a phenomenon sometimes leads to ignoring the characteristics of the behavior itself. In much collective behavior writing, the phenomenon is not characterized at the same social level as the conditions for and consequences of the be-

havior. Our effort has been not only to call attention to this fact, but also to indicate the need for a positive identification of the phenomenon.

We have also emphasized that in everyday life the framework of collective action is provided by a series of social relationships as well as cultural norms. In looking at the collectivities that emerge in collective behavior situations, therefore, both major dimensions of social organization must be taken into account. In recent years, attention has been given to the emergence of norms, but we suggested that this captures only part of what might be involved in collective behavior. In a striking, although isolated, example, Turner and Killian at one point talk of "a rumor group" and that "the rumor process creates an organization, with at least the beginnings of differentiation into roles" (1972, p. 46). Our more extended attempt has been to develop systematically the notion that social relationships are an integral part of social behavior and that they can emerge in collective behavior either in conjunction with or independent of any new norms.

Our conceptionalization of collective behavior avoids the use of logically unrelated criteria drawn from everyday language. For example, labels like crowd, mob, riot, crazes, booms, publics, revolutions, and so on are seldom logically derived from a generic conception of the behavior involved. We agree with Smelser, whose work is also an exception, when he notes that while "the demarcation of lines is not an end in itself," nevertheless classification "is of prime importance" (1963, p. 5). Accordingly, we have suggested a theoretically derived principle for identifying and differentiating types of collectivities. Not until we have the conceptual tools to adequately capture and separate what we are trying to explain will all the search for the conditions explaining collective behavior bear fruit.

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Measures of Organizational Structure: A Methodological Note¹

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The application of multiple instruments for measuring structural characteristics of complex organizations is recommended in order to determine their convergent and discriminant validity. This exploratory study makes a comparison between two different sets of measures, designed to assess the degree of centralization and formalization. One set represents the institutional approach, which relies on documents and informants; the other set relies on the survey approach, which is characterized by the use of questionnaires and interview schedules. The simultaneous application of the two sets of instruments to a small sample of manufacturing organizations suggests a certain amount of convergence, but raises serious doubts about the validity of some of the indicators. The differences in scope of the empirical referent of the indicators appear to be a major source for the lack of convergent validity.

The present study is concerned with the assessment of measures of organizational structure, in order to determine their convergent and discriminant validity. It is designed to establish the degree of association between different indicators which have been constructed by different authors to tap conceptually identical attributes of formal organizations. More specifically, a preliminary attempt will be made to contrast some operationalizations of organizational characteristics, which have been derived from the Weberian construct of bureaucracy.

Weber's (1947) conceptualization of bureaucracy has generated extensive discussions and has stimulated many authors to test empirically the degree of association among structural attributes. The leading reports are those of Hage and Aiken (1967), Udy (1959), Pugh et al. (1968), Hall (1962, 1963), Hall and Tittle (1966), Blau, Heydebrand, and Stauffer (1966), Lawrence and Lorsch (1967), Brewer (1971), Perrow (1970), and Blau and Schoenherr (1971). In general, these attempts to study bureaucracy either as a total phenomenon or by analyzing its constitutive dimensions have yielded a rather healthy empirical basis for developing a systematic theory of bureaucracy. Apart from the trend to test theoretical propositions

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concerning organizational characteristics, there is also a growing concern for the methodological implications of organizational research (Price 1968, 1972; Blau 1965; Hage 1970; Barton 1969).

Although some conceptual consensus is developing among organization researchers, they have generated different and perhaps divergent indicators to tap the same structural attributes. Following Lazarsfeld and Menzel's (1969) classification of global, analytical, and structural properties, these studies may roughly be categorized as focusing either on "global" or "analytical" properties of formal organizations. They differ in terms of whether they rely on direct measures (i.e., global assessment from records or institutional spokesmen) or whether they are based on aggregation of interview and questionnaire data from members. Most organizational studies employing "institutional" measures of organizations make use of official informants and documents (Pugh et al. 1968; Blau et al. 1966; Blau and Schoenherr 1971), and studies which focus on analytical properties employ mainly the survey technique (Hage and Aiken 1967; Hall 1962, 1963; Perrow 1970). Sometimes the first approach has been labeled objective and the second one, subjective (Price 1970), although one can argue that these terms should be reversed.

Obtaining information from documents reflecting the arrangement of positions such as the organizational chart, personnel department files, and job descriptions, or by interviewing some "expert" such as the top manager, may be labeled "objective." If the organizational charts of different organizations reveal different numbers of authority levels, then different degrees of centralization may be inferred. Similarly, managers may describe the range of automation of equipment, such as in the Pugh et al. (1968) study, or indicate the division of labor of their organization by listing the number of full-time specialists, and possibly provide written evidence in the form of organizational handbooks. The institutional approach has the advantage of generating information about organization structure which is rather unambiguous. Such information reveals aspects about organizations which is not contaminated by information about individuals, or colored by those who are being interviewed. If, however, such information is called objective, this adjective should not be equated with (more) reliable or valid. Indeed, the reliance on documents which tend to be obsolete, and the use of one or a few informants who may be vulnerable to error—such as in the Pugh et al. (1968) study—should preclude any arbitrary preference. In contrast, the survey approach has sometimes been criticized for generating "subjective" information reflecting the biases in shared attitudes and role idiosyncracies of the members of the organization (Hinings and Lee 1971). Also, the discussion on ecological correlations has suggested that aggregates of individuals' responses to items do not always stand for organizational characteristics and that therefore the survey technique may be unable to

grasp some group properties (Slatin 1969; Davis, Spaeth, and Huson 1961; Tannenbaum and Bachman 1964).

In view of these and other distinct qualities of two major approaches to the study of bureaucracy, it is of crucial importance to investigate the degree of commonality among various kinds of measurement procedures which have been designed to tap similar phenomena. Agreement on the naming of variables does not necessarily imply conceptual and/or operational agreement, and the inconclusive character of some of the results may be a function of the methodologically limited nature of the conclusions at which different authors arrive. For example, studies using institutional methods (Blau 1970; Pugh et al. 1968) found a negative correlation between concentration of authority and standardization of organizational activities, while Hage and Aiken (1967) and Hall (1963), using survey aggregation methods, found a weak positive and a strong positive correlation, respectively, between these two dimensions. Hall (1963) claims that they both represent the degree of bureaucratization, whereas Blau (1970) interprets his discrepant result by arguing that instead of being complementary, centralization and standardization are alternative methods for coordination and control. The contradictory or paradoxical character of such assertions may be solved by applying the two kinds of measurement instruments without any appreciable time lag to the same sample of organizations.

METHOD

The two sets of indicators that have been subjected to the present validation attempt have been derived from the concepts of centralization and formalization. As a representative study of the institutional approach it takes the operations of Pugh et al. (1968), an abbreviated version of which is provided by Inkson, Pugh, and Hickson (1970). These authors rely on data that are obtained by interviewing one or a few top executives and from documents which the organization made available to the researchers. The alternative approach, as exemplified by Hage and Aiken (1967) and Perrow (1970), provides a second set of indicators. The following chart gives a brief description of the two sets of measurement instruments; the actual operationalizations of the variables involved are described in detail in Inkson et al. (1970), Hage and Aiken (1967), and Perrow (1970).

Institutional approach

A₁. Centralization

Autonomy.—This scale consists of 23 issues to measure whether decisions on these issues are made inside or outside the organization (Pugh et al. 1968, pp. 102-4).

Chief executive span of control.—This indicates the number of subordinates who report directly to the chief executive, regardless of

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the hierarchical position of the subordinates (Pugh et al. 1968, p. 104).

Worker/supervisory ratio.—This value indicates the number of subordinates in production departments per first-line supervisor (Pugh et al. 1968, p. 104).

Number of direct supervisors (%).—This indicates the number of first-line supervisors in production departments, including the assistants and deputies (Pugh et al. 1968, p. 104).

B₁. Structuring of activities (formalization)

Formalization: role definition.—This scale measures the degree to which rules and procedures are written (Pugh et al. 1968, pp. 100–101).

Specialization.—This scale measures the extent to which one or more individuals occupy a non-work-flow function on a full-time basis, regardless of the number of specialists (Pugh et al. 1968, p. 92).

Questionnaire approach

A₂. Centralization

Personal participation in decision making.—This is a Likert scale measuring how much the individual participates in decisions about the allocation of resources and the determination of organizational policies (Hage and Aiken 1967, p. 78).

Hierarchy of authority.—This scale measures the degree to which the organization member participates in decisions involving the tasks associated with his position (Hage and Aiken 1967, pp. 78–79).

Departmental participation in decision making.—This Likert scale measures how much an individual “and his colleagues” participate in decisions involving their work and work environment (personal communication).

B₂. Formalization

Job codification.—This is a scale measuring the degree of work standardization, that is, how many rules define what organizational members are to do (Hage and Aiken 1967, p. 79).

Job specificity.—This scale measures the degree to which procedures defining a job are spelled out (Aiken and Hage 1968, p. 926).

Strictness.—This scale measures a dimension related to rule observation: the degree to which existing rules are enforced (Perrow 1970, p. 17).

Rule observation.—This is a measure of whether rules are employed and enforced (Hage and Aiken 1967, p. 79).

Written communication.—This is a rating scale measuring the frequency of written communication as reported by individuals (personal communication).

The systematic comparison of the two sets of measurement techniques as listed in the above chart has been carried out with the “multitrait-multimethod” model as described by Campbell and Fiske (1959) and Althausen and Heberlein (1970). The multitrait-multimethod procedure is based upon the convergent and discriminant validation of more than one characteristic by more than one measure. Convergent validity is established

by the magnitude of correlation coefficients between pairs of measures purporting to represent the same characteristic relative to the magnitude of the correlations between measures of other pairs of characteristics using the same two methods of measurement. Further, if measures of different characteristics obtained by the same method are uncorrelated, the lack of such correlation is said to imply discriminant validity, provided convergent validity is also present. With the multitrait-multimethod procedure a correlation matrix is constructed, and each entry in this matrix is defined by the coincidence of one of the two or more trait measurements with one of the two or more methods.

Since organizational characteristics can be measured by either the institutional approach or the survey approach, it is expected that the two sets of measures will highly intercorrelate when tapping the same latent trait. Discriminant validity may be determined by assessing the magnitude of correlation coefficients between measures which stand for different characteristics. As mentioned before, however, each of these approaches may have systematic but unknown biases so that it is possible that two "different" attributes measured by a common method may be more alike than the "same" attribute measured in two different ways. Thus two questionnaire indices of professionalization may be likely to correlate higher with each other than with an institutional measure also reflecting professionalization. Under ideal conditions, however, one finds that the common variance of different indicators tapping a latent trait is higher than the common variance among indicators within one approach measuring different traits (Althauser and Heberlein 1970).

Data Collection

The data upon which this exploratory study is based were collected in 10 organizations in the Toronto metropolitan area during the first half of 1970. The organizations varied in size from 175 to 1,200 employees and included nutritional, chemical, electronic, and paper-producing industries. The selection was from 23 organizations which were investigated by McMillan et al. (1970) and have been matched with a British sample to test cross-national variations of structural properties of organizations.

In order to distribute questionnaires to a sample of employees, all these companies were contacted; four refused to cooperate, four preferred to postpone their collaboration, and five failed to reply. Table 1 summarizes the major features of the 10 companies which constituted the sample. Questionnaires were distributed among a systematic random sample of supervisory and nonsupervisory personnel among departments within each company. Some departments and some categories of personnel were over-represented in order to insure sufficient data in small departments. The

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TABLE 1

SAMPLE DESCRIPTION

No.	Main Product Line	Status of Unit	Size (No. of Em- ployees)
1	Wire, sheet metals, nails	Branch	250
2	Copper sheets	Head branch	1,200
3	Corrugated boxes	Head branch	220
4	Motor accessories	Head branch	320
5	Metal finished	Subsidiary (with legal identity)	175
6	Alloys	Subsidiary (with legal identity)	300
7	Gears	Subsidiary (with legal identity)	250
8	Office forms	Head branch	360
9	Biscuits	Subsidiary (with legal identity)	400
10	Stoves, refrigerators, and other appliances	Subsidiary (with legal identity)	660

overall response rate was close to 50%. Given the finding that supervisory personnel tend to perceive their organization as less centralized or formalized than do their subordinates, a proportional representation across the different companies had to be maintained.

Since the proportions of refusals for supervisory and nonsupervisory personnel were rather similar, sampling bias due to nonresponse across companies is negligible. Because the subject matter of the scales dealt with the perception of structural properties of organizations rather than with attitudes, opinions, or feelings, low response rates probably affect possible distortions to a much smaller extent than do refusal rates in research on attitudes and opinions, where systematic biases may be strongly associated with the very attitudes that are measured.

A total of 350 questionnaires were returned—19 of the smallest sample and 70 of the largest sample, the size of the sample being proportional to the size of the organization. According to Tannenbaum (1969, pp. 23–25), such sample sizes, if representative, are large enough to provide reasonably stable measures of organizational characteristics.

The information which was gathered from these questionnaires was aggregated without assigning weights to employees occupying different positions in the hierarchy. The differences in mean scores on each measure of organizational structure were extensive enough to justify the assertion that each organization had distinct properties of its own. The one-way analysis of variance on each indicator as well as multivariate analysis of variance on clusters of scales yielded very high *F*-ratios. The intraorganizational and interorganizational variations were considerable, thus sug-

gesting that both organizations and departments within each organization have significantly different degrees of bureaucratization as measured by questionnaire data. The differences between departments, however, were much more significant than the differences between organizations. The departmental data revealed a smaller dispersion of scores than data from organizations because departments tend to have a more homogeneous arrangement of social positions than do organizations, and because organizational-level variance includes both that from individual and that from departmental sources.

An alternative indication of questionnaire scales discriminating organizations was established by computing the rank-order correlations between the mean scores based on supervisor and on subordinate categories of each of the 10 organizations. All correlations were significant at the 5% level, so that even though supervisory personnel tend to perceive their organization as less bureaucratic than do their subordinates, both categories of personnel within the organization tend to perceive deviations in the same and consistent direction from the population mean. If, for example, an organization is relatively participative or decentralized, both supervisory and nonsupervisory personnel will reveal higher scores on participation than do supervisory and nonsupervisory personnel in an organization that is less participative, although the supervisory personnel of the nonparticipative organization may perceive its organization as being more participative than the nonsupervisory personnel of the participative organization.

On the basis of these findings the aggregated scores of organizations and departments were treated as reflecting properties of the described organization and not merely idiosyncratic responses. The properties as assessed by the alternative technique, the institutional approach of Pugh and his colleagues (1968), were not based on members but on information supplied by a few institutional spokesmen, and therefore the aggregation problem was not accessible for study. Usually one or two chief executives and a personnel manager were interviewed, and, whenever possible, documents sustaining their verbal reports were obtained. As the data obtained with questionnaires appeared to measure collective properties, the paired measures can legitimately be compared with each other.

RESULTS

Table 2 shows the correlation coefficients between the two clusters of measures of organizational structure. Seven measures of centralization and the same number of measures of formalization were applied to the described sample of organizations. It was expected that all measures of centralization, regardless of whether they were obtained from the institutional or survey approach, would show relatively high correlations, and the same was

TABLE 2

MULTIMETHOD-MULTITRAIT MATRIX OF MEASURES OF ORGANIZATIONAL STRUCTURE: PRODUCT-MOMENT CORRELATIONS BETWEEN INSTITUTIONAL AND QUESTIONNAIRE MEASURES OF CENTRALIZATION AND FORMALIZATION ($N = 10$)

	INSTITUTIONAL APPROACH							QUESTIONNAIRE APPROACH						
	A ₁₁	A ₁₂	A ₁₃	A ₁₄	B ₁₁	B ₁₂		A ₂₁	A ₂₂	A ₂₃	B ₂₁	B ₂₂	B ₂₃	B ₂₄ B ₂₅
Institutional approach:														
Centralization:														
Autonomy (A ₁₁)
Chief executive's span of control (A ₁₂)	.58
Worker/supervisory ratio (A ₁₃)	.16	.25
No. of direct supervisors (A ₁₄)	-.40	-.52	-.57
Formalization:														
Specialization (B ₁₁)	.30	-.17	.16	.24
Role definition (B ₁₂)	-.59	-.67	.10	.26	.24
Questionnaire approach:														
Centralization:														
Personal participation in decision making (A ₂₁)	-.73	-.53	-.25	-.10	-.53	.45		.96
Hierarchy of authority (A ₂₂)	-.39	-.41	.05	.21	-.24	.50		.59	.72
Departmental participation in decision making (A ₂₃)	-.41	-.52	.09	.14	-.15	.55		.71	.93	.83
Formalization:														
Job codification (B ₂₁)	-.05	-.09	.49	-.40	-.43	-.05		.45	.50	.61	.78
Job specificity (B ₂₂)	-.02	-.15	-.29	-.16	-.71	-.05		.53	.43	.40	.47	.75
Strictness (B ₂₃)	-.04	.26	-.09	-.63	-.60	-.25		.29	.09	.04	.25	.50	.64	...
Rule observation (B ₂₄)	-.02	.21	.14	-.44	-.82	-.48		.30	.11	.10	.72	.59	.56	.61
Written communication (B ₂₅)	-.57	-.42	.18	.39	.07	.25		.41	.43	.52	.38	.32	.29	.04

NOTE.—A coefficient of .57 is significant on the 5% level. A coefficient of .70 is significant on the 1% level. The values in the diagonal are Spearman-Brown reliability coefficients.

expected of the measures of formalization. Since the two sets of measures involved two dimensions, which, according to some studies (Pugh et al. 1968; Blau et al. 1966; Hage and Aiken 1967), may vary independently of each other, it does not necessarily follow that measures of the two different dimensions would show high correlations. Indeed, if they can be reduced to one overall dimension the conclusion is warranted that the analysis has demonstrated the absence of discriminant validity, thus corroborating the syndromatic nature of the concept of bureaucracy.

The proximity matrix of table 2 displays a typical multitrait-multimethod matrix.² As indicators of proximity, these product-moment correlations reveal the relative degree of association between the institutional and questionnaire measures of the *total organization*. As will be clarified below, the specific nature of the operationalization of some of the variables by Pugh et al. (1968) is heavily dependent on the nature of production departments. Therefore, table 3 has been added to table 2 to show the product-moment correlations between their measures of centralization and formalization and the aggregated scores of the subsample of employees from each organization's *production department*. Such a sampling manipulation rests on the assumption that the scores derived from questionnaire responses may be used as a "baseline" for detecting convergence between questionnaire and institutional measures, if these latter measures reflect aspects of production departments rather than aspects of the total organization. Therefore, a certain indicator may validly measure the dimension it is supposed to measure, but it measures such a dimension of production departments, although it is intended to measure the very same dimension of the total organization. It is evident that most of the coefficients in table 3 in general are higher than the corresponding ones in table 2, suggesting that the institutional measures have an "institutional" bias toward attributing characteristics of the production departments to the whole of the organization. Correlations between the institutional measures and the aggregate scores of other than production departments with some exceptions tend to be lower.

² The matrix includes four types of correlation coefficients, which, according to Campbell and Fiske (1959), may be classified as:

- a) monomethod-monotrait (for example, $r_{A_{11}A_{12}}$);
- b) monomethod-heterotrait (for example, $r_{A_{11}B_{11}}$);
- c) heteromethod-monotrait (for example, $r_{A_{11}A_{21}}$);
- d) heteromethod-heterotrait (for example, $r_{A_{11}B_{21}}$).

The third kind of coefficients are most crucial in that they form the basis on which one evaluates the degree of validity of the measures. Relatively high coefficients of the second kind (monomethod-heterotrait) indicate the existence of specific method bias, and relatively high coefficients of the last kind (heteromethod-heterotrait) reveal general method bias (Althausen and Heberlein 1970).

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TABLE 3

MULTIMETHOD-MULTITRAIT MATRIX OF MEASURES OF ORGANIZATIONAL STRUCTURE,
WITH AGGREGATED QUESTIONNAIRE SCORES BASED ON THE SUBSAMPLE OF
EMPLOYEES FROM EACH ORGANIZATION'S PRODUCTION DEPARTMENT

QUESTIONNAIRE APPROACH	INSTITUTIONAL APPROACH					
	A ₁₁	A ₁₂	A ₁₃	A ₁₄	B ₁₁	B ₁₂
Centralization:						
Personal participation in decision making (A ₂₁)	-.82	-.55	-.26	-.14	-.28	.63
Hierarchy of authority (A ₂₂)	-.58	-.46	.45	.04	.14	.82
Departmental participation in decision making (A ₂₃)	-.59	-.54	.26	.14	.20	.89
Formalization:						
Job codification (B ₂₁)	-.36	-.51	.46	-.27	.11	.67
Job specificity (B ₂₂)	-.42	-.54	-.63	-.63	.23	.29
Strictness (B ₂₃)	-.22	.05	-.21	.08	-.20	.37
Rule observation (B ₂₄)	-.28	.16	.08	.14	-.15	-.10
Written communication (B ₂₅)	-.74	-.69	.00	.58	.44	-.06

NOTE.—A coefficient of .57 is significant on the 5% level. A coefficient of .70 is significant on the 1% level.

Centralization

A striking result is the negative and highly significant correlation between autonomy and personal participation in decision making, and the negative correlations between autonomy and hierarchy of authority and between autonomy and departmental participation in decision making. Personal participation in decision making is scaled in five categories from low to high; the other two measures have a similar pattern of response categories. Autonomy was measured by counting the number of issues or recurrent decisions, out of a list of 23 issues covering a wide range of activities in which the organization has the formal authority to make decisions, that is, the number of decisions which are taken inside the organization. It was expected that autonomy would be positively related to the degree to which members perceived themselves as participating in the decision-making process. However, the findings were in the unexpected direction: organizations which are highly autonomous vis à vis this environment tend to have a nonparticipative internal decision structure. Ad hoc interpretations of such an unexpected result must be treated with great caution. With only 10 organizations, product-moment correlations are very sensitive to even small variations of the actual scores, and statistical type-A errors are likely to occur.

Apparently, autonomy does not measure the variable centralization, but a dimension which is exactly the opposite. It could be speculated that domain and scope of centralization are two different aspects of the power

distribution which may be diametrical opposites, as Bates (1970) has suggested. It seems that the two measures refer to two different conceptual constructs, "autonomy of members" not being the same as "autonomy of organization." Such outcomes suggest at least that decentralization in terms of the organization's discretion in a number of decision areas does not coincide with decentralization in terms of participation in decision making and that one should not treat these aspects indiscriminately as some authors have done (Blau 1970; Chandler 1966). Apparently, other factors such as technology (Perrow 1970), environmental uncertainty (Lawrence and Lorsch 1967), or professionalization (Blau et al. 1966; Thompson 1961) may have a much greater impact on participation than does the degree of autonomy, which may only be a technique for designing a decision structure.

The top executive's span of control, as measured by the number of subordinates reporting directly to him, is strongly related to autonomy ($r = .58$), so that the greater the autonomy the larger his span of control. It seems as if a high amount of autonomy of an organization relative to its parent organization is associated with a heavy workload for the top executive, which in turn requires a large number of subordinates. Autonomy, in addition to a large span of control, seems to point to a strong concentration of power within the uppermost level of the hierarchy.

The three questionnaire measures of decentralization are negatively related to chief executive's span of control, although the product-moment correlations are hardly significant. Again the conclusion suggests itself that width of the executive control span reflects the decision structure of the top of the hierarchy, but its relationship, if any, with the degree of participation among lower echelons is negative.

Worker/supervisory ratio and number of direct supervisors indicate the extent to which the organizational authority structure is differentiated. They can be considered as measuring centralization in an indirect way, because of the underlying assumption that such hierarchical role differentiation is associated with vertical, especially downward communication. A high number of direct supervisors or a low number of subordinates per supervisor points to a narrow control span which may facilitate participation in decision making or reinforces the centralization of authority. Therefore, a strong correlation between these two measures and the questionnaire measure of centralization was expected, but, as the data of table 2 indicate, they are rather uncorrelated. Perhaps a somewhat different indicator of height of the organizational pyramid, such as the one developed by Whisler et al. (1969), which more adequately characterizes the total configuration of authority roles of an organization ("average" span of control) is more likely to be associated with questionnaire measures of centralization. Probably even more important is the finding by some authors that the relation-

ship between these measures is contingent upon third variables (Blau et al. 1966; Whisler et al. 1969; Brewer 1971). They found that the joint consideration of other variables such as subordinates' expertness or the degree of task complexity appears to substantiate in a more refined way under what conditions height of the organizational pyramid is associated with centralization. As reported elsewhere (Pennings 1973), it can be assumed that the level of professionalization or expertness of subordinates in these production departments is relatively low so that the failure to detect any relationship between the different measures may be consistent with the findings of Whisler et al. (1969), who demonstrated that only in highly nonprogrammed departments or organizations does such a relationship appear to be significant. Despite such speculations, these comparisons yield a rather embarrassing conclusion. One should realize that in the current literature all these indicators of centralization are considered as sampling aspects of the same property space. It appears, however, that they cannot be viewed as tapping the same latent trait. Perhaps the most tentative conclusion which could be drawn is that centralization is a multifaceted concept, the dimensions of which have a substantial amount of unrelatedness. There is no satisfactory theoretical explanation for this finding. It is clear, though, that these indicators should not be treated interchangeably, or by those who intend to use them in future research, or by those who quote research results obtained with these instruments.

Formalization

Besides concentration of authority indicators, tables 2 and 3 also incorporate different indicators of formalization. According to Weber (1947), this dimension is complementary to centralization in addition to facilitating the administration of the organizations. Again the matrices show the product-moment correlations between the two institutional measures and five questionnaire measures of formalization both on the organizational level and the production level, respectively. All the measures are to be considered as indicating the degree of formalization or the degree of explicit institutionalization of organizational activities. It is evident that specialization is and role definition is not strongly related to the questionnaire measures of formalization. Like the above-mentioned centralization scales, the questionnaire instruments measuring formalization were scaled from high to low, that is, from a high degree of bureaucratization to a low degree of bureaucratization. Role definition and specialization are said (Inkson et al. 1970) to be highly representative scales of Pugh et al.'s (1968) all-inclusive indicator of formal structure, "Structuring of Activities." Unlike the original study, the present data do not show a sufficient

convergent validity of these two institutional measures of formalization ($r = .24$, N.S.). The short character of the role definition scale and its consequent poor discriminatory power may explain why it is not strongly related to the questionnaire measures of formalization and specialization. It should also be noted that this scale involves primarily role activities of production departments, while underrepresenting roles of other than production departments. In contrast, it should be stressed that specialization in the Pugh et al. (1968) framework bears no relationship with the more conventional indicators of specialization if such a measure includes the degree of professionalization of the organizational staff, as in the studies by Blau et al. (1966) and Hage and Aiken (1967). Indeed, this measure reflects only part of the division of labor or, more specifically, the extent to which organizations employ at least one full-time specialist for each of 16 staff activities that any organization, according to Bakke's (1959) conceptual scheme, has to carry out. These activities do not include activities of production departments, since a heterogeneous sample of manufacturing organizations inhibits the development of operationally defined categories of work-flow activities, which may be subject to specialization and which are comparable or isomorphic across different kinds of organizations. Therefore, a more inclusive measure of division of labor did not seem feasible. Unfortunately, such a scale of formalization is also bound to be contaminated by size, since a small organization cannot score highly on such a dimension—both from a mathematical and empirical point of view. Although most studies show a strong relationship between the structural attributes and size, one would prefer to have scales which are "size proof," especially since such studies argue that bureaucracy is not a unitary concept and that the dimensions may vary independently of each other (Hall 1963; Pugh, Hickson, and Hinings 1969).

The exclusion of work-flow activities in the construction of the specialization scale may explain why this measure of division of labor in table 3 is unrelated to those questionnaire data of formalization, which were based on the subsample of production department employees only. In contrast, as table 2 shows, there appears to be a strong and significant relationship between this scale of specialization and the questionnaire measures of formalization of the total organization. It can be concluded that the degree of specialization implies the existence of many rules and procedures, a strong emphasis on enforcement of such rules, and a heavy reliance on formal sanctions. The degree to which written communication exists is not related to this measure of formalization.

The questionnaire data have a much stronger relationship or predictive power with regard to specialization than does the institutional scale of role definition. Also, the questionnaire data reflecting formalization tend to be independent of the "objective" indicator of formalization, denoting the

degree to which rules and procedures and communications are written. Role definition is even uncorrelated with the degree of written communication. However, as was stated before, this scale points primarily to the formalization of workflow activities, and, as table 3 shows, there is some convergence between this scale and some of the questionnaire scales of formalization of production departments. Job codification, which measures the extent to which incumbents of organizational positions perceive their roles as clearly defined, is strongly related to role definition.

It seems that the convergent validity is not strongly present in these data, and that the institutional measures tend to be independent of each other, thus calling into question their representativeness of the cluster of variables of formalization. According to table 2, the questionnaire measures, excluding written communication, covary strongly so that they legitimately can be considered as constituting one cluster and do possess convergent validity. Strictness, being a scale measuring formalization, correlates lower with the other scales than had been anticipated. The scale, developed by Perrow (1970), had a rather small and skewed distribution and was therefore less likely to fit this cluster of formalization indices. However, first-order correlations, holding size of the organization constant, suggested that this somewhat deficient scale may appropriately be included in the cluster of questionnaire variables of formalization. A revision and extension of this scale to improve its measurement ability should be recommended, in order to maintain this assertion.³

Summarizing, a comparison between the various indicators of formalization suggests a conclusion which is almost as unexpected as the one which was formulated with respect to the centralization indicators. There appears to be some tendency toward convergence, especially if the statements concerning the empirical referent of the indicators can be upheld. It would have been desirable if the specialization scale had also incorporated specialisms which are unique to production departments. Its convergent validity with the questionnaire indicators might be increased if attempts to include work-flow specialisms were to succeed in such a way as to be general enough to be applicable to a wide variety of manufacturing organizations. Similarly, the construct validity of the role definition scale is likely to be enhanced if role activities of other than production departments are included. Again, the magnitude of the coefficients seems to indicate that formalization as defined and measured is not unidimensional.

³ In a personal communication Perrow suggested that strictness should not be considered an indicator of formalization in the traditional bureaucratic, "official" sense, but rather a scale which measures the behavioral implications given a certain degree of formal structure. He claims that the Hall (1962) and Hage and Aiken (1967) items pertain to the factual situation, without including the possible concomitant conformity to the formal expectations of the organization.

Centralization and Formalization

Although the data so far have not convincingly demonstrated that the different indicators of the two components of bureaucracy allow for reducing them to two dimensions, the predictive power of each indicator vis-à-vis the other may be determined by following the Campbell and Fiske (1959) procedure. Hage and Aiken (1967), for example, found participation in decision making to be more substantially related for formalization than for hierarchy of authority, and therefore claimed that the first one has greater predictive power. However, both can be considered as being determined by the same latent trait, or by a third trait.

The other two quadrants of the off-diagonal submatrix of tables 2 and 3 show the product-moment correlations between the different measures of centralization and formalization belonging to the two approaches—the so-called heteromethod-heterotrait values. The entries in the rectangles adjacent to the off-diagonal submatrix of table 2 show the within-approach correlations between centralization and formalization. These latter coefficients tend to be consistent with the ones found by Hage and Aiken (1967) and Pugh et al. (1968) and the replication by Hinings and Lee (1971) of this latter study. The between-approach correlations raise many questions, especially because they suggest controversial conclusions.

Following the conventional norms of significance, the entries of the upper right corner of the submatrix of tables 2 and 3 are not significant, but comparing these relationships with the original findings of Pugh et al. (1968), Hage and Aiken (1967), and Hall (1962) necessitates a skeptical and cautious assessment of the conceptual definition of both role definition and autonomy. Thus, while specialization measures an aspect of formal structure of nonproduction departments, role definition measures the degree of written formalization of the production or work-flow department(s) of the organization. The questionnaire scales of centralization test the degree of centralization of the total organization, including the production and nonproduction department(s). Similarly, worker/supervisory ratio and the number of direct supervisors reveal the differentiation of authority positions of production departments, whereas the questionnaire indices of formalization tap this structural property of the organization as a whole. The nature and scope of all these measurement instruments has to be taken into account when evaluating the size of the heterotrait-heteromethod coefficients in tables 2 and 3.

In the same unexpected way as autonomy was negatively related to the questionnaire measures of centralization, role definition seems to have a direct relationship with centralization and vice versa. Since this scale measures the degree of actual formalization of a production department rather than the degree of overall organizational formalization, the magni-

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tude of the values in table 3 is not likely to be due to statistical artifacts or measurement error. It seems that the degree to which an organization's production department relies on written documents to prescribe activities enhances its participativeness. At this point, no satisfactory explanation can be provided. However, case studies, in addition to a thorough examination of third variables which might have confounded these results, should complement these findings. Most preferable would be longitudinal studies which are able to determine to what extent the institutional measurements to assess properties of organizational structure lag behind the present arrangement of positions and institutionalization of activities as reflected by questionnaire data.

Generally, the institutional measures of centralization tend to be unrelated to the questionnaire measures of formalization. There is one exception, however. As table 3 shows, job specificity of production departments is strongly related to two indices of the differentiation of the authority structure, namely, worker/supervisory ratio and number of direct supervisors. This finding seems to indicate that on the level of production departments two institutional measures of centralization and one questionnaire measure of formalization share a common underlying concept, which for convenience's sake may be called bureaucracy.

In summary, this comparison between scales aimed at measuring centralization and formalization suggests that they are not completely independent. The configuration of authority positions in production departments as reflected by a scale such as number of direct supervisors is strongly related to one questionnaire measure of formalization. This finding is somehow consistent with the tendency earlier noted of institutional measures to have a bias toward production departments. To a certain degree, this finding also implies the presence of non-orthogonality of the dimensions, that is, the existence of a "general method factor." The data do not allow for unequivocal conclusions as to whether these heterotrait-heteromethod coefficients point to the interrelatedness of the dimensions in production departments or to the existence of a general method factor. It appears that formalization and centralization, as measured by these two sets of instruments, are not completely independent. In general, however, these two sets of measurement instruments may be considered as probing different dimensions, both on the organizational level and on the production unit level.

CONCLUSION

Returning to the outset of this paper, the question of whether some existing operational definitions of structural attributes unambiguously grasp "pure" phenomenon has to be answered in negative terms. The simultaneous application of six institutional and eight questionnaire measures of struc-

tural attributes raised serious doubts with regard to the unidimensionality of the components. Most between-approach correlation coefficients call into question the assumption that the different instruments tap identical structural concepts.

Perhaps the major outcome of this exploratory research concerns a validity aspect that is usually ignored. Most attention is commonly paid to the adequate sampling of all possible indicators of a concept. However, regardless of the adequacy of such domain sampling and regardless of the appropriate translation of a concept into observables, it is equally important to cover the totality of the object which is observed and described by such operational definitions. Since the empirical referent of most of the institutional measures pointed to aspects of work-flow departments rather than to the organization as a whole, the convergent validity increased when these measures were compared with the aggregate scores of production departments. Probably a reoperationalization of these measures to encompass the total organization will enhance their validity as measures of organizational structure. However, even if such possibilities are taken into consideration, there still remains insufficient convergence of indicators tapping identical traits. In particular, the indices of centralization derived from the configuration of authority roles, such as, for example, the numbers of direct supervisors, do not bear a significant relationship to questionnaire measures of centralization.

A similar study, using a much larger sample and scales measuring other attributes of organizational structure, may not only increase knowledge about the validity of measurement instruments, but also indicate to what degree correlational studies have to be supplemented by multivariate analyses to determine the nonadditive and curvilinear relationships between the components. Blau and his colleagues (1966), for example, have found that the interrelationships between bureaucratic components do not justify simple correlations between pairs of attributes, and a dimension such as professionalization or expertness might be a substitute for centralization of authority rather than a reinforcer for the other. Using more sophisticated models, we might be able to state under which conditions different scales, purporting to measure identical attributes, do intercorrelate. Methodological development concerning organizational characteristics will enhance our understanding of the complex interrelationships of organizational phenomena and advance the construction of sound theories of complex organizations. But such forward steps cannot even be considered until researchers have developed concepts and corresponding operations that can describe the similarities and differences among organizations along meaningful and differentiated dimensions, using methods that fit the concepts. Even this primitive stage of development has not been achieved.

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Commentary and Debate

COMMENT ON GANS'S "THE POSITIVE FUNCTIONS OF POVERTY"

On behalf of functional analysis, Gans (1972) has set out to accomplish two objectives: (1) to demonstrate the usefulness of functional analysis; (2) to show that functional analysis, rather than having a conservative bias, is ideologically neutral. His essay "The Positive Functions of Poverty" (1972) seems to me to have accomplished just the opposite of these objectives; it is a convincing example of both the uselessness and conservative bias of functional analysis.

With respect to the first objective, Gans has simply demonstrated (if we accept his rather impressionistic evidence as I am doing for the sake of concentrating on theoretical points) that poverty benefits the nonimpoverished. We are told that poverty exists because it is "functional" for the nonimpoverished; or, put in the reverse, the eradication of poverty would be mostly "dysfunctional" for the nonimpoverished.

The mere ascription of "functional" or "dysfunctional" does not, however, constitute functional analysis. If we accept, as Gans does, Merton's definition (1949, p. 50) of function—namely, "those observed consequences which make for the adaptation or adjustment of a given system; and dysfunctions, those observed consequences which lessen the adaptation or adjustment of the system"—Gans's essay has not really pursued the main purpose of functional analysis—namely, the discussion of function and dysfunction from the standpoint of system adaptation or adjustment.

Contriving his own version of functional analysis, Gans has elected to reject the term "system," but more important, he has also failed to use the terms "adaptation" or "adjustment" or some equivalent of the terms. Consequently, he has failed to discuss how poverty affects the capability of a system (or group) to adapt or adjust; more specifically, he has not explored the ultimate question of whether the persistence of poverty has enabled the nonimpoverished to adapt or adjust to their social circumstances and environment. This is the crux of functional analysis.

Through his failure to carry forth a truly functional analysis of the effects of poverty, and considering that this was his avowed intention, Gans has demonstrated through example the uselessness of functional analysis. The point of the essay is that poverty does benefit in very practical and observable ways the nonimpoverished groups. The question of whether this benefit helps groups adapt or adjust as systems, whether it helps (in Parsons's terms) to maintain "equilibrium" or to enable the fulfillment of "functional prerequisites," is a moot question in Gans's essay. (And to commend Gans in an incidental way, I think it should be a moot

question for other sociologists. The terms of functional analysis are vague, reified, incapable—as Gans points out in reference to “system”—of satisfactory empirical application; they are metaphorical, being taken mostly from the biological sciences; and they are based on the questionable assumption that something in the social world resembles a system, a function, an adaptation or adjustment.)

Though not really doing functional analysis, Gans does embrace its liberal conservatism. His overriding moral imperative is the preservation of social order. When it comes down to the crunch of having to serve the welfare of people or of the existing social order, Gans proposes alternatives in the interest of the latter. For example, the poor should continue to do the “dirty work” that cannot be eliminated, only at better wages; or, on the matter of serving as a reference point for the status and mobility aspirations of others, the poor should be given 75% of the median income rather than the current 40% rate.

Gans’s alternatives center around the idea of giving the poor more money. Even the question of power can be answered mostly with money, for (1972, p. 286) “increases in income are generally accompanied by increases in power as well.” He concludes (1972, p. 288) that well-balanced functional analysis “can take on a liberal and reform case, because the alternatives often provide ameliorative policies that do not require any drastic change in the existing social order.”

We are finally told that the conclusions of functional analysis need not differ from those of radical sociology. This understanding of radical sociology is astoundingly naïve. Of course, radical sociologists would not disagree that poverty benefits in many ways the nonimpoverished. But from the Marxist analysis of class struggle and dialectical materialism, the persistence of vast discrepancies in material well-being is a foregone conclusion. The central issue is not that of giving the poor a little more money but of asserting democratic control over the productive instruments and resources of a society which would be classless. While Gans and other liberal sociologists attempt to perpetuate the welfare state and put forth alternatives which “do not require any drastic change in the existing social order,” radical sociologists recognize that we all remain in poverty—if not economic poverty, certainly social, cultural, and spiritual poverty.

As a scientific method of study from which realistic alternatives or remedies can be drawn, functional analysis is useless for radical sociologists. Whereas it does suggest (in an overly vague, abstract, and metaphoric way) how societies or groups adapt and adjust, maintain equilibrium, fulfill functional prerequisites, etc., it has virtually nothing to say about maladaptation or maladjustment, the permanent disruption of equilibrium in a given system, the disintegration of society—in short, about the condi-

tions necessary for the drastic or revolutionary change radical sociologists are seeking.

Most radical sociologists recognize functional analysis to be a liberal polemic against revolutionary change which originated at Harvard University during the 1930s. As a result, they have little use for functional analysis and no use for the ideological conservatism of a would-be functionalist such as Gans.

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REPLY TO HANSON

As a critic of functional analysis, Hanson is surprisingly orthodox, for the first part of his comment accuses me of failing to "carry forth a truly functional analysis." According to Hanson, such an analysis must use Merton's definition (1949, p. 50), must include the terms adaptation and adjustment, and must analyze functions and dysfunctions "from the standpoint of system adaptation or adjustment," and by his criteria I have obviously failed. As I indicated in my article, however, I chose to define functions and dysfunctions as the observed consequences which are positive or negative as judged by the values of the groups and aggregates under analysis (Gans 1972, p. 276), but all of my deviations from Hanson's conception of functional analysis were supported by or borrowed from Merton's 1949 article. The one exception is my omission of adaptation and adjustment, but as I noted in my article (Gans 1972, p. 276, n. 4), Merton has more recently written that "dysfunction refers to the particular inadequacies of a particular part of the system for a designated requirement" (Merton 1961, p. 732).

Subsequently, Hanson engages in a confusing dialectic. After criticizing me for failing to do a truly functional analysis, he argues that I "have demonstrated through example the uselessness of functional analysis," and then commends me for eschewing a systems-centered analysis. I do not understand why my analysis exemplified the uselessness of functional analysis when I did not, according to Hanson, do a truly functional analysis in the first place, and why I am then praised for not doing so.

Hanson's second criticism is that even though I am not doing a functional analysis, I am still embracing "its liberal conservatism" and that "my overriding moral imperative is the preservation of social order." If social order is defined as any social arrangement not preceded by a Marxist revolution, as Hanson defines it, then his criticism is justified, but raising the income of the poor from below 40% of national median income to 75%, and thus nearly doubling it, does not strike me as liberal conservatism, particularly since it would require a considerable amount of income redistribution (Gans 1973). And while I did not say that "the poor should continue to do the dirty work that cannot be eliminated, only at better wages," but said instead that "society's dirty work could be done without poverty, some by automating it, the rest by paying the workers who do it decent wages" (Gans 1972, p. 284), again, I would think that raising wages for dirty work is hardly conservative doctrine. Actually, however, I did not recommend either of the above measures; I only suggested them as functional alternatives to replace poverty. Here as elsewhere, Hanson falsely treats my analytic statements as prescriptive ones. Also, I did not conclude that the application of functional alternatives would make for "a well-balanced functional analysis"; rather, I said that this would make functional analysis "more complete" (Gans 1972, p. 288) but indicated in the very next sentence that it would still be incomplete.

Nor did I say that "the conclusions of functional analysis need not differ from those of radical sociology"; I wrote that they "are not very different" (Gans 1972, p. 288). Again, Hanson confuses analysis with prescription, for I thought I pointed out what Marxists also point out, albeit in different words: that the affluent now run American society. To be sure, I did not call for "asserting democratic control over the productive instruments and resources of a society which would be classless," partly because I do not know any better than contemporary Marxists how one achieves a classless society, but I did not attempt to "perpetuate the welfare state and put forth alternatives which 'do not require any drastic change in the social order.'" All I said was that functional alternatives "often provide ameliorative social policies that do not require any drastic change in the social order" (Gans 1972, p. 288), but this was an analytic statement rather than a prescriptive one.

Finally, Hanson is wrong when he argues that functional analysis "has virtually nothing to say about maladaptation or maladjustment, the permanent disruption of equilibrium in a given system, the disintegration of society—in short, about the conditions necessary for the drastic or revolutionary change radical sociologists are seeking." While some contemporary functionalists may be more concerned with functions than with dysfunctions, Marx was himself a functionalist, and his analysis of

capitalism said all the things functional analysis has, according to Hanson, virtually nothing to say about. After all, functional analysis came into being long before the 1930s, and although contemporary radicals do not have to use it in their own research, they only put unnecessary constraints on their analytic approaches by automatically viewing it as the tool of a conservative conspiracy.

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COMMENTS ON SZYMANSKI'S PAPER "MILITARY SPENDING AND ECONOMIC STAGNATION"

I have read Mr. Szymanski's paper (*AJS* 79 [July 1973]: 1-14) with interest, but I am afraid I cannot agree with its methodology. This is not a matter of the particular techniques used but of the way he sets the problem up. He treats 18 relatively developed capitalist nations as though they were independent entities. The trouble is that they are not. They are interrelated parts of a single world capitalist system. Moreover, they are parts of very unequal weight: what happens in the United States has an enormous impact on all the others, while what happens in Norway has a negligible impact on the others. In my opinion, these considerations vitiate his procedures from the outset and deprive his conclusions of any presumption of validity.

If Szymanski agrees, as he appears to at several points, that it was military spending which prevented the United States from falling back into a state of deep stagnation such as prevailed in the 1930s, he should ask what would have happened to the rest of the capitalist world if that had been the situation in the United States in the period with which he deals. If the answer is that world capitalism would have been dragged down, again more or less in the manner of the 1930s, then surely the conclusion would follow that, for monopoly capitalism as a whole, military spending has played the role in the post-World War II period attributed to it by Baran and Sweezy.

There are of course many complex and difficult problems involved in the questions raised by Szymanski—problems of theory and of the uses of theory in the interpretation of history—which cannot even be broached, let alone discussed, in a brief note. But I think it is important to understand that these problems cannot be fruitfully dealt with by the methods he attempts to apply.

PAUL SWEEZY

Monthly Review

REPLY TO SWEEZY

I agree completely with Sweezy's contention that the 18 developed capitalist countries I treat are interrelated parts of a single world capitalist system, that the United States has a tremendous and asymmetrical impact on this system, and that military spending in fact has prevented the United States from falling back into a state of deep stagnation such as that which prevailed in the 1930s (i.e., that military spending is a *sufficient* condition for avoiding stagnation). But the conclusion that military spending must *necessarily* be the major factor in preventing economic stagnation in monopoly capitalist countries does not follow. The data presented in my paper refute this claim. For me to show that there is no positive correlation between military spending and economic prosperity within advanced capitalist countries is sufficient to refute his argument, providing that Sweezy or other critics cannot demonstrate how a specific property of the international capitalist system, the power of the United States, or some other third variable concretely intervenes to account for the observed correlations. For example, Sweezy could refute my argument if he were to show that the United States bought a higher share of the surplus production of the countries which spent the least on their militaries for use by the U.S. military machine. Sweezy's blanket rejection of the comparative method for analyses among advanced capitalist countries on the grounds that they are interrelated is superfluous. The comparative method is a powerful tool of analysis, providing sufficient care is taken to control for relevant variables. Variables related to interrelations among the units compared can be controlled for, as can any other measurable factors.

There are important differences among the leading capitalist countries, differences which must be understood if we are to understand the historical possibilities for change in the United States. The implication of my paper is that the United States could possibly develop a welfare capitalism of the Scandinavian variety, converting the locus of state spending from military to other uses. Such a conversion would necessitate a drastic re-orientation of the United States to the rest of the world, but the signs of

this historical possibility are emerging with the growing reapprochement with the leading socialist countries, the ending of the war in Indochina, and the growing likelihood of McGovern-type domestic and foreign policies being implemented by a Democratic president beginning in 1977. The implication of Sweezy's argument that only military spending can prevent economic stagnation could very well lead us into expecting that a non-military-interventionist McGovern-type policy is impossible, a conclusion which might very well not be justified.

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Review Symposium

The Intellectuals and the Powers and Other Essays. By Edward Shils. Chicago: University of Chicago Press, 1972. Pp. xiii+481. \$12.50.

Robert Nisbet

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No one in this country has concerned himself with the problem of the intellectual in modern society longer, more persistently, and with greater insight than Edward Shils. Most of the essays in this volume, 23 in all, have been published before, over more than a quarter of a century. Professor Shils describes them as "fragments of an unfinished mosaic" and as "explorations preliminary to" a larger and more systematic work. There are enough good things in this volume to lead one to look forward to that work.

There is always a certain difficulty in the word "intellectual" used as a noun. Is it synonymous with what Florian Znaniecki, in his classic of a third of a century ago, called "the man of knowledge." Are all sages and technologists, priests, artists, scholars, and scientists by very virtue of these roles intellectuals? Or is there an essence possessed by the intellectual as we find him in history not found, at least necessarily, in the roles I just mentioned? Znaniecki, so far as I can tell from a quick scanning, did not use the noun "intellectual." I don't think it was then much in currency. Even so, it is clear that by "man of knowledge" Znaniecki had in mind a role, or plurality of roles, substantially different from what we commonly mean by the noun "intellectual" today.

At the beginning of his book Shils describes the intellectual in terms reminiscent of Znaniecki's work. "In every society . . . there are some persons with an unusual sensitivity to the sacred, an uncommon reflectiveness about the nature of their universe and the rules which govern their society. . . . In this minority, there is a need to externalize this quest in oral and written discourse, in poetic or plastic expression, in historical reminiscence or writing, in ritual performance and acts of worship. This interior need to penetrate beyond the screen of immediate experience marks the existence of the intellectuals in every society" (p. 3).

There is nothing the matter with that. The dictionary will support any definition of the intellectual that refers to the activities of sages and technologists, priests and accountants, scholars and scientists. History, though, provides warrant for endowing the idea of the intellectual with a somewhat different, or at least variant, meaning: one that implies political thrust, a kind of rootlessness, a view of knowledge as primarily a weapon, and a certain adversary position with respect to culture, convention, and tradition. I think most readers would be disappointed if in a work titled "New York Intellectuals" they were confronted with role analyses of architects, chemists, engineers, scholars, philosophers, and others who work professionally with the meanings, signs, and symbols of our culture. They would

surely expect types more nearly like regular contributors to the *New York Review of Books*: those whose hard, gemlike flame is an acetylene torch rather than a lamp.

Much of Edward Shils's book is concerned with the intellectual in this sense of adversary or potential adversary of the established order. If I understand Shils correctly, though, he is implying that the role of intellectual as disturber of the political and moral peace emerges rather naturally from the role of man of knowledge—scholar, scientists, or philosopher. I question this. If I recall correctly, there was a craft in the Middle Ages, the weavers, that over a substantial period of time produced an extraordinary number of religious heretics. Clearly, we would be wasting our time trying to deduce heresy from the nature of weaving as such. There were other variables, chiefly historical.

I am inclined to think there is intrinsically no more reason why the man of knowledge should become an intellectual, that is, a disturber of the peace in moral and political affairs, than a machinist or ladies' garment worker should. The man of knowledge tends to be, on the evidence, as subject to conventionalization, routinization, and largely tranquil assimilation by the social order as any other role. I am not juggling meanings. There are men of knowledge and there are intellectuals: the existential roots of the two breeds are quite different, and while the twain can assuredly meet in a given individual, there is no more warrant for seeing each man of knowledge as a potential intellectual than there is for regarding full-time intellectuals as potential men of knowledge—that is, sages, technologists, scholars, and scientists. Quite apart from the different existential roots of the two classes, there are, as I will indicate briefly below, different historical roots. Ours, it might be noted in passing, is just about the only age in which the universities and other havens of knowledge have opened their doors to the class of intellectuals, a fact that explains a good deal of the distinctive kind of turmoil we've known since this practice began.

Sociology comes by its interest in the intellectual legitimately, for it was, after all, founded by as fascinating a pair of intellectuals as ever lived: Saint-Simon and Comte. Each was moderately learned and capable on occasion of rich insight, but each was also far less concerned with the light cast by his flame than with what could be burned away from the landscape. Each was, without question, a messiah. But so, deep down, is every intellectual: that is, he believes, he knows, and he seeks to convert. Interestingly, the sociology of intellectuals begins with Comte, for, in addition to being one, he observed and, to a degree, analyzed them. He didn't much like them and saw to it that the intellectuals who governed his utopia were properly clad in episcopal attire and anchored by a full measure of required writ, liturgy, and ritual. It takes an intellectual to distrust one, and Comte's distrust of the unattached intellectual was boundless.

Not really until the pioneering works of Le Play, Quetelet, Spencer, Durkheim, Weber, and a few others can sociology be said to have separated itself from the pursuits of messianic intellectuals, though it is unlikely that separation will ever become final divorce. Memory of the 1960s is sufficient

to that point. Scratch a sociologist and you may get a scholar, but you are almost as likely to get an intellectual, at least an intellectual *manqué*. In any event, sociology's interest in intellectuals has understandably persisted, and it has assuredly done more than any other discipline to illuminate the nature, sources, contexts, and aims of intellectuals. One thinks of, in addition to Edward Shils's scholarship on the subject, that of Merton, Parsons, Coser, Dahrendorf, and a few others who have diligently and systematically dealt with the class.

There is good reason for such scientific interest, for although our age is by no means the only one in the history of the West in which intellectuals—in acknowledged contrast to scholars and scientists—have prospered, ours is surely the age of the largest number of intellectuals and, quite possibly, of their greatest influence on the governing of the social order. One of the merits of Edward Shils's book is its detailed documentation of this point. The intellectual class at the present time is a powerful one in the West, and, such is the seeming thirst for intellectuals by governments, the class is likely to become more powerful.

This is becoming equally true, as Shils explains in a long and valuable section, of the new nations where we can already see the thrust of the intellectual. In this emerging class there is much the same negative attitude toward authority, tradition, morality, and convention that we associate with our own intellectuals. There is also the same uneasy relation to possession of power. Characteristically, the intellectual prefers to sit at the right hand of power. Occasionally, however, and this is especially true in the new nations, he is obliged to wield it, to become cabinet minister or administrative chief of some kind. Then comes what Shils well calls the "oppositional mentality."

"The transformation of the intellectuals in power discloses the duality of the oppositional mentality. The hatred of authority is often no more than a facet of the fascination and love that it evokes. When they come to power, intellectuals who have hated it quickly allow the identification with it, against which they have struggled previously, to come into full bloom. They attach to themselves the regalia of authority and feel that they and the state are now identical" (p. 419).

True, all true. One need but think of the intellectuals who managed to get themselves places in the French government after 1789 (at least those who were not hunted down as was Condorcet), who took posts in the Bolshevik government and held them until Stalin exterminated the class wholesale, or even the intellectuals who went to Washington, D.C., under President Kennedy. The oppositional mentality—which sees crises everywhere, along with injustice, inequity, and moribundity—quickly becomes, in its way, the bureaucratic mentality, though with usually greater self-assurance and, because commonly this class can get closer to its preferred seat at the right hand of power than can other functionaries, with greater short-run success. Impressive things can be done: wholesale reconstitution of calendar, poor law, education; also inauguration of a *levée en masse*, collectivization of agriculture, military entry into Vietnam. Then begins

oppositional mentality in a different sense: that provided, often fiercely, by intellectuals outside the structure of power. These latter become uneasily aware that there is more difference between two intellectuals one of whom is a bureaucrat than between two bureaucrats one of whom is an intellectual.

In his essay "Intellectuals and the Center of Society in the United States," not previously published I believe, Shils makes plain how fixed a pattern in modern society is the intellectuals' sense of being chronic outsiders, members of a minority group. The "penumbral culture of intellectuals in the West" has included a distrust, "even abhorrence," of the nonintellectual elites in politics and economy. "Institutions, established traditions, incumbents of positions of authority, and intellectuals who have accepted these have come in for severe criticism and rejection." Although, as Shils says, intellectuals in America could hardly have achieved their not insubstantial influence without a good deal more indulgence by the non-intellectual powers than intellectuals have been commonly willing to concede, there was, nevertheless, built up early a feeling of being rejected, discriminated against, separated from proper rank and dignity, and being generally ignored by those in power, whether economic or political. (To be sure, as Shils properly notes, there was in fact a gulf between intellectuals and other elites in America wider than anything that could have been found then in Paris or London or many another capital in Europe: hence the great appeal of European culture to so many American intellectuals down through the Depression.)

"Thus American society, although it was nurturing a vigorous and creative body of intellectuals of many diverse interests and talents, can scarcely be said to have been structurally dependent on their cooperation, and insofar as it was actually dependent, in agriculture, in the chemical industry and in the scientific civil service, these functions were invisible to those members of the intellectual classes—the literary and humanistic intellectuals—who designated themselves as 'the intellectuals' and who had grievances against American society for its neglect of intellectual things" (pp. 163–64).

I think it can be shown that it has always been this way, at least in the West. If there were no other reason for distinguishing between men of knowledge and intellectuals, and stressing the difference in their existential roots, comparative history would supply one. If there is a major omission in Shils's book it is the comparative perspective in time, though I do not deny evidences of his historical awareness. Perhaps this will be remedied in the larger study he is now at work on. My point is, just as one can profitably compare intellectual classes from nation to nation, continent to continent, in modern times, one can compare these classes in the light of their spasmodic eruption over the past two and a half millennia. The more deeply one looks into the nature of the Sophists in the Athens of the fifth century B.C., the Greek rhetoricians in Rome after Rome's conquest of the Greek world, the humanists of the Italian Renaissance (the heroes, if that is the word, of Burckhardt's classic), the *philosophes* in the French 18th century, the denizens of Europe's coffee houses around 1848, and, far from

least, the intellectuals of our day in New York, London, Paris, and other capitals, the more one realizes that we have here a recurrent social type in history.¹

It is this kind of analysis indeed that confirms the distinction we are prone to make between man of knowledge and intellectual. For whatever else the Sophists, humanists, and *philosophes* were, they were not primarily philosophers, scholars, scientists, or creative artists. Much of the flavor of the Sophists and their successors in history comes from their almost uniformly hostile and contemptuous attitude toward men of knowledge as such. And in each of the eruptions of the intellectual in Western history there would appear to be much the same identifying role characteristics, though it would be unwise to miss the very real differences from age to age. In all of the groups I've mentioned there is the basic rootlessness of mind and conscience, the clear tendency to flout the establishment, to assume more often than not adversary position toward the official and the conventional, and to proffer instant reason or intuition in place of either traditional authority or the kind of knowledge that goes into scholarship and science.

It should not be thought that intellectuals as we find them in the West are hostile to the *works* of literature, science, philosophy, and scholarship. They will borrow wholesale from these and often construct works which have the manner if not substance of those from which they have borrowed. But the intent is radically different; among intellectuals it ranges from sport—dealing with ideas as playthings—to revolution in the moral and social order. The style of the intellectual is different. Brilliance serves the intellectual in expression as profundity does the philosopher or thoroughness the scholar and scientist. The intellectual must sparkle! Finally, there is in each of the efflorescences I have mentioned in the West the same fascination with the properties and uses of power, characteristically the kind of power that accompanies new wealth, itself rather rootless, or recent political office. As I say, the intellectual enjoys sitting at the right hand of power, and he will not ordinarily boggle, on the evidence, even if this may have pronounced military thrust. That is just as true of Sophists, humanists, and *philosophes* as it is of the intellectuals who flocked in 1961 to Kennedy's White House. Whether Thrasyarchus the Sophist, Salutati the humanist, Voltaire the *philosophe*, or McGeorge Bundy, the pattern seems to run pretty much the same: crisis is cherished, military if necessary, as the context of the kind of thrust of mind, of brilliance of manner, and iridescence of decisions that intellectuals thrive on.

There is every good reason, then, for distinguishing the roles of man of knowledge and the intellectual (though naturally on occasion the same individual may assume both, however unequally in grace, effectiveness, and conviction). Most of us would have no diffidence about applying the label "intellectual" to Thrasyarchus or Hippias, Petrarch or Valla, Diderot or

¹ I have dealt with this in some detail in my "The Myth of the Renaissance," *Comparative Studies in Society and History* (October 1973).

D'Alembert, Robert McNamara or Daniel Ellsberg, or the regular contributors to, say, the *New York Review of Books*. We would probably draw back, though, from applying it to Homer, Aeschylus, Aristotle, Aquinas, Leibniz, Newton, Darwin, Planck, Shakespeare, Goethe, Tolstoy, and their numberless progeny and imitators, just as most of us would think it inappropriate to praise *The Iliad*, *The Origin of the Species*, or *Faust* with the adjective "brilliant."

Shils properly emphasizes, as did Lewis Coser in his *Men of Ideas* a number of years ago, that in our particular age it becomes increasingly difficult to separate intellectuals as a type from those who are supposed to be servants in the house of knowledge. The reason for this is the nearly unprecedented luster of the intellectual in our age; his light dazzles us, makes distinction difficult. Add the probably more important fact of the flocking of intellectuals—the intellectual type of mind and style—to the universities during the past quarter of a century and we have most of the explanation, I should think. The scholar and scientist have not been ruling in the academy in quite as unrivaled a way as they once did. There is too much competition from the intellectual-in-residence (never mind his title) whose sense of abiding crisis, need to flout or subvert official culture, and brilliance of rhetoric make him inevitably attractive to students, alumni, even administrators and trustees. Like it or not, the intellectual is a magnetic personage in our time; not just in New York but, thanks to the media, in Sauk Center.

So, for the most part, has the intellectual generally been magnetic (not really liked perhaps but magnetic) in history. People may have had an uneasy feeling about the long-run consequences of the Sophists, Greek rhetoricians in Rome (Cato was far from alone among Roman conservatives in detesting them), the humanists in Italy, and the *philosophes*, but so far as I can tell they paid to read and hear them. Voltaire died a millionaire, though shrewd instinct for financial speculation helped here. Scholars, philosophers, and scientists have, ever since Plato and Aristotle, given intellectuals a bad press, but this can perhaps be ascribed in some part to jealousy.

As Shils emphasizes, the greatest difference between the intellectual class in our day and earlier times lies in the growing tendency for this breed to become more and more attached to institutions, or if not actually attached, at least resident in them, often with tenure. We shall have the rest of the century to see how this comes out. Historically, intellectuals have not loved organizations nor they them. Indeed, much of the charm of the intellectual in historical writing and literature comes from qualities intellectuals have shared with the Robin Hoods of history—elegant outlaws and bandits supposedly helping the people from time to time. Humanists and *philosophes* loved assaulting the Middle Ages—sprung university and its scholars and philosophers; they did not covet membership. Our intellectuals seemingly do covet membership. As I say, we shall have the rest of the century, with no doubt countless books, articles, and dissertations done on the matter in sociology, to see how it all comes out. Will the universities shake free of

the intellectuals? Or will intellectuals shake society free of the universities? The latter of course would have appealed to the humanists and *philosophes*.

I predict, though, that other corporations besides universities will find it expedient to put intellectuals on their payrolls. The late Adriano Olivetti in Italy, I recall, collected intellectuals as American businessmen collect art. I understand that other business corporations do that regularly today in Europe. Why shouldn't General Motors or Xerox or Polaroid have intellectuals-in-residence precisely as our universities do today? The careers of McNamara, the Bundys, Kissinger, and Ellsberg give evidence of how versatile the ideals and proclivities of intellectuals are and, for that matter, always have been. The age of the multinational corporation, with its novel opportunities, should help here. So will the sheer abundance of intellectuals, each with a gun for hire and limitless willingness to travel. Anyhow that's my prediction.

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Edward Shils has been a sociological representative, interpreting diverse phenomena to diverse publics, for several decades. His numerous essays have appeared in many learned journals and scholarly collections in this country and abroad. Those who, unlike Professor Shils, confine their attentions to the professional sociological journals will have encountered his contributions only infrequently. Now, however, he is in the process of collecting a substantial number of these works for republication between hard covers. *The Intellectuals and the Powers and Other Essays* is the first volume of a planned series, the others to treat Indian intellectuals, macro-sociology, and the history of sociology. It is not too soon, however, for an overview of Shils's brand of sociology, for these essays are characteristically broad in scope. Not only as to geography, with sustained analyses of the American, British, and Indian scenes, but also in subject matter are these essays of general sociological import: Shils applies himself here to phenomena more conventionally considered under the narrower rubrics of political sociology; the sociologies of knowledge, science, culture, and education; and social stratification. Such essays as "Ideology and Civility," "Intellectuals and the Center of Society in the United States," "Daydreams and Nightmares," "Plenitude and Scarcity," "Metropolis and Province in the Intellectual Community," and "Intellectuals in the Political Development of the New States," as well as the title essay, are among the 23 collected here. They will be and have been of interest to sociologists in a large number of specializations.

It is necessary to remark that the volume is unabashedly a collection of essays. There is a rudimentary grouping of the pieces into three parts—"Intellectuals," "Intellectuals in Modern Societies," and "Intellectuals in Underdeveloped Countries"—and Shils does make the customary apology

for the occasional nature of these writings: "It will be seen that the essays in this volume are fragments of an unfinished mosaic. There are many omissions" (p. xiii). Indeed, he promises a full-length, systematic treatise on intellectuals within several years, a treatise for which these essays are to be regarded as preparatory excursions. Nonetheless, he is right to claim that many of the writings included are relatively self-contained. Moreover, Shils is a committed practitioner of the art of essay writing. He intends to communicate, inform, and persuade, and to do so in as elegant a fashion as possible.

The immediate question is whether the book succeeds in transcending its format. The answer is negative not so much because the essays are fragments but because of the large measure of redundancy inevitable in such a collection of writings on what one author regards as a single focus of interest and because of Shils's expository style. A few words, then, on the typical Shils essay. It begins with a pronouncement: "Western European and North American student rebellions in the present decade are more comprehensively and more fundamentally hostile toward authority than they were three and four decades ago"; or, "In every social system, there is a center from which authority emanates and to which deference is granted"; or, "The greatest menace to the political development of the new states is demagoguery." It proceeds to invoke a terminology—"center," "periphery," "tradition," "community," "primordially," "affinity," and "civility" are favorites—more by way of allusion than definition and to generalize about its topic historically and comparatively. Signposts, such as subheadings and other guides, are employed sparingly or shunned altogether. Apparently reasonable but wholly unsupported assertions are abundant. Documentation in any meaningful sense of the word is absent; even citations to the literature, of which, evidently, Shils has a thorough command, are scarce indeed. And opinions—whether implied or declared, abstract or pointed—blossom. The essay concludes with an exhortation, which typically entreates men of good will and high purpose to overcome the dangers the essay has analyzed. (Not all the essays have this form; an early [1938] treatment of limitations on academic freedom is a piece of concrete, moderately expressed, muckraking research. To repeat: the typical essay has this form.)

The prevalence of exhortations raises a theoretical problem to which we shall return below. The opinions, while they are often gratuitous and only purportedly unfashionable, can be welcomed as relief from the soporific character of too many of Shils's essays taken at one sitting. It aids comprehension to know that Shils is appreciative of Michael Polanyi, George Kennan, Henry Kissinger, and *Encounter*, and deprecatory toward Noam Chomsky, I. F. Stone, McCarthy (Joe, Gene, and Mary), and the *New York Review of Books*. It communicates something of his image of society when Shils extols dutifulness, privacy, the family, and bookshops, and reproves rebelliousness, populism, New Leftism, Old Leftism, and departments of black studies. It stirs attention when one sees much of modern social science chided, then excused, for its "niggling puniness and triviality."

The lack of documentation and the often unnecessary degree of global generality to Shils's assertions require the reader to do much of the author's work for him or to grant him a great deal of intellectual authority; but that is no new problem for readers of sociology. The density of declarations and truisms—"no country can ever be fully self-sufficient in its intellectual life"—and of redundancies, whether of whole discussions repeated in different essays or of overused concrete illustrations, can be avoided by using this volume properly as a resource to dip in, ponder awhile, and reflect upon, rather than proceeding cover to cover, as is the reviewer's responsibility.

The matter of style, however, will not rest with that. Because Shils is addicted to the highly general but reasonable assertion, and because of the uncertainty of his use of concepts, the reader is all too often left wondering precisely what is intended. For example, on page 401 we are told that "the socialistic and populist elements in the politics of the intellectuals of underdeveloped countries are secondary to and derivative from their nationalistic preoccupations and aspirations." On the next page, however, a rather different picture emerges: "The socialism of the intellectuals of the underdeveloped countries grows, fundamentally, from their feeling for charismatic authority, from their common humanity, and from the antichrematistic traditions of their indigenous culture. More immediately, it is a product of the conditions and substance of their education, and [finally] of their nationalistic sensibility." No doubt an empirical researcher schooled in the techniques of multivariate analysis could sort out this list of factors, weight them, and determine which were more "fundamental" than others. But Shils's efforts too often obscure rather than illuminate such proto-empirical propositions. One wonders how much of a commitment is represented by a highly general declaration in Shils's essays.

More consequential is the matter of concepts. Take the obviously pivotal term "intellectuals." Who are they? As in many sociological investigations, such a status concept can be defined structurally—incumbents of such roles as scientist, teacher, student, journalist, etc.—or functionally—those who provide society with its required meaningful interpretations of the cosmos (a definition rather more difficult from the point of view of empirical research). Then again, from an aggregative perspective, "intellectual" can be defined in terms of easily measurable individual attributes—years of formal education, for example—or in terms of dispositions—the intellectuals are those with "an unusual sensitivity to the sacred, an uncommon reflectiveness about the nature of their universe and the rules which govern their society" (p. 3). From time to time, Shils employs each of these definitions. In particular, the structural definition seems to be the implicit operationalization for intellectuals in modern societies and the educational one for the new states, whereas the functional and dispositional criteria seem most congenial to Shils's more general vision of society. If one holds to an Aristotelian faith in the congruence between the characteristics of an individual and the attributes of his social role, such definitional ambiguity may be pardoned. Yet for the purposes of judging the adequacy of the assertion,

say, that intellectuals were incorporated as never before into American society following World War II, it is important to know what is intended by the term (professors, engineers, scientists, poets, clergymen, ideologues?) and to know it in something other than an ad hoc fashion. Here and also for "ideology," "center," and other terms, Shils seems to assume the existence of a consensual universe of learned discourse, which he seeks but realistically denies. Although he is aware that ambiguity and vagueness are serious hindrances to cumulation of sociological knowledge (see his essay, "Tradition, Ecology, and Institution in the History of Sociology," *Daedalus*, vol. 99 [Fall 1970]), his own writings compound rather than alleviate the problem.

It is doubtful that Shils would claim clarification, refinement, and codification as his major theoretical contribution. Years ago, he proposed a different role for theories in sociology: "Sociologists must cease to look upon them as finished products, waiting to be applied, *in toto*, in an orderly and systematic way. They must be brought into operation only on the basis of a feeling of personal intimacy. They must be used only after an osmotic assimilation which involves discriminating acceptance and rejection, which rests on the sense of fitness and appropriateness rather than on any formal test" ("Primordial, Personal, Sacred, and Civil Ties," *British Journal of Sociology* 8 [June 1957]: 145). What one gains from a reading of Shils is most of all such an osmosis of a general guide. He offers a distinctive vision of society and its intellectuals.

That vision, and not merely the opinions that decorate it, is a conservative one. With Burke, Shils views man as motivated by nonrational but not wholly barbarous inclinations, notably including religious, personal, and "primordial" (kinship, locality, sex, race, and age) bonds. Such bonds cannot be exclusive of wider commitments if social life is to be possible, yet they are valuable both intrinsically and instrumentally. Without the required recognition of birthdays, anniversaries, and illnesses by greeting cards, we would have even fewer and more pitiful bookshops in American towns than we do. Without the matter-of-factness and parochiality of the activities of the mass of men—Shils evokes resonances of Martin Luther on this score—society would too readily fall prey to the ideological zeal of the intellectuals. The need for a cognitive and moral map of the universe is experienced by all, but is attenuated among the mass and accentuated among intellectuals. If, as experience indicates, intellectuals are deficient in a sense of affinity for the average man and his mundane attachments, their passion for purity and consistency and their disregard of the complexity of virtue can create havoc for civil society. On the other hand, without some dispersion of more elevated concerns throughout society—and this is especially required in the new states—primordial bonds can be an impediment to development—social, economic, and political. Whether either side appreciates it, the intellectuals' attachment to the central value system and the masses' conservatism, complacency, and hedonism are complementary. Such issues of dispositions and their social consequences comprise much of the content of these essays. Shils's observations on the differential dis-

tribution of attachment to the central value system, while hardly a step in the direction of a conflict theory, is a useful corrective to the Parsonsian imagery (to overstate the case only slightly) of a universal, fully internalized, normative consensus.

If Shils's social psychology is imbued with the spirit of a moderate conservatism, so also is his macroscopic imagery. Scarcity, in spite of the contemporary youth movement's daydream to the contrary, is an ineluctable fact of human society. Authority is not only a need of all men and an obsession for intellectuals, it is a permanent feature of social life. More consistently and over a longer period of time than his erstwhile collaborator, Talcott Parsons, Shils has held—without using the terminology—to a fiduciary image of elites in society. The biologically suggested imagery that stability and progress are automatic processes is not congenial to him. (Likely for this reason, what appreciation of Parsons's contributions is evident in Shils's writings is primarily confined to Parsons's early synthesis and elaboration of "action theory" and does not extend to the system theory of the fifties and later.) On the matter of authority, intellectuals are again a necessary but troublesome element. They provide much of the knowledge and the inspiration for social improvements, but their attraction to intolerant, oppositional, self-defeating, and unrealistic ideologies (a term Shils uses to connote much of what Mannheim meant by "utopias"), combined with their increasingly central role in both modern and underdeveloped societies, is conducive to disorder, as seen especially in the 1930s and late 1960s in this country. The Tocquevillian proposition that the intellectuals and the powers can be euphoniously interrelated under the right circumstances is qualified by Shils's perception of the persistence of the intellectuals' uncivil ideological traditions.

Shils's conservatism should not be mistaken for romantic reaction. True, as many of the literati do, he feels disdain for the brutality and philistinism of mass culture and worries about the abundance of narrow specialists in advanced society. But he insists that society is becoming more humane, more inclusive, more affluent, and that those who evince a desire for a return to the past are masochistic. He recognizes the noise and violence of American society but appreciates too its vigor and creativity. (His twin institutional and residential attachments, Cambridge and Chicago, are perhaps symbolic of these highbrow and mundane inclinations.) Unlike many sociologists of knowledge, Shils is conversant with and admiring of ("hard") science, and in this area the United States excels. Shils's manifest approval of the institutionalization of citizenship and of cognitive rationality, as well as his long-standing civil libertarianism, are elements of an Enlightenment heritage.

The conservative and Enlightenment aspects of Shils's world view are nicely combined here in his studies of the scientific community. Just as his concept of the political "constitution" is not confined to formal provisions and regulations but includes a substantial component of informal relations as well, so also the advance of scientific knowledge is not merely a matter of formal codification. The scientific tradition contains anecdotes and

stories as well as laws, and its communication comes by way of gossip and conviviality as well as by publication. Shils is skeptical of the heroic-individualistic model of scientific advance and instead recommends the creation and maintenance of scientific communities and institutions. This recommendation is directed not only toward social science, but also (as Thomas Kuhn and Shils's student Joseph Ben-David have shown) toward science generally. The lassitude of science and culture in the new nations is attributable not to their citizens' intellectual deficits but to the insufficient development of personal and day-to-day intellectual dialogue. Just as his studies of value systems emphasize such structural attributes as distribution of commitment, relative consistency, and institutionalization (in contrast again to Parsons's stress on the content of values), so also his studies of scientific intellectuals center on the structure of their social action. More important than the integration of a body of knowledge is the integration of its carriers.

In another respect, (natural) scientists have provided a model for Shils, and that is in their incipient "civility." Civility, a virtue not confined to politeness but including as well a conscious sense of care for the whole of society and affinity for one's fellows, is a requirement of a stable and decent social order. It is not likely to be found with any great intensity among the mass of men, but its shortage among them is ordinarily no severe threat to social order. Unfortunately and more consequentially, it is also often lacking among the ideologically inclined intellectuals. But some scientists, perhaps because their *Sachlichkeit* holds in check their proclivities toward questions of ultimate meaning without vitiating their attachment to traditional values, have acted politically in a serious and responsible manner (e.g., the scientists' movement for arms control and the peaceful uses of atomic energy following World War II). Presumably in order to encourage the institutionalization of this source of civility, Shils founded and spends much time editing *Minerva: A Review of Science, Learning and Policy*. Like Durkheim, who worried lest politics corrupt sociology but whose own editorial and institutional activities were impelled by a politics of knowledge, Shils is a scholar of minimal passion for state politics but of great commitment for the active institutionalization of a point of view. In that sense, a sense that Shils would not accept, Shils's activities are highly political.

We are thus brought back to the characterization of these essays as hortatory in nature and to the issues raised thereby. To note that this book is in large part a collection of sermons is not simply to castigate Shils for his violation of the canons of *Wertfreiheit*; it is to remark upon his image of the world. The crucial aspect of that image is the imputed audience of the sermon; that audience is composed of intellectuals.

Shils, it develops, is a consistent elitist. He is not among those who assign responsibility for social benefits to an elite but who blame social evils on their inferiors. On the contrary, he upbraids the intellectuals when he deems them to have contributed to irrationality, philistinism, chaos, or brutality, and that is often indeed. Presumably the poor quality of Holly-

wood films and of undergraduate instruction, the prevalence of ghetto insurrections and students' attacks on university administrations, even much of the anti-intellectualism of the 1950s, are attributable to the intellectuals. In his preface, Shils states this perspective to be a theme of the volume:

The decade and a half of the Weimar Republic seemed to me to have been a wonderful time; I thought of it as having been begun and conducted for the most part by men of good will, unaided at best and most of the time frustrated by Junkers, businessmen, communist and nationalist fanatics, and despised or disregarded by most of the prominent intellectuals of the time. It seemed such a pity to me that the intellectuals should have been responsible for destroying a society which in so many respects conferred such benefits on intellectuals.

The intellectuals of the Weimar Republic, with all their talent and, in some cases, genius, had helped by their active mischief and by their complaisant indifference or approval to ruin what might have been a decent society. [Pp. vii-viii]

The focus is on the harm intellectuals can do to society, and occasionally on their contributions. Other actors, other factors, are for the most part disregarded. Even when Shils immediately qualifies the judgment regarding the role of the intellectuals in the downfall of Weimar, the qualification is without effect: "Of course [the intellectuals] were not exclusively responsible. Even had they been more sensible, they might not have been able to withstand the ravages of military defeat and national humiliation, inflation and unemployment, the cruel manipulations of the KPD, dominated by the Soviet Communist Party, the brutality of the nationalist sects, the infidelity of the army and the civil service" (p. viii). The reader of the book will encounter such lists of other factors, though rarely such a long list, but he will find little sustained analysis of them. Intellectuals are the independent variable par excellence.

But, it may be objected, should not an author be allowed a point of concentration? Surely he should, but that concentration should delimit as well as expand the role of the factor he wishes to illuminate. Yet typically Shils will adduce factors in the context of which intellectuals act, only to dismiss them with an exhortation. The incentives for professors to disregard undergraduate teaching can be overcome by the "best man," as can the seductive attractions of work in the mass media. Moreover, the variables Shils places greatest emphasis on are dispositional ones: utopianism among intellectuals, lack of self-respect among authorities (especially in the 1960s in the United States), and the mass contempt elicited by weak and ineffectual authority. That the common man's primordial attachments may play a role in communal violence; that anti-intellectual movements may be fostered by conventional political elites; that structural constraints on the position of intellectuals may be overwhelming; that interests play a major political role—these propositions receive little attention or are dismissed as ideological. The subjectivist and voluntarist message of early Parsonsian action theory is manifested here in such a way as to distort rather than clarify the place of intellectuals in society.

Book Reviews

Durkheim: Morality and Milieu. By Ernest Wallwork. Cambridge, Mass.: Harvard University Press, 1972. Pp. viii+224. \$10.00.

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We are so accustomed to claiming Émile Durkheim, with Max Weber, as the greatest of figures in the history of sociology that we tend to ignore his impressive contributions to pedagogy and to moral philosophy. Teachers' colleges, of course, have given pedagogy a bad name (someone has uncharitably suggested that they are places where artificial pearls are cast before real swine), but the history of education in Western civilization, as exhibited in Durkheim's two-volume work of 1905 and in various separate papers, merits renewed attention. Paul Fauconnet, Harry Alpert, Everett K. Wilson, and the late Sherwood D. Fox—along, of course, with Talcott Parsons—have dealt with these contributions to pedagogy, but some of us have lost sight of them in our concern for the massive contributions to sociology proper. It is one of the many agreeable aspects of Ernest Wallwork's book, a doctoral dissertation at Harvard, that it encourages us to look again.

Wallwork, in fact, regards Durkheim's lectures on pedagogy, delivered at the Sorbonne during the academic year 1904–5, not only as remarkable but as one of his most important theoretical works. The history of pedagogical ideals, as traced by Durkheim from the early Middle Ages to his own time, has a moral dimension, and it is his insistence upon the virtues of discipline, autonomy, and attachment (attachment, that is, to our own groups in society and to the collectivity itself) that gives meaning to the enterprise. Wallwork notes that Durkheim sees the roots of modern science and of secularization in none other than Martin Luther and does so with some excitement, because it was in the same academic year that Weber published *Die Protestantische Ethik* in the *Archiv für Sozialwissenschaft und Sozialpolitik*. This is a coincidence that will give pleasure also to Edward A. Tiryakian and Robert K. Merton. (Incidentally, Anthony Giddens has recently found one exception to Tiryakian's brilliant discovery that Durkheim and Weber studiously ignored each other's work, but it is an exception so minuscule in character that it strengthens Tiryakian's case.)

Durkheim's lectures on the family and on the state also receive a careful treatment in Wallwork's book. But it is Durkheim's place in the history of ethical theory to which he devotes the majority of his pages. In 1893 Durkheim began the preface to the first edition of *The Division of Labor* with the dramatic announcement that he intended "to treat the facts of the moral life according to the method of the positive sciences." Twenty-four years later, in the last summer of his life, he repaired to Fontainebleau

to devote himself to writing his book on ethics. Death forestalled the effort, and accordingly we have to piece the theory together from sometimes disconnected passages in the extant works. It is a task that Wallwork has now done for us, exhibiting in the process an impressive command of the materials. His contention that Durkheim's sociological theory was a by-product of his ethical concerns may be a slight exaggeration, but it is certainly a forgivable one in view of the fact that for Durkheim "social" and "moral" were almost synonymous adjectives. In any event, it is a "neonaturalistic" ethic and one that belongs in the mainstream of 20th-century ethical thought.

Wallwork's accomplishment is not entirely free of blemishes, of which a fondness for split infinitives is the most trivial. It is something of a mystery also why his quotations from Durkheim are sometimes in English and sometimes in French and why, even in the former instances, page citations are frequently given to French editions. Substantively, more serious questions arise. It will not do for the author to tell us (as we all knew) that Durkheim subscribed to a sociological realism and at the same time entertained certain propositions consistent with sociological nominalism. The problem is more complex than that. It is at least as old as Boethius's translation of Porphyry's *Introduction to the Categories of Aristotle*, and Wallwork's rather strenuous efforts to rescue Durkheim from his metaphysical difficulties do not succeed. It is confusing, too, to be told that Durkheim engaged in a new kind of "reductionism," namely, a sociological reductionism, unless one wants to use that word in an idiosyncratic manner. The notion, furthermore, that "the symbols of religion have their own integrity" and, presumably, autonomy, apart from society, is unacceptable unless one is willing to reject altogether the thrust and emphasis of Durkheim's sociological derivation of the categories of the human understanding.

These quodlibets aside (and others that might be mentioned), it is a pleasure to say that Wallwork has made a major contribution to our knowledge of Durkheim. The book as a whole takes its place with Morris Ginsberg's 1951 essay as a superior treatment of Durkheim's ethical theory, and indeed it is far more comprehensive in content and detail.

The Agrarian History of England and Wales A.D. 43-1042. Vol. 1, pt. 2. Edited by H. R. P. Finberg. New York: Cambridge University Press, 1972. Pp. xviii+566. \$37.50.

George C. Homans

Harvard University

This volume—in theory but not in fact only part of a volume—is the second to appear of a projected eight-volume *Agrarian History of England and Wales*. The first to appear (1967) was the volume on the period 1500-1640, edited by Joan Thirsk.

The present volume deals with the 1,000-year-long period from the

Roman conquest of Britain to the beginning of the reign of Edward the Confessor, just before the Norman Conquest. It is divided into three parts. The section on Roman Britain is written by S. Applebaum, the section on post-Roman Wales by Glanville R. J. Jones, and the section on Anglo-Saxon England by the editor himself.

Agrarian history is a vast field, for it includes consideration of the physical environment, agricultural technology, and the social organization related to them. It becomes a particularly vast field in periods like the one treated in this volume, when more than nine-tenths of the population made their living directly from the land. It could become a study of nearly every aspect of society. But in the period in question, what agrarian history could be in theory is severely limited in practice by the evidence available. Thus in the almost complete absence of documentary evidence, the agrarian history of Roman Britain must depend heavily on the results of archaeology—not that these are inconsiderable. But this in turn limits the aspects of agrarian history that can be treated with any confidence. For instance, "You can dig up a villa, but you cannot dig up its land-tenure" (p. 39). Only for the end of the Anglo-Saxon period does any appreciable amount of documentary evidence become available, and the interpretation of much of it is in doubt.

It is clear that the Roman conquest of Britain was not simply a military matter. It really did bring a technically higher civilization. In agriculture it brought advances both in technology and in organization. The balance shifted away from pastoralism and toward a cereal economy. Several new fruits and trees were introduced, notably the cherry and the chestnut. The Roman *villa*, with its fields around it, seems to have been the social form through which the advances were made, though we know little of the legal and other relations between the villa owner and his dependent peasants. But the villa was common only in the lowlands of the southeast. Elsewhere the older pastoralism and tribal organization of the Celtic peoples must have persisted.

The author of this section tends to lose the big picture in the archaeological details, particularly those concerning the forms of houses and barns. He extrapolates overconfidently from physical finds to social organization. And he seriously exaggerates the institutional continuities between Roman Britain and the society of the Anglo-Saxon invaders. For instance, on pages 92–93 he presents a map of a medieval open-field village in which he claims to see traces of a Roman field division into uniform squares (centuriation). All I can say is that I do not see what he sees. The Anglo-Saxons really destroyed Roman Britain institutionally—which does not mean that all the new crops and fruits were lost.

The editor himself is the author of the section on Anglo-Saxon England. He spends rather less space on agricultural technology and archaeology and rather more on agrarian social structure than does the author of the section on Roman Britain, in keeping with the fact that the increasing documentary evidence from this later period allows us to speak with increasing confidence on the latter subject. What he says is sound and

comprehensive but does not tell us much that is essentially different from what we could learn from other standard texts on Anglo-Saxon society.

One of the great issues in this field is whether the Anglo-Saxon invaders brought with them to England the so-called open-field system of communal agriculture as part of their cultural baggage. In the endeavor to show that they did not, but that it developed later, the author argues that a famous 7th-century passage from the laws of Ine, king of Wessex, need not refer to open-field arrangements. But the rules that the passage refers to can be shown to have been observed at a later date in open-field villages; and the high degree of similarity in institutions and agricultural terminology between the English open field and that of northwestern Germany suggests that the invaders did indeed bring the system with them—which does not in the least mean that a similar system could not have been introduced still earlier, perhaps by the pre-Roman Belgic invaders.

The third section, on post-Roman Wales, perhaps breaks the most unfamiliar ground. After the departure of the legions, Wales certainly reverted to a Celtic tribalism based largely but never wholly on pastoralism—not that it had ever, as a mountainous backwater, been thoroughly Romanized. Here the author produces the best discussion of that very difficult subject, the relationship between Welsh kinship, settlement patterns, and land use, that I have yet encountered. Anthropologists tend to assume that a society organized in lineages cannot also be organized in bilateral descent groups. It looks more and more as if Wales, and perhaps other Celtic societies, possessed institutions of both types: a landholding lineage of four generations in depth, the *gwely*, and a more comprehensive and more loosely organized local, bilateral, descent group, the *cededl*.

The Language of Sociology. By Charles W. Lachenmeyer. New York: Columbia University Press, 1971. Pp. vii+129. \$7.50.

Edward A. Tiryakian

Duke University

What do you do as a budding author when a grandiose paper of yours has been rejected by a journal editor on the grounds of lack of empirical support? One adaptive response to this common vexing experience (which may also be seen as a *rite de passage* into the profession) is to write a book with an equally grandiose scope. Such seems to be the case at hand.

Charles Lachenmeyer devotes the four chapters of *The Language of Sociology* to hammering out the thesis that "sociology is not a science because its theory language is not adequate" (p. 1). On what rests this inadequacy? Essentially, he argues, it rests on the core language of sociology being riddled with ambiguity, vagueness, opacity, and contradiction. What will remedy the situation? Basically, he proposes at the conclusion, on the one hand sociologists must use "explicit and rigorous definitions" which indicate concern with the referential meanings of core terms, and,

on the other, sociology must adopt a new research strategy oriented to "observable phenomena and with explicit spatiotemporal referents, rather than with reports about these phenomena" (p. 115). This thesis is likely to produce a *déjà vu* (or *déjà lu*) feeling on the part of readers, since the imprecision of the language of sociology has already received extensive discussion. So what else is new about this book?

The inspiration for the author's notions of an adequate theory language seems to come from two sources: analytical philosophy and philosophers of science (e.g., Quine, Carnap, A. Kaplan, and Hempel) and linguists and psycholinguists (e.g., Naess and Chomsky). After a detailed exposition of what, ideally, goes into a scientific language system—an exposition in several places so rigorously and tediously precise as the following: "Abstract object predicates (conditional and unconditional) as superordinate sets of object predicates (conditional and unconditional) whose members are actually subsets of object predicates whose members, in turn, are subsets of these subsets of object predicates, and so on, are further removed from empirical accessibility than object predicates that refer directly to observables" (p. 22)—the author turns his attention to sociology.

Lachenmeyer posits that sociology's core language is more that of conversational language than scientific language because of its lack of (definitional) precision. This shortcoming is reflected in sociology's lack of predictive rigor. Moreover, he adds, sociologists with few exceptions have reified the heuristic distinction between theory language and object language (the language of research into observables). He examines, in his last chapter, recent attempts at systematization and formalization in theory construction, such as H. Zetterberg's deductive axiomatization and H. M. Blalock's mathematical model building of causal analysis. Though these attempts would seem to be in the right direction, they are found gravely wanting because the core terms in each lack definitional precision. Two other approaches which have possible merit insofar as they focus on the desideratum of "observable human behavior," namely, exchange theory and ethnomethodology, are equally deficient in their failure "to assign reliable spatio-temporal referents to verbal phenomena" (p. 114). Conclusion? Sociology is presently (or indefinitely?) *not* a science.

Having indicated the thrust of this book, I will now turn to what I view as its inadequacies. First, for all its precision, it would have been better in condensed form as an article. Second, it would have been fruitful had the author presented us with a detailed examination of another discipline, whether economics, biology, or whatever, whose theory language Lachenmeyer feels *is* adequate for its investigations, using the same criteria by which he condemns the language of sociology. I say this because I have the impression that the model of a theory language the author has in mind reflects more the idealized state of affairs of philosophers of science than actual scientific language(s). Third, if linguists, psycholinguists, and others dealing with communication are doing what is necessary with verbal phenomena, as he indicates (p. 114), it would have been constructive to spend considerable space explicating just what it is they are doing as a model

of what sociologists should be doing, rather than a simple passing mention of this achievement. Finally, for a book which so emphatically stresses definitional rigor, I find a surprising lack of rigor in some key statements. Take the following assertion: "Human behavior is the stuff of every social science: the limited empirical domain that defines the ultimate empirical utility of every theoretical statement in social sciences" (p. 65). What does the author mean by "human behavior," "stuff," "ultimate empirical utility"? And the call for sociologists to "deal with observable phenomena and with explicit spatio-temporal referents" (p. 115) also has similar difficulties: what is precisely meant by "observable phenomena" and "spatio-temporal referents"? Like most sociologists, I may not be able to give a linguistically precise definition of our subject matter, but I think a lot of what we do and theorize about would be excluded by the author's statement that "verbal behavior is *the* social reality" (p. 69); this position implies the exclusion of structural variables and seems to veer toward an extreme nominalist and behaviorist position.

All this is not to say that the language of sociology is what it should be at this stage of development, but then, neither is *The Language of Sociology*.

Concepts and Society. By I. C. Jarvie. London: Routledge & Kegan Paul, 1972. Pp. xiv+214. \$8.25.

Richard G. Dumont

Bates College

Concepts and Society is part of the Sociology and Philosophy Series of the *International Library of Sociology*, and it is written by a philosopher whose primary interests have led him into that nebulous-lucid and intellectually exciting border stream between philosophy and sociology. The notions and insights advanced and developed in this monograph are wide ranging, stimulating, and controversial, involving and integrating key issues in and aspects of theory, methodology, the sociology of knowledge, and metaphysics. The latter two areas in particular contribute most notably to a universe of discourse which allows the author to develop a sophisticated approach to such perennially problematic questions as those of ontology, explanation, and understanding in the social sciences.

There are two major problems which set the tone for and give direction to Jarvie's work: "One is how social change is to be explained. The other is the extent to which beliefs can explain action" (p. ix). He argues that Karl Popper's philosophy of the social sciences is vital because it reveals linkages between these problems as well as suggests strategies of inquiry in pursuit of their resolution, linkages, and strategies which are represented only as lacunae in the more traditional orientations.

Part 1, "Preliminaries," deals with the complex issues of explanation and understanding. In chapter 1, "The Logic of the Situation," Popper's model

for explanation in the social sciences is laid bare and its features and implications discussed in detail, such exposition being made more intelligible through the use of well-chosen illustrations. Chapter 2, "Understanding and Explaining in the Social Sciences," tackles the thorny issues surrounding the explanation and understanding of aspects of alien cultures and societies (e.g., Zande magic). Jarvie takes to task relativist doctrines, especially that variant of relativism advanced recently by Peter Winch, which essentially precludes cross-cultural explanations and understanding.

Part 2, "Case Studies," represents an attempt to illustrate how beliefs affect actions and to demonstrate the power of situational logic in the explanation of the unintended consequences of actions. In chapter 3, "Between Adult and Child . . .," that phenomenon popularly known as "the generation gap" is considered. Chapter 4, "The Idea of Social Class," examines critically popular and sociological models and theories of class. Sociological explanations of class and class-related behaviors are revealed to be not-so-sophisticated versions or variants of popular conceptions, both being based upon and perpetuating false beliefs.

Part 3, "Concepts and Society," considers the sociology of knowledge and select ontological questions more directly. Jarvie's special concern in chapter 5 is with summarizing briefly, criticizing, and modifying not only Mannheim's contribution, but especially the more recent work of P. Berger and T. Luckmann (*The Social Construction of Reality* [London: Allen Lane, 1966]). In the final chapter, Jarvie attempts to show the importance and special relevance of certain of Popper's recent contributions to metaphysics. In particular, he explicates and investigates some of the consequences of Popper's ontology, which conceives of the world as consisting of three realities: "the first is the physical world or the world of physical states; the second is the mental world or the world of mental states; and the third is the world of intelligibles, or of *ideas in the objective sense*; it is the world of possible objects of thought" (p. 151; from Karl R. Popper, "On the Theory of the Objective Mind," *Proceedings of the 14th International Congress of Philosophy* [1968], 1:25-53). Jarvie argues that this philosophical idea allows the sociologist to overcome many of the problems inherent in extreme objectivist and subjectivist approaches and in the varieties of ontological monism. He concludes by showing how ontological pluralism is compatible with a sophisticated and enlightened version of methodological individualism. Together they provide a foundation for the emergence of a sociology and a sociology of knowledge with enhanced potential for the explanation and understanding of social action, social stability, and social change.

This book is recommended for all sociologists, but especially for those among us who seldom have occasion to reflect seriously upon the epistemological and metaphysical assumptions which give direction to our work. Most of us will find both major and minor areas of disagreement, however. For example, those of us inclined toward positivism will probably find Jarvie too "subjective." We may criticize him for failing to distinguish clearly between the contexts of discovery and justification and particularly

for his failure to give more than passing and implicit attention to problems of verification. On the other hand, it is likely that phenomenologists will interpret Jarvie as yielding too much to positivism. Similarly, Marxists, historicists, behaviorists, and the like will no doubt find cogent criticisms derived from their respective points of view. To the extent that we have allowed ourselves to succumb to the all-too-easy tendency of a lazy doctrinal adherence to one approach, however, *Concepts and Society* should make us sufficiently uncomfortable so as to stimulate a fresh sense of uncertainty and perhaps insights into that ever-changing and ever-elusive reality that we call the social.

Professions and Professionalization. Edited by J. A. Jackson. Cambridge: Cambridge University Press, 1971. Pp. vii+226. \$10.00.

Wilbert E. Moore

University of Denver

In the conventional semantics of academic publishing, this volume is a symposium, not a reader: the six essays that comprise the book were especially written for the occasion. The result is not as comprehensive or systematic as one would expect from a textbook or reader, but rather a curious mixture of broad topical treatments and much more specialized analyses. The first chapter, by the editor and announced as an editorial introduction, usefully raises some conceptual problems—defining and differentiating the professions—and some theoretical issues—the rewards and hazards of setting professionalized occupations apart from more mundane economic pursuits. Three other essays address broad topics: “Occupations and Professions” by C. Turner and M. N. Hodge; “Professionals in Organizations” by G. Harries-Jenkins; and “Critical Notes on Sociological Studies of Professional Socialization” by V. Olesen and E. W. Whittaker. The other two essays focus on a specific service-rendering organization (“Professions or Self-perpetuating Systems? Changes in the French University-Hospital System” by H. Jamous and B. Peloaille) and a specific occupation (“Teaching as a Profession” by T. Leggatt).

The most ambitious analysis is that of Harries-Jenkins, who, has rather less to say about professionals in organizations than might be supposed from the title of his essay and a great deal to say about professionalization. He distinguishes (pp. 58–59) six elements of professionalization (structural, contextual, activity, educational, ideological, and behavioral), each divided into two to five subelements for a total of 21. Then, so help me, the author proceeds to illustrate virtually every one of the 21. These elements and subelements, though used to analyze professionalization (that is, the way an occupation improves its position on a scale of professionalism), could equally well be regarded as an elaborate enumerative definition of professionalism. The author, in fact, uses them in both ways. He neither grades his criteria nor discusses any possibility of their being sequential in

the course of professionalization. Rather, he suggests that professions be compared by "multivariate analysis," though it is not clear what significant propositions would thereby be revealed.

The problem of defining professions also troubles Turner and Hodge, and their solution is to suggest a set of activities, 11 in this instance, useful for describing occupations generally. These activities include such items as development of theory, development of practical techniques, transmission of each, regulation of market conditions, and promotion of standards of practice. Now these activities are suggested as appropriate to "mapping" any occupation—a concept that authors also have difficulty in defining—but that intellectual enterprise apparently yields "understanding," which in this case (as in most others where authors state understanding as a goal) yields no predictive allegations or relationships, that is, propositions.

I am selecting essays for comment. What else can one do in reviewing a nonbook? My final selection is the volume's final essay, by Olesen and Whittaker. I got one clear and significant thought from this 41-page exercise in conceptual futility: that professionalization and professional socialization are theoretically related because the occupation may be changing. Otherwise, pages and pages of contorted and involuted prose on questions of conceptual adequacy, without a clear delineation of those theoretical problems for which adequate concepts would be adequate.

The authors of these essays did not have access to such sources as my book (Wilbert E. Moore, *The Professions: Roles and Rules* [New York: Russell Sage Foundation, 1970]), or Eliot Freidson's *Profession of Medicine* (New York: Dodd, Mead & Co., 1970), which was published later, both of which may have been published about the same time as the Jackson symposium. I cherish the fond belief that the useful descriptive materials and analytical distinctions available in the Jackson volume would have amounted to more had the authors hit upon some of the theoretical contributions of those other books.

Organizations in Theory and Practice. By Cyril Sofer. New York: Basic Books, 1972. Pp. xxii+450. \$12.50.

Thomas M. Lodahl
Cornell University

Sofer's account of organization theory is not yet the definitive treatment that many of us as teachers have been wishing for, but this probably reflects the state of disagreement over the contents of the field more than any failure on Sofer's part. He has written a fresh and highly personal text, based on his lectures on organizational sociology at Cambridge and spiced throughout with observations from his years as a researcher-consultant with the Acton Society Trust and with Tavistock.

In his preface, Sofer begins by describing the latter experiences in some detail, building a context against which the reader can evaluate some of

his concerns and emphases as they later emerge. This section is a concrete demonstration of the view that perceptions and ideas about organizations are shaped by personal experiences and establishes the notion for the reader that understanding organizations is as much a human as it is a scientific business.

The introductory chapter lays out, basically, a Weberian approach, updated to the mid-20th century; for me, it is too sketchy on the division of labor and the relationship of organizations to society. He emphasizes the enormous and pervasive power of organizations over many aspects of work and personal life and clearly sees changes in the bureaucratization of society since 1900—the emergence of a powerful managerial class, whose proportions have grown five- or sixfold in 70 years. One wishes he had said more in this chapter about the role of occupations and careers in organization life, but then most writers ignore this topic altogether.

The historical section, which represents a great deal of original work on Sofer's part, is a major contribution and is what sets this book apart from others in its class. The treatment of Taylor is thorough—perhaps the best in the available literature—but his critique is disappointing in that the criticisms tend to be the standard ones. Sofer misses the opportunity here to relate the human assumptions of Taylorism, which are well described, to the modern-day assumptions of operations research, which shares a part 2 Tripos at Cambridge with organizational sociology and economics.

There is an interesting account of early British industrial psychology (especially C. S. Myers and the NIIP) and an equally good treatment of early American industrial psychology. Both accounts stop about 1935; too bad, but the chapter as it stands is valid—perhaps not that much progress has been made since then. Sofer brings Mayo's history and influence into the modern context with the work of Goldthorpe et al., but ignores the Michigan work with groups, which is a severe gap. Lewin's work is well described, with a brief sketch on later influences (T-groups, other managerial training).

The chapters on "classic" experiments are adequate descriptions of some of the Sherif's work and that of Asch and Milgram, but there is relatively little material relating the lab experiments to organizational data or experience, although mention is made of Shepard's account of the Esso inter-group labs.

In the "Principles" chapter Sofer does a major service in identifying the work of Lee and of Sheldon, both British writers of the twenties who are probably unfamiliar to U.S. audiences. Lee's thinking sounds remarkably like that of M. P. Follett, who is also treated in chapter 9. Sofer misses what many would consider the central contribution of Barnard (the theory of authority), but presents a brilliant integrative review of decision-making theory through about 1964. The historical section is concluded by useful and straightforward accounts of the Army and Tavistock work during the war.

Chapter 12—"Propositions"—which Sofer did at Stanley Udy's suggestion, is very disappointing. The propositions are largely a rehash of

accumulated wisdom in the form of existence statements—for example, “Each role (in a work system) has a rank relative to each other, and a specified area of jurisdiction and carries with it a specified amount of authority over persons and resources.” Unfortunately, the propositions do not grow in any organic way from the preceding history nor the subsequent substantive chapters. Because of this they are less impressive as summaries and not as useful as mnemonic devices as they might be.

Taken as a group, the last seven chapters of the book, while uneven, form an exciting and original contribution. Part 5 of the book—two chapters on “Bureaucracies as Working Communities”—has an excellent chapter integrating bureaucratic theory with two case studies from Sofer’s practice: one a study of bureaucratizing a family firm, and the other an interview study of scientists in organizations. By comparison, the next chapter—“Decision-making in a Bureaucracy”—is disappointing. It is a case study of conference planning not at all well suited for integration with the excellent earlier treatment of decision making.

The final section of the book begins with a straightforward chapter on leadership which gives a reasonably clear treatment of the major issues. Chapter 16, on organizational change, is an excellent summary of the relevant literature, focusing on change in flexibility, centralization, and lateral distribution of power. But it makes no mention at all of the current research on these dimensions now available from comparative organizational research. Chapter 17, on union-management conflict, gives some excellent new British case material. Chapter 18, on conflict between colleague groups, is very original, but makes no use of the Sherif material on intergroup conflict presented earlier. Sofer also makes some excellent points here about secondary socialization, but they need a case study. The material on managerial intervention in crisis is new and excellent. The case studies of win-lose competition could easily be integrated with the Shepard material given earlier.

The final chapter, “Professional Interventions in Conflict and Change,” gives some excellent new thinking but ignores the recent contributions of the Bennis-edited Addison-Wesley series on organizational development. In this chapter Sofer also raises, but does not answer, some important questions on who benefits from “social consultancy.” In closing, he makes a strong plea for “sustained methods” of organizational research—namely, extensive and intensive case studies over long periods of time.

In my judgment, this book will appeal to and be useful to practicing managers as well as students. It is attractive in its strong experiential base and easy personal style (except for chap. 12). Its main scholarly contribution is in its excellent and often original historical treatment, better than any other general organizational theory text I know. Here, the most serious gap is its omission of the Michigan work of Likert and his colleagues. Another gap is the failure of the book to deal with the now burgeoning literature on comparative organizational research, which seems to be the main direction neo-Weberian work is now taking. A teacher hoping to cover this field would want his students to read Richard Hall, Charles

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Perrow, or James Thompson in addition to this book. But these faults could be remedied in a second edition, which would hopefully retain the disciplined structural-sociological approach combined with the easy, personal, perceptive social-psychologist eyes that Sofer shares with us.

Technology, Power and Social Change. Edited by Charles A. Thrall and Jerold M. Starr. Lexington, Mass.: D. C. Heath & Co., 1972. Pp. v+169. \$9.50.

Bryce Ryan

University of Miami

This volume is the edited transcript of a conference organized by University of Pennsylvania graduate students wherein well-known social scientists and humanists discussed contemporary social issues related to technology. The main symposia were devoted, respectively, to: "Bureaucracy, Centralization and Decentralization," "Technology and Authority," "Technology and the Counterculture," and "Technology and Humanism." The conference was keynoted by addresses from Lewis Mumford and Robert Theobald. The present volume includes these addresses, transcripts of the opening statement or paper by each participant in the main symposia, and the ensuing discussions. Eight additional small symposia are reported upon in a brief summary chapter. The book is concluded by the editors' summary of the conference.

The conference keynoters leave no doubt as to a state of crisis in our society stemming from man's relationship to technology. Mumford has lost none of his eloquence as he recounts the history of man's slavish tribute to the "myth of the machine," for example, "Science Discovers, Technology Executes, Man Conforms." He argues convincingly that subservience to technology and its associated power complex is uniquely dangerous in our contemporary societal structure. Theobald's opener is addressed more to our hopes for the future. Discounting neo-Luddites and Consciousness III romanticism, he offers some evidence and considerable faith that we are turning the corner toward salvation. With Mumford laying out the roots and qualities of the crisis, Theobald implies that he knows how to lick it. Perhaps one who has read Theobald and Turner's "survival scenario," *Teg's 1994*, which is well cited here, will be more impressed.

The other contributions vary immensely in their scholarly preparation, credibility, and professional orientations. Victor Ferkiss, Murray Bookchin, and Max Lerner address themselves to bureaucracy, centralization, and decentralization. Ferkiss briefly and cogently foresees new options in these spheres to be opened up by technological advances. Bookchin completely denies the social value of hierarchy, property, and centralization and urges the necessity of his utopia, wherein decentralized, small communities of sovereign individuals will live in balance with the ecosystem. Lerner is less demanding, or at least less specific, in requiring of us a new world view

wherein the radical humanist tradition is so internalized that we can live with our technologies rather than by them.

The technology-authority symposium brings us thoughts from an industrial engineer, Seymour Melman, and sociologists, Melvin Tumin and E. Digby Baltzell. Melman attacks the mystique of technology wherein man is visualized as a creature of his techniques. Tumin offers a paradigm for the analysis of change wherein technology is soundly fitted into its sociocultural context. He then goes on to outline the intellectual bases of contemporary concern with "quality of life." Baltzell contrasts social values in the United States and Britain to show the saving grace of traditionalism in the latter country as it persistently qualifies the pursuit of rational, secular goals.

The section on technology and the counterculture is saved from disaster by Seymour Martin Lipset and self-styled "planetary enzyme," Ira Einhorn. Lipset's paper is a coherent, analytical essay which should take some of the steam out of counterculture enthusiasts as he puts the "new" youth in sober, historical perspective. Einhorn's contribution is an ecstatic, mod, blank-verse poem (?) set up in a startling array of contrasting type sizes. Einhorn's explosion actually expresses the major theme of the conference with some clarity and great impact as he inveighs against man's ignorant, wicked, and destructive use of technology and, with perceptive warnings, cheers us on to "focussed pre-awareness" of our dangers as we move toward transformation and karuna.

The final major symposium finds William Boguslaw emphasizing the fact it is men not machines who enslave or destroy other men, and here he picks up an earlier emphasis by Tumin that technology is to be understood as but one element or force in complex social change processes. Melvin Kranzberg is concerned that technology be turned toward human want satisfaction, but he does not go much beyond suggesting democratic decision making as a means toward this end. Charles R. Dechert is interested in utilizing technical advancements in such ways as to rebuild personalized relationships. If not very convincing that this can and will be done, he at least stimulates some thinking along this avenue. The last presentation is by Ashley Montagu, and, while brief, is expanded upon in the following discussion. Montagu says that man began to become dehumanized with the advent of agriculture, but there is some hope that he may again learn to live with and love all life, the key to a viable future. The editor's review of the minor symposia on special phases of technological impact is a review of topics discussed with a potpourri of observations ranging from the banal to the startling. A representative of the Aquarian Research Foundation, for example, told his symposium on Psychic Research, Social Change, and the New Age the advantages of thought control, mind reading, controlled dreaming, and scientific astrology and "heralded the rise of a new Era, forecast in the Bible, in which there would be peace, communal living, a sexual revolution, and women's liberation."

A book of this sort defies overall assessment including as it does a limited amount of well-organized analysis, somewhat more meandering

thoughts, plus high-minded exhortations and dashes of speculative futurism. It might be reasonably concluded that what was undoubtedly a popular, and, to many, a stimulating series of discussions does not add up to a major contribution to the sociology of change.

Class and Politics in the United States. By Richard F. Hamilton. New York: John Wiley & Sons, 1972. Pp. ix+589. \$14.95.

Jonathan M. Wiener

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In *Class and Politics in the United States*, Richard Hamilton has written the most convincing and sustained critique of mainstream American political sociology, particularly of S. M. Lipset's *Political Man*, that has yet appeared. Hamilton's alternative conception is based on secondary analysis of the Survey Research Center's (SRC) election studies from 1952, 1956, and 1964, with some use of the 1968 study. His concern is not with analyzing election results, but rather with using the opinion polls to identify "basic social structural characteristics and relatively stable issue orientations" (p. 12).

Hamilton rejects both the pluralist and mass-society theories of American society. Politics in the United States consist of neither competition between equally representative and powerful veto groups nor elite manipulation of passive and unorganized masses. Instead, he argues the "group politics" theory, which sees political attitudes passed on by social groups with stable political traditions. Following V. O. Key, Hamilton makes an impressive case for the political rationality of these groups. He shows that there is a persistent and unsatisfied demand in the working and lower-middle classes for an increase in federal welfare services, particularly for health insurance and a job guarantee. The principal source of opposition is the white, Protestant, upper-middle class.

Hamilton finds that the distinction between manual and nonmanual labor, generally assumed to be the fundamental cleavage in attitudes and life-styles in American society, is not particularly important for either party identification or issue orientation. The lower-middle class is as democratic and as liberal as the working class. The biggest break in attitudes and party identification comes not between working class and middle class, but between lower- and upper-middle classes. This is perhaps the most significant finding in the book. Because the lower-middle class is as democratic as the working class, upward social mobility out of the ranks of the manual workers has few political consequences. Moreover, the evidence shows that the better-off working-class "aristocracy" is as liberal and Democratic as the less-well-off workers.

Hamilton argues that there has been relatively little class polarization in American politics because the upper-middle class has divided along religious lines. The upper-middle-class, white, Protestants have cut them-

selves off from the rising non-Protestant groups, creating a situation in which high-status Catholics, Jews, and blacks have allied politically—in the Democratic party—with working-class groups of the same religion and race.

An entire chapter is devoted to an evaluation of the “working-class authoritarianism” thesis, Lipset’s argument that the working class provides the mass support for movements that threaten civil rights and liberties and ultimately the existence of democracy itself. Hamilton presents evidence that the classes outside the South do not differ in their “tolerance” of blacks, measured in terms of opinions on federal action to assure equal employment opportunities, school integration, and open housing; substantial majorities exist in both working and middle classes in favor of each. What evidence there is of greater working-class racism is restricted to the South.

He admits there is evidence of less working-class support for civil liberties for Communists, particularly on the polls concerning Joseph McCarthy. But he points to Michael Rogin’s argument that the electoral support for McCarthy came predominantly from the upper-middle class. Disproportionate representation of conservative Southerners and former farmers in the working class also contributes to the evidence of greater working-class opposition to civil liberties for Communists. Thus, Hamilton argues, what evidence there is of working-class intolerance on civil liberties questions seems to be a consequence of situational factors, rather than the supposedly “authoritarian” character of working-class culture and family life, as Lipset argues.

Hamilton is critical of liberal and left intellectuals who are hostile toward opinion polls, suggesting that they prefer to “base their judgments on free and unhindered intuitions rather than to suffer the constraints imposed by evidence” (p. 523). But he does not examine the character of his own opinion-poll evidence. What, for instance, should we make of the answers to Samuel Stouffer’s famous question, “What kinds of things do you worry about most?” Certainly people are more willing to list some “things” than others to a well-dressed stranger standing at their door. Exactly how the polling situation and questions influence people’s answers is difficult to evaluate, but simply presenting the answers is insufficient.

Some will object that Hamilton’s reliance on the 1964 Johnson-Goldwater election poll exaggerates the extent of support for liberal politics in American society, since the Republicans did worse that year than any other time in recent history. Hamilton replies, first, that the election is particularly important because it presented the basic political alternatives clearly, and second, that party identification and issue orientation—as distinct from presidential vote—remained surprisingly stable throughout the fifties and sixties. He presents some 1968 SRC evidence that supports his line of reasoning; it shows that there was no difference between the working class and middle class on support for Wallace outside the South, and that there was much greater support for a negotiated end to the Vietnam war in the working class.

Others will object that the 1972 Nixon-McGovern election disproves the thesis of the book. However, Hamilton's argument is not that the Democrats will always hold onto their working-class base, but only that the Democrats will lose if they turn away from bread-and-butter welfare politics to issues of high-minded morality and distant foreign policy questions, as they did with Stevenson in 1952 and 1956. He could also point to evidence that, in spite of substantial working-class defections to Nixon, the electoral support for McGovern's kind of liberalism came overwhelmingly from the working class.

In short, the book will be an outstanding candidate for assignment in courses on political sociology, American society, and public opinion, either alongside or instead of Lipset's *Political Man*—but not until Wiley's high-priced hardcover (\$14.95) comes out in a paperback edition.

Class, Conflict, and Mobility. By Joseph Lopreato and Lawrence E. Hazelrigg. San Francisco: Chandler Publishing Co., 1972. Pp. xix+576. \$12.50.

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This work is a strange mixture of a number of things which do not make a book. Some of the chapters appeared previously as articles. These, as well as many others, are typical in content and style of articles that are found in sociological journals. Their data were obtained from a survey study, funded by four American agencies, and conducted in Italy. As one plows through the 535 pages of text, one cannot help but wonder for whom and for what this book was intended. Part 1 (93 pages) has the quality of a textbook: it offers a standard rendition of leading theories of class structure. (There is a separate chapter for each theory: that of Marx, Weber, Pareto, Dahrendorf, and Kingsley Davis-Wilbert Moore.) However, when I reached part 2 of the book, I realized that the book is too limited to be used as a textbook. (Its empirical materials consist of data from only one survey conducted in one country, Italy.) If so, then it was superfluous to include part 1 in the overstuffed book, the bulk of which is in the nature of a monograph. Surely, anyone who is equipped to tackle such material should know at least as much theory as is presented in part 1. And if he did not, he could easily make himself available of such knowledge in a standard stratification textbook or reader. As for the authors of this book, it was not necessary to present these extensive summaries of leading theories in order to realize its goal: "To apply to a particular body of data some of the insights and propositions generated in those theories" (p. xviii).

Aside from part 1, the book is overfilled with detailed statistical information, difficult to digest and retain, which was obtained from the survey. It certainly could have benefited from some selectivity in its presentation. Had the authors focused on certain findings, the exposition could have

been deepened and made more interesting. For example, in presenting their findings bearing on regional differences in class consciousness in Italy, the authors point to regional loyalties as possible obstacles to the development of class consciousness. But this thought-provoking topic of Italian regional differences—comparable to ethnic differences in multiethnic societies—is disposed of in less than a page.

I thus covered 400 pages of cumbersome and detailed material from an Italian survey, contained in 11 chapters (part 2, five chapters; part 3, two chapters; part 4, four chapters). Noting that part 5 consisted only of one chapter, 34 pages long, I approached it with fresh hope. Regretfully, I was soon taken aback by the authors' declaration that the aim of this chapter was "to assess the plausibility of limited aspects of Dahrendorf's theory at a point in time by broaching certain perceptions and attitudes that are intrinsically connected to the basic issues of the theory" (p. 505). I was taken aback because the authors had a chapter devoted to Dahrendorf's theory at the beginning of the book, and I was puzzled why they returned to this theory in the last chapter. It soon became clear that at least part of the reason was the great esteem in which the authors hold Dahrendorf and his theory. In the first sentence of the chapter they designate his theory as a "rich formulation that was intended as a rectification and 'supersedure' of Marx's theory" (p. 505). Their enthusiasm mounts steadily, and in the second paragraph they tell us that "Dahrendorf's work represents an intriguing intellectual cross between the brilliant German master of class analysis (Marx) and the great Italian cynical observer of the political processes (Pareto)" (pp. 505–6). Although they consider him great, they nevertheless do not heed Dahrendorf's view that survey data do not constitute evidence concerning the validity of his theory. They do remind us of this view by Dahrendorf but then proceed to use their Italian survey data to test his theory. However, they succeed in comforting themselves, if not us, by pointing out that "despite Dahrendorf's aversion to survey data as evidence of the validity of his theory, he finds it quite convenient to 'illustrate some points' of his theory with precisely that kind of data" (p. 513). Since when do the same rules governing illustration apply to testing validity, or did the authors mean to imply something by putting the words "illustrate some points" in quotes?—I could not help but wonder. Still, I went on reading and to my surprise discovered that their survey data cast doubt on Dahrendorf's theory. In the authors' words, "Our most considered doubt about the validity of his theory concerns the finding that internally directed conflict *within* the command class, totally unaccounted for by Dahrendorf, is often more pronounced than conflict between two classes. To this extent, his revamped model of class conflict leaves something to be desired" (p. 518). Their data also failed to support Dahrendorf's theory about the frequency of dichotomous class images of society in the different classes. Still, these findings did not succeed in deflating the authors' enthusiasm, as testified by the rhapsodic last paragraph of their book: "When all is said and done, no intellectual praise is sufficient for Dahrendorf's work, our above strictures notwithstanding. In

the midst of the current tendency in studies of social stratification to lack explicit awareness of the continuing relevance of Marx' great synthesis, Dahrendorf's work hits our scene as a veritable breath of fresh air" (p. 535).

To conclude by borrowing some of the above words, when all is said and done, I have little intellectual praise for this book. However, should you have much time on hand, read it, and you might succeed in fishing out from the heap of empirical details a few nuggets which might connect with some ideas that you are concerned with.

The American Way of Violence. By Alphonso Pinkney. New York: Random House, 1972. Pp. xiv+235. \$7.95.

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Professor Pinkney asserts that his is not a "balanced" account but, on the contrary, one which seeks to correct the imbalance in most works on violence in America which give too much weight to property destruction and not enough to the destruction of human beings. While making appropriate distinctions between individual and institutional violence, overt and covert violence, Pinkney argues, with depressingly abundant documentation, that the United States is one of the most violent societies ever to exist. His major thesis, with which a humane reader can scarcely disagree, is that "violence will continue to thrive so long as America's social institutions are organized around values which lead to the debasement of human beings" and "private property tends actually to be esteemed over human life" (pp. xii-xiv).

Pinkney accomplishes his task brilliantly. Not all aspects of violence in America are covered, but the most important ones are. His separate chapters on "International Relations" (Vietnam), "Persecution of Black and Red People," "Police Violence," and "Cultural Supports for Violence," are lucidly written, cogently argued, well documented. No study on violence can be taken seriously until it confronts the arguments raised by Pinkney. For students in introductory sociology courses, especially, this book represents a good starting point. Pinkney's summary of the history of violence against Indians and blacks focuses upon precisely what most standardized, mass-distributed textbooks on the subjects omit or underestimate.

This is the first book I can remember reading where the American people as a whole (with the exception of dedicated agents of social change) are held responsible for the American way of violence: "The citizens of this country, like their leaders, have little regard for human life and human dignity" (p. 164). Pinkney's courage in confronting this disturbing reality leads him to an essentially "classless" analysis of American violence, one which fixes upon the pervasive culture of violence and a value system which rewards acts of violence while placing property values above human ones. There is little investigation or analysis of the material and historical basis

for this value system. Pinkney writes off the importance of "social class" as merely another force alleged to affect violence but in fact as irrelevant as "race or ethnicity, age, geographical location" (pp. 202-3). The major force, after all, consists of "the dominant values of the society as a whole" (p. 202).

But where do these values come from if not from the "ruling class," whose role in violence Pinkney notes elsewhere, if only in passing? (pp. 86, 93, 118, *passim*). And on what material basis is this ruling class founded, if not on "our capitalist system," as Pinkney finally pins it down on page 182? That passage, in fact, represents the best reasoning in the book and merits quotation in full: "Since the demand for profits is the *sine qua non* of our capitalist system, the destruction of banks or other real property strikes at the very foundation of the system, and individuals involved in these acts are dealt with swiftly and harshly. On the other hand, the killing of individuals who advocate social change strengthens rather than weakens the system." Or similarly, on page 140: "While vigorously proclaiming political neutrality, colleges and universities have steadfastly supported American imperialism at home and abroad, ignoring the wishes and needs of students . . . [whose] demands for reform, backed by demonstrations and the occupation of buildings, force the agents of the existing order . . . to summon police and National Guardsmen to use whatever force is necessary to contain the rebellious students." Southern University students take note.

Such straight talk is this slim volume's strength, while at the same time its underlying weakness, since it occurs without adequate analysis of underlying sociological causes. Some profoundly disturbing questions could be asked by sociologists once the truths raised in this book are confronted. For example, to what extent is American violence practiced on Vietnamese civilians and schoolchildren premeditated, calculated, and deliberate? Why do newspapers like the *New York Times* contribute to the climate of "peace is at hand," a climate which repeatedly coincides with ever higher levels of violence in the Vietnam war? What additional types of violence are practiced on American children, besides the physical ones the author documents, such as subtle psychological ones which can in turn generate subsequent violent behavior on the part of the children so victimized? What about violence imposed on America's old people? Or the violence imposed upon America's supposedly "mentally ill"? Or what about labor "accidents," as they are euphemistically called, from which over half of America's work force emerges maimed, crippled, hospitalized, killed, or laid off by age 50? Or the sheer mental violence and anguish which accompanies everyday work alienation in our society? And what about the complicated intersection of individual psychological responses of the oppressed, the victims of ruling-class violence, and outright social-class war, during which the oppressed take up arms or resort to other violent countermeasures? To what extent do the weapons of violence themselves assume an autonomous irreversible force of their own in social events (B 52's are used because they exist)?

In brief, Pinkney glimpses, without adequately explaining or analyzing, the underlying social-material basis of American violence. Nevertheless, he makes an excellent case for his cultural explanation of the phenomenon, even as he does for the autonomous causal force of racism which is so closely linked to the American way of violence. A good book—but a tough one to read during Nixon's bombing of Hanoi and Haiphong, in the context of which the author's occasional hyperboles appear as understatements.

Busing and Backlash: White against White in a California School District. By Lillian B. Rubin. Berkeley: University of California Press, 1972. Pp. viii+243. \$7.95.

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Though two decades have passed since the Supreme Court's famous school desegregation decision—during which time desegregation controversies have troubled hundreds of cities—remarkably little research attention has been devoted by social scientists to such communities in conflict. Thus a welcome addition to the scanty literature available is Lillian Rubin's *Busing and Backlash*, a historical and ethnographic study. Using field observation and interview techniques, as well as examination of naturally generated organizational data, Rubin reconstructs the critical events surrounding the rise and fall of desegregation proposals in a middle-sized California school district (Richmond, California), concentrating on the years 1966–70.

Much of the book is devoted to a blow-by-blow account of the history of a proposed desegregation plan and the organizations which vied with one another for control of the district's schools; the well-researched descriptive analysis traces out the replacement of a liberal school board with a conservative one and the issues and organizational developments linked to that replacement.

In what is essentially a descriptive case study, Rubin provides empirical materials which might be used, together with other such studies, to develop new generalizations about desegregation politics. Moreover, though much of her theoretical analysis is at a rather elementary level, Rubin offers some interesting critical comments on theories which have attempted to explain working-class behavior. Drawing out the implications of her own research, she argues that the revolt of the silent majority (seen here as the working and lower-middle classes) cannot be adequately interpreted in terms of conventional theories of status politics, working-class authoritarianism, or "private-regardingness." Rejecting the suggestion of some status politics advocates that the revolt reflects the irrational fantasies and actions of frustrated workers whose wide-ranging demands are impossible to attain, she argues that the revolt in Richmond could easily be viewed as the result of realistic perceptions of the community situation and was essentially rational in character, focused as it was on the short-run self-interest of the

working and lower-middle classes. With regard to arguments about the distinctive "public-regarding" character of upper-middle-class elites, Rubin also points out that data on the Richmond controversy indicate that "the common good for which public-regarding elites allegedly are willing to sacrifice too often is coincident with elite good to be an artifact of chance" (p. 46). This leads her to suggest that the distinction between "public-regardingness" and "private-regardingness" may be irrelevant to analyzing class politics, since in the real world affluent elites rarely pay serious costs. Similarly critical of working-class authoritarianism theories, Rubin provides some documentation for her argument that such theories are too often "fraught with the bias of their middle-class intellectual originators and they have little explanatory force when tested in the empirical world" (p. 51).

While candid about her own liberal bent, Rubin offers an insightful analysis into the administrative and political behavior of the liberal school board prior to its being replaced by a conservative board, probing in some depth the weaknesses of liberal politics and the strengths of conservative politics. In this analysis she raises an interesting policy question for officials acting in regard to desegregation. Thus she suggests that one solution for the liberal board might have been to implement, at the very beginning, a comprehensive desegregation plan and then let the community be faced with a *fait accompli*. Yet this was impossible at that point in time, for the liberal board harbored racial fears of its own and was thus ambivalent in its commitment to integration. This ambivalence led the board to develop a desegregation plan particularly favorable to its upper-middle-class constituents (unfavorable to the working class), to equivocate about busing, and to delay implementing an integration plan. Those dilatory tactics gave the conservatives a chance to organize and play a decisive role in preventing school desegregation.

Yet Rubin does not spend much time assessing the impact of these political shifts on nonwhite groups. The implications of community school conflict in which white working and lower-middle classes gain greater power are nonetheless quite serious, for upper-middle-class white losses in such struggles are not nearly as critical as those of black communities. As Rubin herself notes in a conclusion, this fact points up one of the continuing dilemmas of modern America, "how to protect minorities from majority tyranny" (p. 208).

Migrant in the City: The Life of a Puerto Rican Action Group. By Lloyd Rogler. New York: Basic Books, 1972. Pp. ix+251. \$8.95.

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Migrant in the City fails to deliver on either the implicit promises in the title or the explicit promises in the introductory remarks. Offering a contribution to our understanding of how assimilation may be achieved through

organized, collective action, the author focuses on the Hispanic Confederation—a "Puerto Rican Action Group"—and the relationships of confederation members to each other, to city agencies, to representatives of the outside community. Rogler also proposes "to delineate social change in a small natural group" (p. 7). Unfortunately, he never provides a coherent theoretical framework for his analysis and never defines such key terms as "assimilation," "collective action," "social change," or "natural group." In the end, his book provides a little of something for everyone—a mere taste of honey—which leaves one with a gnawing sensation of hunger.

Rogler describes his attempts to combine a number of methodological approaches, but no aspect of this documentary report is developed fully enough to satisfy a sophisticated researcher. Assisted by two female assistants and a tape recorder fondly named "Bartola," Rogler completed over three years of fieldwork in a middle-sized, northeastern city. He was a participant observed at confederation meetings and at informal group events. In addition, he carried out open-ended, somewhat directed interviews with all regular confederation members, plus less directed, in-depth interviews with principal leaders of the city's Spanish-speaking population.

A wealth of raw materials appears in the book, the bulk of which consists of eight chapters plus an epilogue of detailed, dry, often repetitive narration of group meetings. Interspersed within these chapters are two short autobiographical statements, one by the Puerto Rican political boss and one by the leader of the Hispanic Confederation. In these chapters, Rogler tries to imitate Oscar Lewis's style, but produces instead a rather awkward prose. Finally, there are four chapters on methods and results from his survey of confederation members. His review here of the participant-observer role and its inherent conflicts is interesting, though often tedious and unnecessarily apologetic. It might serve as a reference for others who undertake similar field studies. Rogler's quantitative analysis of survey items is less useful. To begin with, the small number of group members interviewed (approximately 40) limited him to a very simple analysis of the relationships between the personal characteristics of these people and their participation in the confederation. In reporting the results, Rogler fails to recognize important intervening or related characteristics behind the patterns studied. When he views inter- and intragenerational mobility, for example, he does not control for sex of the respondent even though it has been shown that father-son, father-daughter mobility patterns, and individual career lines by sex are quite different (see tables 13-1 and 13-2). Furthermore, he often assumes causal sequences to be evident in simple correlations among three or more variables, where there is no justifiable basis for such assumptions.

More striking than these minor fallacies, however, is the impression gained after reading *Migrant in the City* that the author has grossly exaggerated, or highly overrated, the importance of the group he chose to investigate. The Hispanic Confederation, as reported, included only 42 Spanish-speaking persons in a city with an estimated 4,500 Puerto Ricans. The handful of active members were all part of one extended family! Per-

haps even more critical is the author's determination that it was an "action group." In our estimation, an "action group" is characterized by involvement in political activity, in violent and nonviolent protest, etc. None of this behavior was evident in the history of the Hispanic Confederation. From the outset, the confederation was pledged to stay out of politics (its members also agreed not to pay dues, since previous Puerto Rican organizations had been plagued by intrigue and argument over the handling of funds). Its first achievement was a meeting with the mayor to discuss problems of the Hispanic population, an encounter arranged through a series of letters and phone calls over a period of more than three months. Few confederation members actually helped arrange the meeting and fewer attended. Another confederation achievement was the establishment of a "Hispanic Referral Office" mandated to channel the problems of individual Spanish-speaking clients through appropriate city agencies. After some rather clumsy negotiations with the confederation, this office was supported by the city administration; one of the two referral office staff members was transferred from other employment within the city bureaucracy, and the second was hired from among the confederation membership.

A number of issues which are only mentioned in the book seem more deserving of attention than those discussed at some length. For one, the question of ethnic identity and its relationships to the growth of ethnic group organizations is brushed over in a number of instances. Rogler notes that while all members of the confederation harbored negative, cynical evaluations of their peers in the city, they still participated in the activities of an organization avowedly devoted to improving the lot of these same Puerto Ricans. In the words of the female mainstay of the confederation: "I cannot really explain why I am so active (p. 111)." The career of this same key figure, Mrs. Esteban, suggests that the leadership role of women in the Puerto Rican community might be a worthwhile area of study. She comments on "how difficult it is to be a woman . . . but now we have as much power as men" (p. 114).

Rogler might also have provided some insights into the contribution of ethnic group organizations to individual assimilation and/or co-optation of ethnic group leaders. The autobiographical chapters contrast the lives of the classic political boss, Vincente Serrano, and Mrs. Esteban. The similarities between their experiences—the personal gains accruing to the recognized ethnic leader—are not dealt with sufficiently in the book. Serrano, in the tradition of local politicians, was drawn into the Democratic organization with the call, "Vincente, let's get the Puerto Ricans to vote" (p. 63). His cooperation with this organization was rewarded with patronage jobs for his followers and other special favors. Mrs. Esteban's privilege was gained through involvement with the City Poverty Board. Once noted as a community leader and named to the board, she earned a job, free medical and counseling services, and a limited number of positions in city agencies which were distributed among her family. Eventually she was transformed into a defender of the city against the confederation. At one stage she urged her colleagues in the confederation to be less demand-

ing: "You cannot force these people [the City Administration]. We [the confederation members] have to be humble . . . We have to be conforming" (p. 148).

The volume as is may be a useful source of unedited data for other researchers. Hopefully, they can learn from Rogler's work that a broader, theoretical framework is essential to the type of study attempted here. Though the author of the foreword praises Rogler for his lack of direction: "He does not talk about models, theories, or hypotheses and he does not give us a lengthy set of instructions as to where his work fits into the rest of the sociological literature" (p. vii), we would like to see future studies more solidly grounded in the context of other theoretical and empirical efforts. Notwithstanding this criticism, *Migrant in the City* is a welcome change from previous studies of the Puerto Rican community which emphasized social disorganization—family problems, mental illness, welfare, crime, drug use, etc. It is one encouraging sign of a long-overdue redirection of sociological research toward analysis of social and political organization among Spanish-speaking groups in the United States.

Indian Americans: Unity and Diversity. By Murray L. Wax. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971. Pp. xix+236. \$6.40 (cloth); \$2.95 (paper).

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This brief volume, part of Prentice-Hall's series on ethnic groups in American life, is a notable sociological treatment of subject matter long the domain of anthropologists and historians. Murray Wax has synthesized research findings, demographic details, theoretical perspectives, and historical documents in a remarkably readable and informative text. I believe it is the best introduction to the sociology of Indian-white relationships presently available.

The book has three parts: (1) historical developments and comparative relationships; (2) contemporary U.S. tribal communities; and (3) Indians and the greater society. The first contains chapters on historical and ecological background, demography, and Indian-white relationships. The second part illustrates modern tribal communities in extensive discussions of Plains Indians and of the Oklahoma Cherokee. The final section has chapters on pan-Indianism, urban Indians, and the interplay between Indian identity, ideology, and the "Indian problem."

Wax concentrates on Indians in the United States, yet spans national boundaries enough to provide insights on Indians in Spanish America and Canada. He thereby makes the point that the reading into history of present political boundaries distorts our understanding of Indian people and minimizes the ties of North American Indians with the high civilizations of Central and South America. Although the predominant focus is on

contemporary Indian-Americans, he includes enough history to debunk some prevalent myths. For example, he highlights the fluid nature of Indian societies even before the coming of the whites. He also outlines the historical patterns of ecological succession and emphasizes the social nature of the identity "Indian." Prior to Columbus there were no "Indians": the social identity emerged from the interaction between the invading Europeans and the various native tribes.

The chapter on demography is very impressive. Wax manages to hold the reader's interest while presenting historical data on the Indian population of Middle America, the linguistic definition of Indianness there, and the distribution of Indians in the United States and Canada. Two excellent statistical appendices itemize federal expenditures in the name of Indians for 1968 and 1969 and present current statistics on Indian education, racial isolation in schools, fertility, and mortality.

Wax is a gifted writer, and quotable gems abound. Some samples:

What the missionaries and educators had in mind were not the actualities of the social life from whence their mission was supported. Indians were not to be transformed into replicas of conquistadores or English sailors, but into saintly Christian versions of those ethnic types. Recognizing the depravities of their European countrymen, the missionaries often tried to impose social isolation upon their Indian flock to shelter them from intimacy with such evil exemplars. [P. 177] Were Indian cultures totally to disappear, Indians would not emerge as prosperous middle class Americans but as disorganized and deracinated proletarians. [P. 183] The tragedy of these rural schools is the lack of preparation of their teachers, for they are no worse than teachers elsewhere, and would presumably like to perform their jobs competently. But they do not realize their own ignorance, and, coming from a parochial and constricted rural background, they have no understanding of what it is to be linguistically and culturally alien to the school and its teachers. [P. 122] No business enterprise is quite so subject to the whim of governmental bodies as is an Indian tribe. Nor has any business enterprise or any normal local governmental unit been so subjected to the destructive onslaught of the Army Corps of Engineers and its allies in building dams, altering river courses, and flooding landsites. [P. 193]

The chapter on the Oklahoma Cherokee stems from a federal research project directed by Wax on the relationships between Indians and local public schools. He characterizes the Oklahoma Cherokee as a people largely ignored by academic investigators; highlights their poverty, exploitation, and powerlessness; and notes their position at the bottom of the status structure: "It is the Tribal Cherokee in this region who performs the underpaid, difficult, and degrading labor which is reserved for the Negro in the Deep South and the Chicanos in the Southwest" (p. 94).

Indianness is defined in terms of culture. The essential element is participation in the rural communities where Cherokee is the primary domestic and religious language. Others of Cherokee lineage live like white people and are detached from the lives of the Tribal Cherokee. For

these, "Being Cherokee is expressed through participation in benevolent, cultural, or political associations which are concerned with Cherokee history, or through assisting those whom most consider to be backward and ignorant Indians" (p. 93). In fact, comparison of project findings with the U.S. census reports led Wax to conclude that persons counted as Indians tended to be the impoverished and marginal types, while the upwardly mobile Indians were more apt to be classified as "white."

Wax provides three examples of religious pan-Indianism (the religion of Handsome Lake, the Ghost Dance movements, and the Native American church). Political nationalism is represented in accounts of the origin and growth of the National Congress of American Indians and the National Indian Youth Council. Indian alcoholism and crime are mentioned in the context of pan-Indian reaction to disruption and invasion.

Apart from case studies little is known about urban Indians. Upper- and lower-class people of Indian heritage are particularly difficult to identify and assess, the latter because they move frequently and have high rates of personal disorganization, poverty, and disease, and the former because they often identify as Indians in an heirship sense but not in terms of life-style. Easiest to identify in the cities are the working-class Indians, who tend to participate in Indian institutions and preserve strong attachments to kindred in rural communities or reservations. Wax's treatment of city Indians is limited to four case studies: Barrow, Alaska; the Caughnawagas who work in high steel and have urban colonies in Brooklyn, Detroit, Buffalo, and other cities; the Sioux in Rapid City; and urban Indians in Minneapolis. The extreme adaptability of Indian culture is noted: "Quite contrary to the views of most reformers, the very ethos and culture of the Indians sometimes allows successful adaptations to urban roles" (p. 171).

In the final chapter, five main ideologies of Indian-white relationships are summarized (genocide, innate inferiority, Euroconformity, the melting pot, and cultural pluralism). Continued application of Anglo-Saxon heirship principles is seen as a major source of disruption of Indian communities. Problems of Indian identity are dealt with in terms of three types of Indians. The "socially conservative Indian" remains a part of a tough, flexible Indian community. He may not manifest many "Indian" traits, but Indianness is not a function of specific traits but rather of social relationships. The "white Indian" draws his cues from middle-class America rather than from the Indian band. The "generalized Indian" lacks a firmly defined role as Indian, is detached from local bands of kindred, and may have an intertribal marriage or be the product of an Indian boarding school. For him, relationships to other Indians are in process of creation.

For Wax, there is neither an "Indian problem" nor a white problem, but rather "a set of diverse problems involving the *interrelationships* of Indians and non-Indians in a broad ecological and institutional context" (p. 193). The patterns of interaction between these two populations must be understood if meaningful programs are to be carried out: "Neither suffering nor guilt provide a sufficient basis for analysis and planning"

(p. xiv). The competent scholarship and congenial style of this book clearly meet the author's objective of helping people become familiar with the historical and present realities, that better analysis and planning may be achieved. *Indian Americans: Unity and Diversity* deserves wide acceptance and use.

Communication and Modernization in Urban Slums. By Vishwa Mohan Mishra. New York: Asia Publishing House, 1972. Pp. xiv+128. \$6.95 (cloth); \$4.50 (paper).

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Vishwa Mishra is a conceptual and empirical descendant of Daniel Lerner, Alex Inkeles, Joseph Kahl, and Wilbur Schramm. Unlike the work of his intellectual ancestors, however, Mishra's study deals entirely with the process of modernization among slum dwellers, a low-status and relatively traditional population that constitutes a fourth to a half of all urbanites in most developing nations. Further, Mishra focuses mainly on the role of mass-media communication in modernization.

Like most other scholars of modernization, Mishra views modernization as a multidimensional phenomenon, which he indexes mainly as empathic ability, political participation, and freedom from family dependence. The main independent variables are mass-media exposure and credibility, with some attention given to such other variables as literacy, formal education, and sex. The data come mainly from personal interviews with a sample of 401 male and female slum dwellers in Delhi. The results are generally consistent with previous findings on communication and modernization among other modernizing populations: mass-media exposure is a relatively strong predictor of modernization. There are also findings of some surprise to me: the rather high level of mass-media exposure among the respondents (about 40% read one or more newspapers at least once weekly, and 63% listened to radio at least one hour per day), and the high credibility of the mass media (many slum dwellers believed that radio always tells the truth).

This slim book is extremely well written and covers a thorough review of the growing literature on modernization, mass communication, and urbanization. The Delhi slums seem to be much like those in other countries, including the United States, and Mishra's results may be applicable to a wide range of sociocultural conditions. Even though the main findings on mass communication and modernization are hardly surprising, they are a solid academic contribution in showing the parallel to previous investigations, mostly completed among villagers. Further, the present work was conducted by one of the first non-Western scholars to study this topic, and he is to be lauded for his attempts to escape Western biases in the study of modernization.

Perhaps it is unfortunate that the author did not give more attention to

the two-step flow of communication by which the mass-media messages are passed along from opinion leaders to other slum dwellers and did not use multivariate statistical methods to provide understanding of the complex interrelationships among his variables. Nevertheless, Professor Mishra's contribution advances our grasp of communication and modernization in important ways.

Modernizing Racial Domination: The Dynamics of South African Politics
By Heribert Adam. Berkeley: University of California Press, 1971. Pp. xi + 203. \$2.65 (paper).

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If one holds that the job of the intellectual is to formulate the right questions and suggest the right answers, then Heribert Adam's book is to be counted a success. Discarding the customary polemics and wishful thinking as he does, he comes up with fundamental questions about South African society and builds a realistic and detailed critique in which theory and description are happily welded. So lucid is the writing, and so cogent the thought, that the book can be easily read though the thoughts it provokes can occupy the serious student for many hours after the book has been put aside. As in every general and provocative study there are things to question, but few will find the basic arguments other than convincing.

The key conception is that South Africa is governed by a "pragmatic oligarchy"—an autocratic, racially defined minority caste ruling other caste hierarchies which, though protesting from time to time, are and have been unable to mount an effective assault on their rulers. To accomplish this the government actively encourages traditional power structures and ideologies within the framework of the officially segregated and hierarchically ordered racial castes. Much of the book elaborates on this theme.

Past analyses were inadequate, Adams argues, because they rested, in part, on the assumption of "an essential contradiction . . . between an irrational race policy and the requirements of a rationally organized, expanding industrial society" (p. 145). Adam posits, instead, that although racial prejudice is itself irrational a racially prejudiced society may be organized on rational lines, that is, the policy itself may be logically consistent and efficient with respect to its intended purposes, as the South African politics is. The approach is interdisciplinary, for, as the author explains, "without a perspective applying a truly interdisciplinary approach including economic as well as political insights, an isolated analysis of racialism and colonialism is inadequate" (p. 36). To show that South Africa is not, often pictured, "the outdated relic of a dying colonialism" but is instead "one of the most advanced and effective patterns of rational, oligarchic domination" (p. 16), he begins with an analysis of the historical and economic

nomic origins of apartheid and continues to argue, in the chapter that follows, that South Africa resembles a country containing intermingled within it both the metropole and the colonial empire. This allowed the South African government a number of advantages: given that it was able to augment its resources largely from within, a high rate of growth could be maintained because the planners—and central planning was an early feature—did not need to take the wishes of the majority or their needs into account. Given its breadth, necessary in a short book, it is a stimulating and useful account of the working of the South African economy.

The third and fourth chapters are the heart of the book, taking up 83 of its pages. They are certainly among the most interesting. Chapter 3 is a step-by-step comparison of South Africa with Hitler's Germany, and while every argument is aired and properly considered, Adam (rightly) rejects the notion that South Africa's political system can be classed as either "fascist" or "totalitarian." Structural differences between Hitler's Germany and South Africa of today make the lumping of both under these rubrics "ahistorical and useless." In chapter 4 Adam explains how the white oligarchy works to secure a "smooth, frictionless, and tolerable domination over cheap labor and political dependents as a prerequisite for the privileges of the minority" (p. 53). The techniques by which this is achieved are considered from every angle. Opposition among white dissidents is tolerated because it is largely ineffective. If it begins to look threatening it is nipped in the bud. For the white majority, who feel some uneasiness at least about their country's policies, pragmatic domination is legitimated in terms of otherwise acceptable values, such as the independence of the judiciary and the belief that the black majority are largely content. This is not without substance, as Adam indicates, for terror and sheer repression would alone be insufficient, given the disparity in white and black numbers. Therefore, the situation has to be explained in other terms. These terms are, first of all, the partial success of the policy of "separate development"—the creation of autonomous African "homelands" and, through their joint participation in the industrial economy, the benefits which have, in part at least, spilled over to the least privileged as well.

It is in the growth of the economy and the power that this will confer on the subordinate Africans that Adam sees the greatest hope of change. He considers the prospects of overthrow and overseas intervention in chapter 5 and believes that neither has, for good reasons, much prospect of success. Chapter 6, therefore, raises the issue of change and points to the growing shortage of white labor, the irresistible economic pressures leading to wider utilization of nonwhite labor, and the consequences this must have. Interestingly enough, one point that Adam makes is similar to that of a well-known Afrikaaner Nationalist economist, Professor P. J. van der Merwe. That is that the "homelands" may become the basis for an African trade unionism. Again, and again all Adam's arguments seem convincing, because recent events in South Africa bear his analysis out.

Although, as the reader will have gathered, Adam's book is indeed a fine

work, there are points where anyone other than believing Marxists must feel uncomfortable. Adam seems too obsessed with "contradictions" as if these had life and motivation of their own. Also, apparently influenced by Marx, he is too inclined to apply a class analysis where this seems irrelevant. He argues, for instance, "that the historical friction between the English- and Afrikaans-speaking populace is gradually being replaced by class contradictions between the two groups" (p. 179). Few who know the South African scene will agree with this. Recent political campaigns have shown that the old bogey of "Boerehaat" still has its charms for an Afrikaaner majority. He also gives the desire for cheap labor as the motivation of industrialists. He writes: "The interests of [government and entrepreneurs] both coincide on the central issue of maintaining a *cheap labor force* as the single most important factor in the economic boom and high profit rates" (p. 148). He then argues, on the same page, that labor is characterized by a "high degree of inefficiency" in South Africa. Well, one cannot have it both ways, inefficient labor is not cheap. Of course manufacturers want to purchase efficient labor at the lowest cost, efficiency being measured by, say, production per man hour. But offered the choice, entrepreneurs will choose efficient over "cheap" labor because inefficient labor is really not cheap. The quarrel between entrepreneurs and the government in South Africa is caused by the restrictions the government imposes on them in their efforts to use labor in the most efficient way and by the government trying to enforce industrial decentralization, which most entrepreneurs see as detrimental to their interests. Low wages, it should also be recalled, mean low consumption of industrial products and loss of economies of scale. The argument Adam adduces might apply to the mineowners but hardly does to commerce and industry. A more valid argument might be that a "labor-aristocracy" of white skilled workers, determined to retain their bargaining advantage conferred by their scarcity, are the greatest obstacle to African economic advance. This may not suit the Marxists but seems more true. Another point on which issue could be taken is the use of the term "settler" for white South Africans. After being in South Africa as long as white Americans have been in the United States, the term "settler" is, surely, hardly appropriate.

These points should not be construed as condemning the book but rather as insisting that the book encourages thought. One might quarrel with one or another matter of detail or interpretation without his admiration for the range, subtlety, and breadth being in any way diminished. Indeed, a short review of a book such as this one can hardly do it justice. For those interested in pursuing issues further, the author has provided careful footnotes and a well-selected and most useful bibliography. In the text the author has pulled together the best of recent writing as well as providing his own insights and interpretations. Of necessity, therefore, readers must discover for themselves the many facets of interest and value contained in its pages. It is one of the few worthwhile books of a general nature that has appeared on South Africa.

Unions, Parties, and Political Development: A Study of Mineworkers in Zambia. By Robert H. Bates. Yale University Press, 1971. Pp. xi+291. \$12.50.

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It is by now almost a commonplace to suggest that political studies of independent African nations have given gratuitous prominence to ethnicity and intertribal tension. There remains to establish the analytical viability of alternative foci for examining the process of political development. In this respect, Robert H. Bates's excellent study of trade unions in Zambia has a singularly important theoretical significance. It calls attention to the overwhelming importance of the relationship between state leadership and a powerful, well-established industrial class, in this case the mine workers of the Zambian copper belt.

As described by Bates, the tensions between these two powerful forces comprise a classic confrontation. State leadership, intent on the goal of national economic development and largely dependent upon revenue from the copper industry to finance it, is primarily concerned with the productivity of the copper miners. Its policy toward the miners as workers is to insist on industrial discipline, wage restraint, and the avoidance of strikes. Bates demonstrates forcefully that, by any number of criteria, this policy has on the whole been a failure. Since independence, absenteeism has risen, productivity has fallen, industrial discipline has deteriorated, strikes have increased, and wage increases have far outstripped the industry's faltering productivity. The country's political leaders have been consistently frustrated in attempts to use the trade-union movement as an agency for the implementation of state policy.

The reasons for this failure constitute the core of Bates's analysis. Why have the mine workers so persistently refused to accept their political leaders' injunctions to forego material gains in the interests of the society as a whole? Part of the answer lies in the simple fact that these workers, like workers elsewhere, have always been determined to improve the material conditions of their lives, and independence has in no fundamental way altered this determination. Perhaps a more important explanation lies in the racial structure of the mining industry. In Zambia's copper mines, the high-level supervisory, managerial, and administrative positions have been held by expatriate Europeans. The roughly 6,000 white European workers receive incomparably higher wages and more generous fringe benefits than the approximately 42,000 African miners. In 1966, for example, African workers, though they constituted about 90% of the work force in the mines, received less than half the total wages. The average wage for a European worker was about eight times higher than the average wage for an African, and the European workers received lavish housing allowances, longer leaves, and educational benefits for their children while

Africans received none. Moreover, European bosses and shift captains often behaved toward African subordinates with attitudes of racial disparagement and contempt.

From the standpoint of the Zambian mine workers, independence has in no basic way affected the colonial character of the mining industry. It may, in fact, have contributed to the militancy of the Zambian mine worker. Bates employs the concept of status inconsistency to describe the bitter irony of the situation in which African workers found themselves. "For many African employees, the dominance they possess over the Europeans in the political realm is difficult to reconcile with their subordination to the white man in the industrial sphere" (p. 89). Though African mine workers may be substantially better off materially than their kith and kin in the agricultural sector, it is inevitable that they should measure their economic lot against the continuing privilege and affluence of the expatriate elite stratum.

While the sociology of the mining industry explains the lowered morale and heightened militancy of the Zambian mine workers since independence, Bates also holds certain structural factors accountable for the government's failure to gain acceptability for the policy of industrial discipline among the miners. Perhaps the most basic of these is the highly decentralized organizational pattern of the Mineworkers Union. The government has consistently sought to use the union as an agency for influencing the behavior of the mine workers, and the official reasoning behind the policy of industrial discipline has been accepted by union officials in the head office. But the union structure accords considerable operational autonomy to union leaders at the branch level, and these officials, far more than those at the union headquarters, are alert to the day-to-day grievances and injustices experienced by the miners. The result has been a continuing series of strikes, work stoppages, and wage demands initiated at the local level of the union. Since leaders at the head office are dependent upon the branches for their positions, they are powerless to have any countervailing effect on local union militancy.

Bates's conclusion is that the governing party, the United National Independence Party (UNIP), is superior to the union as an output structure. The reasons for this are somewhat complex but have to do primarily with the fact that the government's policy toward the miners is actually an ambivalent one. If its policy toward them as workers is to urge industrial discipline and wage restraint, its policy toward them as Africans is to work toward greater social justice. Though the party has been powerless to affect the social structure of the copper industry where the miners work, party leaders have been able to accomplish a good deal in the way of social reform in the African townships where the miners live. The successes of UNIP in this respect have given it credibility among the workers and this, in turn has lent plausibility to the government's exhortations of industrial discipline.

The end result is a kind of paradox. In spite of the strikes, work stop-

pages, grievance proceedings, and wage demands initiated by local union officials and countenanced by union leaders at the top, the racial structure of the mining industry remains unchanged, and the union has difficulty maintaining legitimacy among its members. The governing party, despite its articulation of the policy of industrial restraint, has been able to use its power to accomplish some concrete gains for the workers and derived from its success a greater measure of popularity. Whether this will continue to be the case after nationalization, with the government in a position to exercise more direct control over mine management than was formerly the case, remains to be seen.

Entwicklungsbewusstsein und wirtschaftliche Entwicklung in Indien [Development consciousness and economic development in India]. By Werner Draguhn. Papers of the Institute of Asian Studies, Hamburg, vol. 28. Wiesbaden: Otto Harrassowitz, 1970. Pp. 288. DM. 58.

Reinhold Riedel

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The author tries to grasp the role of consciousness in the development of one country—India. One of the main targets set for India's economic policy is the increase of the average per capita income. The three "classical" factors of production are available in abundance or at least in the necessary amounts. The reason for the lack of larger economic growth in spite of this fact is seen in the absence of adequate "development consciousness."

Draguhn develops a hypothesis based mainly on Weber's thesis, according to which religion plays the decisive role in economic development, and attempts to prove its validity for the Indian case. He defines development consciousness: "Thus, we shall conceptualize development consciousness here as a positive behaviour, based on a positive attitude towards economic matters generally, a positive behaviour regarding *all forms and accompanying phenomena* of economic development and therefore also of economic growth, including the goals and motivations ruling over economic man" (p. 32; my translation and italics).

The criteria for the study of development consciousness are found by transferring the point of view of developed, industrialized, Western countries to India (pp. 27, 39). Draguhn believes that in the long run the developing countries ought to attain a situation corresponding to the existing conditions in the industrialized Western nations; he therefore analyzes factors supporting or hindering development consciousness and ways and means to achieve the intended development (p. 39).

This would suffice to show that the concept of Draguhn's study is basically conservative. Conservative in the sense that he wishes to achieve

economic development without changing the dependence of the economy on foreign capital or the existing social conditions in India.

The book is a compilation of a large number of partly contradictory facts pertaining to development consciousness. The conclusions drawn are often surprisingly interpreted to fit the above-mentioned purpose. Draguhn describes characteristic elements of the economic behavior of three main groups: the rural population (constituting about 80% of the total population), the industrial workers (less than 11% of the employees), and the entrepreneurs (a few hundred families). Characteristically, Draguhn does not bother to differentiate among the groups that make up the rural population, nor does he quantify relevant factors.

The discriminating influence of British colonial rule against the Indians is described in some detail concerning aspects of "free" trade policy, administration, jurisdiction, opening up of the country by better means of transportation, etc. This seems to have been accidental, however, as the author regards foreign domination as a "challenge" (Toynbee) (p. 96), as the clash of two cultures (p. 97), or as the influence of dynamic English elements on the static economic conduct of the Indians (p. 23). He also seems surprised at the lack of "wide-spread economic opposition" (p. 285), though it is clear that those who could have opposed either collaborated with the mightier or were suppressed by force of arms, legislation, economic policy, etc. This may as well be due to the fact that Draguhn never consciously includes political opposition—uprisings, mass movements, etc.—and obviously neglects the interaction of politics and economics as a whole.

When analyzing the effects of religion on economics, the author comes to the conclusion that Parsism has had more dynamic influence on consciousness than Jainism, Sikhism, Christianity, Hinduism, or Islam. The "diaspora" situation of small religious communities is not taken into consideration. Christianity, present in southern India since at least A.D. 400, has not shown the impact on Indian culture and development consciousness it purportedly had in Europe. No satisfactory explanation is given.

The description of the objective and subjective elements of development consciousness tends to be covertly presumptuous. Draguhn states that neither the Indian nor the imported English educational system was able to further development consciousness—consisting of rational economic behavior, logical thinking, self-discipline, and self-confidence among others.

On the whole, Draguhn has worked hard to gather facts and aspects of development consciousness. However, he has not been able to bring them into a satisfactory overall concept and tends to overestimate or overemphasize the role of consciousness as he sees it in economic development. A revolutionary change of the educational system is the conclusion at which Draguhn arrives. But this change in education is suggested solely with the aim of promoting economic development; economic development that in his sense can only be in the interest of the entrepreneurs. Welfare for the toiling masses of India remains a mere side effect of this policy.

Class and Race in Cities and Suburbs. By Leo F. Schnore. Chicago: Markham Publishing Co., 1972. Pp. xii+106. \$6.95 (cloth); \$1.95 (paper).

Daniel N. Gordon

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In this short volume, Leo Schnore formulates an evolutionary hypothesis for the residential structure of cities. His hypothesis draws its theoretical roots from the Burgess concentric zonal scheme, which he examines in some detail. He points out that the major assumption made by Burgess is that the zonal hypothesis is an ideal type (perhaps approximated in some cities, but always affected by local conditions) and applies only to growing cities. However, whereas Burgess's scheme focused on urban land uses and changes in them, Schnore focuses specifically on "the validity of the zonal hypothesis as a description of the residential distribution of social classes" (p. 14).

In the first chapter, there is a detailed discussion of the concentric zonal hypothesis—what it says, the assumptions which underlie it, and the criticisms which have been made of it. Furthermore, commonly assumed patterns in which the lower social classes live near the center and the middle and upper classes live at the periphery did not always apply. Evidence from South American cities, medieval cities, and even early Chicago indicates that where transportation (and this is a key element in Schnore's evolutionary scheme) is poor, the upper classes live near the city center.

The nub of Schnore's evolutionary hypothesis is contained in the following passage: "Given growth and expansion of the center and appropriate improvements in transportation and communication, the upper strata might be expected to shift from central to peripheral residence, and the lower classes might increasingly take up occupancy in the central area abandoned by the elite. Despite mounting land values occasioned by the competition of alternative (nonresidential) land uses, the lower strata may occupy valuable central land in . . . high density 'slum' arrangements" (p. 120).

Although Schnore formulates an evolutionary scheme, his data are drawn only from U.S. censuses for 1950 and 1960. Basically his data consist of comparisons between the socioeconomic status of cities and suburban rings in standard metropolitan statistical areas and urbanized areas. And his indicators of socioeconomic class are composed of measures of education, occupation, and income.

A variety of techniques is used to compare the socioeconomic status of central city and suburban populations. Analyses are presented both cross-sectionally (for 1950 and 1960) and longitudinally (for the 1950–60 decade). Among other things, Schnore finds that age of the central city (i.e., the census year in which the central city first reached 50,000 population), the percentage of the total urbanized area's 1960 population living in the central city (which Schnore labels "annexation policy"), and, to a

considerably lesser extent, the size of the central city are related to the socioeconomic differences between cities and suburbs. Age and size are positively related to suburban populations with higher SES than central city populations, while annexation policy is related in the reverse direction. In addition, at one point Schnore looks at the 1960 distribution of black ghetto populations within central cities and finds that they, too, are distributed by socioeconomic status—the poorer living closest to the center of the city while the better off live further out. Moreover, city-suburban status differentials for blacks are similar to whites, except in the South. Finally, shifts (1950–60) in city-suburban differentials tend to be in the expected direction, though there are a number of exceptions.

These are only part of the results presented in the book, but they give a flavor of the type of evidence marshaled in support of Schnore's evolutionary hypothesis. Schnore concludes that his evidence is at least supportive of his hypothesis, but he realizes that he needs much additional data (both prospective and retroactive) to prove his view.

My own view is that the data he presents do not contradict his hypothesis, yet they are hardly adequate to test it. Schnore's hypothesis deals with "improvements in transportation and communication," yet his data are from periods (1950 and 1960) when the automobile, truck, telephone, and other communication and transportation technology are well developed. In addition, his hypothesis is also concerned with the shift of the upper strata from the center of the city to the periphery, yet nowhere, except for blacks, does he deal with the distribution of social classes in relation to the city center. Moreover, he presents no more than anecdotal data on movements away from the center. Merely comparing city-suburban status differences does not get at the issues Schnore raises. To prove his hypothesis, as stated, he needs historical evidence on the residential movement of social classes in central cities and suburbs as well as concurrent data on transportation developments.

Also, with further analysis, Schnore's hypothesis might need additional refinement. Where historical data might in fact support his hypothesis for older cities, what about cities which grow up in the automobile age? The adage that "Los Angeles is a set of suburbs in search of a city" illustrates this point. The development of newer cities might possibly deviate from the pattern observed in older ones, because the newer ones have more flexible transportation alternatives from the start.

Overall, this is a useful book which should be read by urban sociologists. Schnore's discussion of the Burgess zonal hypothesis puts it in a proper perspective—one that demonstrates its strengths as well as discusses its weaknesses. In addition, the use of a zonal perspective provides a theoretical focus for the synthesis of studies on city-suburban status differences. However, this book, by the very nature of its contents, does not provide any real "breakthroughs." Hopefully, though, this volume signals Schnore's transition from emphasis on current cross-sectional studies to emphasis on historical and longitudinal ones.

Drugs and the Public. By Norman E. Zinberg and John A. Robertson. New York: Simon & Schuster, 1972. Pp. 288. \$2.95 (paper).

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When confronted with difficult policy questions, many individuals respond to the immediate and visible while simultaneously neglecting those dimensions that are more remote and intangible. This immediacy has certain advantages, but it does militate against an intelligent and comprehensive course of action. Such a limitation can result in the overweighting of immediate pressures in policy formulation. This characteristic is particularly relevant in the area of drug control.

The systemic effects of American narcotics enforcement during the past half-century are legion; they include an increase in urban lawlessness, police corruption, a lowered respect for the law, and a heightened degree of public cynicism. Drug-enforcement agencies in the United States have helped to foster alienation toward the political system. The punitive approach to drug enforcement, which began with passage of the Harrison Act in 1914, has produced a situation in which the effects of drugs, including the most emotional laden one of all, heroin, are distinctly secondary in their social and personal consequences to the effects of the control laws themselves. Those laws have transformed inexpensive chemicals into price-less commodities that support a vast criminal drug traffic.

In *Drugs and the Public* Norman Zinberg and John Robertson argue that the legal approach to drug control represents a preoccupation with moral control based on ideas of what threatens the prevailing values of American society. The drug laws, they contend, reflect society's reactions to perceived threats to basic values and are based on dogmatic and non-empirical assumptions, the overriding one being that drug use is inherently bad and that only severe sanctions can eliminate it. Those assumptions, which reflect numerous myths and fallacies about the subject of drug abuse, have transformed "an issue which, contrary to the general impression, is a comparatively small matter as far as public health is concerned" (p. 11), into a matter of overriding social and political importance.

The authors of *Drugs and the Public* do not dispute the contention that valid objectives can be realized through drug regulation. However, they argue that drug enforcement should not overrule the preservation of liberty as a prime goal of society and that a policy which "discourages or ameliorates the effects of harmful drug use" (p. 250) should be in harmony with the principles of a democratic society.

Zinberg and Robertson call for the repeal of existing drug laws in the United States on the grounds that they are not justified, either by the effects of drugs on the individual or the society, or by "the results of punitive legislation" (p. 249). The present laws reflect the viewpoint of a

Congress which has sought to reflect the viewpoint of a misinformed and emotional citizenry. Present government policies contribute to a growing sense of alienation which undermines the legitimacy of the government. Zinberg and Robertson contend that government efforts must be redirected from an emphasis on punishment toward an emphasis on treatment. The authors advocate a systemic approach to drug usage which is flexible, responsive to empirical evidence, and which aims to deemphasize the issue. Their approach would require the recognition that tolerance toward the use of drugs by certain members of society is not totally undesirable. In this respect the authors' position is not far from the present laws which govern the use of alcohol. The authors argue that reason must gain ascendancy over the myths surrounding drugs; that the public must discard the fallacies which have contributed to the development of a punitive approach to drug enforcement.

Drugs and the Public merits praise for several reasons. First, it wisely asserts that a deficiency of many public-policy studies is the failure to investigate the publics that are concerned with alternative policy solutions. Analyses of policy alternatives frequently fail to mention the publics that are directly involved, "let alone the larger public which is potentially in contact" (p. 74). Moreover, the authors have underscored the lack of impartiality in the administration of drug-enforcement laws, as well as the rigidity and lack of responsiveness that have characterized their implementation. Finally, the authors allude to the problem of "bureaupathic behavior," which includes resistance to change. This problem can threaten the kind of rapport that is necessary to have a reasonable discussion of goals and in order to rationally consider alternative policies.

Drugs and the Public is not infallible. For example, Zinberg and Robertson err when they contend that "hitherto, all the costs have been thought to flow from drug use; the social costs of a devalued law have seldom been considered" (p. 20). This statement is untrue. Indeed, the social costs of a devalued and discredited punitive approach to drug enforcement have been considered by many observers since the Harrison Act was passed. Probably the most articulate of these observers is the sociologist Alfred R. Lindesmith. The authors also fail to perceive one (and perhaps the only) advantage that accrues from the present punitive approach to drug control. The new heroin user is vulnerable to changes in the availability of heroin because he may not be willing to make the intensive search necessary if it is not readily available. Extending the search time for beginners may discourage or at least reduce the frequency of heroin use.

Drugs and the Public contributes to the development of a broader and more complete picture of the drug problem in the United States. It should be of interest to those persons who want to discard the punitive and moralistic approach to enforcement which the country has suffered with for so long. Generally, it should appeal to all who are concerned with understanding more about one of this country's most devastating social ills.

Religion and the Solid South. By Samuel S. Hill, Jr., with Edgar T. Thompson, Anne Firor Scott, Charles Hudson, and Edwin S. Gaustad. Nashville, Tenn.: Abingdon Press, 1972. Pp. 208. \$2.95 (paper).

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The white South's bonds to the national Democratic party have been dissolved, by mutual consent. Its practice of racial discrimination is increasingly subtle and Yankeeified. Its rural poverty is masked in the statistics by new urban and suburban affluence. For many students of the South, these data present the same problem the Salk vaccine posed to the March of Dimes. Those who study Southern religion, however, find more to write about each year. As Southern culture's other props drop away, the importance of their subject is increasingly apparent.

In 1972, at least half a dozen books (and essays and chapters in as many more) were devoted to aspects of white Southerners' religious life. For sociologists interested in the topic, this volume will be one of the most valuable and—at the same time—one of the most exasperating. Growing out of a symposium sponsored by Duke University's Center for Southern Studies, the book contains a general introduction by the editor and six essays. The essays are only loosely related to one another, but serve to illustrate the range of approaches being taken to the study of this fascinating and complex topic.

Much of the important recent work on Southern religion has been done by historians, and that discipline is represented here by Anne Scott, who (in a chapter adapted from her book, *The Southern Lady: From Pedestal to Politics, 1830-1930* [Chicago: University of Chicago Press, 1970]) examines the changing religious concerns of Southern women. The form of their religion, she demonstrates, "gradually changed from intense personal piety to a concern for the salvation of the heathen and for social problems [and they] came bit by bit to develop something which was in practice, if not in theology, a social gospel" (p. 93). (The distinction between practice and theology is, as Samuel Hill implies elsewhere in the book, an important one, of course.) Scott argues that the churches provided an institutional framework, almost the only one, for the nurturance of "women leaders of considerable significance in the shaping of southern society and even southern politics in the ensuing decades" (p. 93).

Edwin Gaustad, another historian, provides a useful survey of the historic settlement patterns of different religious groups within the South and of the present-day religious geography of the South. His 19 pages of maps are useful, but less so than they might be: the datum plotted is usually the number of churches per county rather than the number of churches per capita (or, better, members per capita).

Charles Hudson, an anthropologist, analyzes "a fundamentalist Christian belief-system"—attempting in particular to explain how a fatalistic individ-

ual can believe in free will. He bases his conclusions on interviews with "rural-born, working-class" informants (p. 122), and notes that one is free to doubt (as I do) that the world view he describes characterizes even middle-class members of the same denomination (p. 139). Nevertheless, persons like his informants make up a significant proportion of the population of the South and have migrated in large numbers to many eastern and midwestern cities as well.

For sociologists, the book's interest will stem principally from the essays by Edgar Thompson and Samuel Hill. Thompson, the only sociologist among the contributors, applies his knowledge of the plantation as a social system to a discussion of the relation between that institution and the religion which served for awhile to sustain and to validate it. (The volume is dedicated to him, in recognition of a lifetime largely devoted to enlarging our understanding of the South.) Hill, in two wide-ranging and free-wheeling essays, extends the analysis of Southern religion and society begun in his 1967 book, *Southern Churches in Crisis* (New York: Holt, Rinehart & Winston). He touches on many subjects ("the Southern saint" as a social type, the role of the churches in generating Southern nationalism in the 1830s and 1840s, the apparent contradiction between the rich, textured personalism of Southern folk culture and the austerity of Evangelical Protestantism), but returns again and again, in both essays, to an examination of the implications of Southern popular theology for the social and political views of Southern Christians. Many observers have argued that the congregational polity of the region's dominant churches has served to damp the influence of specifically religious ideas in political controversy. Hill makes the point that the "religious ideas" at large in Southern Protestantism would not make much difference anyway. He argues that an almost exclusive emphasis on the nature of the relationship between God and the individual sinner relegates to secondary importance the nature of relationships between men. If one "possesses certain knowledge that he has passed . . . to a new state, where guilt is pardoned and heaven guaranteed" (p. 193), who is to say one should do anything one is not spontaneously moved to do? Moreover, in this world view, if one is moved to do something for one's neighbor, what greater gift can one give than to work to bring him to a like certitude—to get him "right with God"?

The introduction to this volume states that all of the authors take as given "the role of religion as conservator and reinforcer of, as distinct from agent of change within, popular (white) southern culture" (p. 22). Hill seems to be saying that this is not a perversion of that religion. When it is most true to itself, it will—by the nature of its theological underpinnings—serve to perpetuate whatever social arrangements exist by "deflecting moral earnestness" into campaigns to save souls through individual conversion (p. 33). Hill is not pleased with this conclusion, and the purpose of his concluding chapter is suggested by its title: "Toward a Charter for a Southern Theology."

I said earlier that the book is both valuable and exasperating. It is valuable because it is sprinkled liberally with suggestive speculation and hunch.

It is exasperating because so many of the most appealing generalizations remain at the level of speculation and hunch. Hill notes as much in his preface but claims that the book is at least a beginning. It is that, and will serve as a valuable foundation for further—and more systematic—examination of the secular role of religion in Southern culture.

Family Issues of Employed Women in Europe and America. Edited by Andrée Michel. Leiden: E. J. Brill, 1971. Pp. 166. Gld. 42.

Valerie Kincade Oppenheimer

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This is a collection of 10 articles plus an interpretive introduction by the editor, who also contributed an article on Parisian working women. Six of the articles deal with family issues of employed European women and four presumably deal with parallel issues regarding American women. The quality of the articles varies considerably. Two of the most interesting are the Piotrowski article, reporting on a nationwide survey of Polish married women, and Kharchev and Golod's article on women factory workers in the Soviet Union. Of the two, Piotrowski's is the more sophisticated and imaginative. What is especially interesting about both these studies is their revelation of the pervasiveness of traditional sentiments and behavior in both countries—despite the fact that their women have among the highest rates of labor-force participation in the world and that both societies are ideologically committed to social and economic equality for women. Somerville's article is also about working women in the Soviet Union and gives an interesting historical overview of their situation. There is also an article on Czechoslovakian women by Prokopec which is insightful but rather short and hence somewhat superficial. The only article on working women outside of Eastern Europe is Michel on Parisian working women. It is, unfortunately, not nearly as imaginative as Piotrowski's article on Polish women, although it deals with some of the same issues.

Unhappily, the worst article in the volume is the first one—Grønseth's "The Husband Provider Role: A Critical Appraisal." If I had not been obligated to read the volume, Grønseth's article alone would have immediately caused me to put the book aside as just another collection of polemical outpourings. Headings such as the following abound, with a text to match: "The Neurotically Symbiotic Character of Marriage and Parent-Child Relationships," "The Non-Autonomy of the Nuclear Family and Its Subordination to Capitalist Economic-Political Systems and Class Structures," "Sexual Oppression and Repression," "Socio-Political Alienation, Apathy or Destructiveness as Further Personality Consequences of the HEPR [Husband Economic Provider Role]," and so on.

The four articles on American women are a rather odd assortment in terms of the main theme of the collection, and without reading the introduction the reader would be rather confused by the inclusion of these

particular articles. Even then, it is not always clear. Weller's article on "The Impact of Employment upon Fertility" is very interesting, though the gist of his argument appears elsewhere. However, his discussion is probably more relevant to the question of the relationship of labor-force participation to fertility in developing societies rather than in the United States, as one would expect from his earlier work on the subject—both alone and with Stycos.

Feldman has an article on the effects of children on marital happiness, but women's employment does not enter into the analysis at all. His major finding is that couples with children have a *lower* level of marital satisfaction than those without children and that this is particularly true in companionate marriages. However, the data are not presented in sufficient detail to evaluate the study properly. One wonders, for example, whether the differences in marital satisfaction are large enough to be substantively as well as statistically significant. Also the author fails to evaluate the significance of the many selective factors which are probably operating. For example, there is the possibility that unhappily married childless couples have less reason to stay together than unhappy couples with children, or that some couples may have a child to help patch up an unhappy marriage, etc. More important is the question of just what the relevance of this article is to a book on family issues of working women. According to Michel, it is in its indication that American society will have to do something to reduce the conflict between motherhood and the desire for husband-wife companionship. The connection with the main topic still seems rather tenuous.

Robinson has an article on historical changes in how people spend their time. Michel indicates that the reason for including this article is that it shows that in spite of the growing importance of electrical appliances women were just as overburdened with housework in the 1960s as in the 1950s and 1930s. And presumably, until these burdens are reduced, women's occupational status and their marital happiness will remain in jeopardy. However, Michel overlooks the fact that there is a considerable element of choice involved in the hours devoted to housework. For example, many products are available to substitute for the housewife's labor, and unless we know that the costs of such appliances are beyond their incomes, we cannot assume that such tasks are imposed rather than willingly taken on. In addition, some of these burdens are those involved in choosing a more elaborate life-style—living in a house rather than an apartment, for example.

The only article in the section on family issues of American working women which specifically deals with this topic is Poloma and Garland's on professional women, and it really is not good enough to carry the whole load. For one thing, the sample size is 53 couples where the wife was either an attorney, physician, or university professor. Thus the sample is tiny—too tiny to represent working women in general or even upper-level women, and so tiny that to include three rather different types of professions makes it very heterogeneous—not to mention that these women were at various

stages of the family cycle. Furthermore, there is no description of how the sample was selected—always a rather disquieting omission in small studies. The main gist of the authors' argument is that these women do not have true career commitments (in some sort of ideal sense of the term) but rather value career after the family. While I am sure that there is a certain truth in this, I also think that Poloma and Garland overstate their case. This is partly because they seem to have started the study with some rather naïve expectations regarding the degree of occupational commitment such women would exhibit and have overreacted to the less than total commitment actually encountered. Second, their analysis suffers from the fact that they only have ideal types to compare these women with rather than flesh-and-blood reference groups. For example, I think some direct measurement of professional men's career commitments would be in order rather than a presumption of what these are. Poloma and Garland concede that not all men are career oriented in the ideal sense they have in mind, but they argue that the husbands of the women studied generally were much more career committed. It would seem to me, however, that, given the kinds of selective factors that will be operating in the mating of such women, their husbands would hardly be representative of all professional men but would rather tend to be among the more successful and committed to their careers.

In sum, there are several interesting articles in this volume. However, the coverage of women in different countries is very uneven, to say the least, and the articles do not hang together too well. But, then, an integrated set of 10 articles, all by different authors, would indeed be a rare achievement.

On Military Intervention. Edited by Morris Janowitz and Jacques Van Doorn. Rotterdam: Rotterdam University Press, 1971. Pp. xxi+520. Dfl. 95-.

The Military-Industrial Complex: A Reassessment. Edited by Sam C. Sarkesian. Beverly Hills, Calif.: Sage Publications, 1972. Pp. xii+340. \$12.50 (cloth); \$7.50 (paper).

Peace, War, and Numbers. Edited by Bruce M. Russett. Beverly Hills, Calif.: Sage Publications, 1972. Pp. 352. \$17.50.

James A. Stegenga

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These three books reinforce a suspicion that the proceedings of symposia probably should not be published. The organizer of the conference can't know in advance if the papers he solicits will all merit publication. But the editor is pretty much stuck with whatever is turned in; he can try to persuade the writers to fix up their papers, but he doesn't enjoy the journal

editor's luxury of rejecting 80% of the material submitted. Consequently, papers are published that appropriately wouldn't make it through an ordinary referee process. And whatever visions the editor had about the shape of the book are at the mercy of notoriously uncooperative participant writers. (Compare the symposium editor's task with that of the editor of an anthology who can select from among hundreds of already screened and published articles representing most conceivable methodological and substantive perspectives in order to arrive at a balanced—or unbalanced—collection.) But perhaps most importantly and regrettably, any outstanding symposium papers are apt to get lost in the information explosion shuffle. Published in the symposium proceedings, the important paper will not be published later in any journal since journal editors normally have a policy against using papers that have already appeared in print; hence, it will not be indexed in the usual indexes that scholars and students rely upon to develop their working bibliographies and will thus be available primarily to that small handful of investigators who happen across it accidentally. Who knows how many papers that might usefully have appeared in the *American Journal of Sociology* or another indexed outlet have instead "disappeared" following publication in symposia proceedings? Perhaps I can rescue several here as well as make a few additional general points.

In the first symposium, military intervention refers to the intervention of military men in the domestic affairs of their own nations, mostly in the rather overt form of outright seizures of governmental roles and structures by upset colonels and generals seeking, as the two generals who led the 1962 takeover in Argentina explained, "to put an end to corruption, hatred, resentment, chaos and despair" (p. 42). British sociologist Philip Abrams shows that historically the soldiers themselves have been "largely responsible for the problem they claimed to be solving," a crisis of legitimacy regarding societal institutions (p. 43). He goes on to suggest that once in power military governments "founder because soldiers cannot convince civilians that the order derived from awe is a necessary alternative to that derived, however untidily, from law" (p. 57). Papers by Morris Janowitz and C. I. Eugene Kim support this contention by showing that the performance of military governments seems to be better when the soldiers (like Turkey's Ataturk and Korea's Park) who have seized governments work to create civilian political institutions and once again separate (or differentiate) the military from continuous involvement in governing.

Philippe C. Schmitter puts his finger on another possible indicator when he shows with his careful statistical analysis that military regimes in Latin America tend to spend more on the military and less on welfare than civilian regimes, perhaps accounting for a poor performance rating not only in terms of improving the lot of their subjects but also in terms of the tenuous order they attempt to maintain.

There is not enough attention given in any of the papers in the Janowitz–Van Doorn symposium to the effect of military regimes on such ultimate values as well-being, freedom, and justice (as distinct from the penultimate

value of order that the authors have used as their dependent variable). And since they focus on the causes and consequences of military regimes, the authors neglect the more subtle but sometimes dominant influence of the military on nominally civilian regimes.

Such influence and what to do about it constitutes the very fuzzy focus of the papers in Sarkesian's symposium. Several papers raise questions about the existence, extent, and role of the military-industrial complex (MIC). Charles Moskos begins with a nice bibliographic essay dividing writers on the MIC into several camps and then disappoints us by asserting without elaboration that "the concept of the military-industrial complex is ultimately incapable of explaining the surge of opposition to the military-industrial complex which has come to surface on the American public scene in recent years" (p. 17). Albert Biderman uses statistics to show pretty persuasively that military retirees are not very influential in defense industries, despite some isolated cases. And Charles Wolf assaults some straw men before stating some obvious truths about the complex, pluralistic nature of the military establishment which not even the editors of *Ramparts* would dispute.

For those whose constant complaint is that writers do not offer concrete alternatives and should, Seymour Melman offers a detailed alternative military budget roughly one-third the size of recent budgets; and Bruce Russett offers an argument for replacing our present deterrent policy with regard to the Soviet Union with a slightly less morally objectionable policy of threatening retaliation only against Soviet military targets. Melman can be criticized as a politically naïve visionary whose budget would never win approval of the congressional committees. And Russett accepts unskeptically the very dubious psychological assumption of deterrence doctrine that terrifying a nervous adversary will ensure his continued rationality in a crisis situation. But at least both authors have made concrete proposals addressed to some of the consequences of heavy military influence in our domestic affairs.

The basic shortcomings of the papers in Russett's symposium stem from their common methodological orientation. All employ a macroscopic or aggregate data approach to the problem of explaining warfare. The unit of analysis in each paper is some collectivity rather than the individual. The behavior under examination is that of states as actors. The authors focus, then, on the characteristics of these collectivities, principally those structural characteristics of states or systems of states that are quantifiable and thus statistically manipulable. The question becomes: what structural characteristic(s) seems to be correlated with the incidence of warfare?

Russett and Betty Crump Hanson in their introduction concede two difficulties with the method. The first—frequent reliance on too few cases—can presumably be remedied by more and better data gathering (though the unreliability of such sources as speeches, government statistics, and newspapers is notorious and not all that encouragingly dealt with in these papers). The second, though, is more sticky and too easily dismissed by

Russett and Hanson: if structural variables are as important as the hypotheses suggest, longitudinally based generalizations about the international system are bound to be worthless since technology has changed these structural properties so dramatically from decade to decade that instances from the 1870s (or even the 1950s) cannot be meaningfully classed with instances from the 1970s.

But maybe structural variables are not so important. The paper by J. David Singer, Stuart Bremer, and John Stuckey seems to show that neither distribution of power nor relative rates of change of national capabilities can be meaningfully correlated with incidence of warfare. Nazli Choucri and Robert North suggest that high rates of technological development tend to be negatively related to military preparation; but what of pre-World War II Japan and Germany? Or Israel and South Korea today? Russett and Hanson point out that the collected papers "produce an image of heavy environmental constraint on nations' behavior, of the serious degree to which nations' leaders are prisoners of the situations in which they find themselves" (p. 17). The macroscopic approach leads one, almost definitionally, to this kind of determinism, whereas any case study shows dozens of points at which the individuals could have chosen differently and some where the nature of the situation would seem to have dictated a different choice (think of Hitler's foolish decision to invade Russia or Admiral Kimmel's failure to fly air patrols north of Pearl Harbor). Surely we should resist structurally based explanations of national behavior that have to concede at the outset so many exceptions that they are some of the most important instances the explanations should account for.

Anyway, whatever correlations between structural variables and national behavior turn out to be generally accurate would be merely halfway, unfinished explanations, since the structures as well as conformity by statesmen to them would both next require explanations, explanations that can probably only be psychological in nature. We may speak for convenience of nations, governments, and institutions as actors, but we know that their "acts" are the products of many individual decisions. For explaining social acts, some form of psychological reductionism would seem to be preferable to the artificial and distorting reification and determinism that the aggregate data analysis approach inevitably seems to involve. And it would be more compatible, too, with our legitimate efforts to hold statesmen responsible for their actions rather than to permit them to plead that they were mere helpless prisoners of structures or circumstances or social forces.

The macroscopic approach will doubtless persist. Most social science "methods" courses stress it, many exclusively. Officials in government agencies and foundations like to support its use because they're committed to social engineering which seems to call for lots of data to make decisions look rational. Graduate students like it because aggregate data analysis theses are quicker and let them use the skills they were forced to learn and socialized to believe are the only respectable tools of scholarship. But

the results so far are not promising. Perhaps the approach itself is flawed. At the very least, it is not so dazzling as to warrant rejection of alternative approaches.

The Military in Politics: Changing Patterns in Brazil. By Alfred Stepan. Princeton, N.J.: Princeton University Press, 1971. Pp. xiii+313. \$10.00.

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University of Toronto

It is an interesting fact that the military, one of the most important institutions of any state, has received relatively little attention from those social scientists writing in the Anglo-American tradition. This absence is notable when one considers, for example, the immense amount of material written about education or voting behavior. This lack of interest probably reflects a cultural bias in those countries where the military has seldom been an important force in political or social life; and the lack persists despite the fact that writers such as Stanislaw Andreski (*Military Organization and Society* [Berkeley: University of California Press, 1968]) have powerfully argued the immense importance of the role of the military in all societies through their "bellic" interpretation of history.

Another factor is the sheer difficulty of carrying on research about the military. This is especially the case in Latin-American countries even though here the political and cultural role of the military is often very pronounced. In many cases the military forms a highly organized and disciplined organization with information flow limited essentially to the officer corps, a high degree of group solidarity, and immense political power. Thus research may be difficult and even dangerous, especially for local civilians. There is, therefore a tendency for research on the military to emphasize historical rather than contemporary topics, and rigorous sociological analyses of military institutions are very rare indeed in Latin America. Thus Alfred Stepan's major monograph on the Brazilian military must be considered as a significant event for anyone interested in that country. (An extensive article on his material was published in the February 9 edition of the Brazilian newsmagazine *VEJA*, which noted that no Brazilian publisher had yet shown interest in translation.)

Briefly, the focus of Stepan's work is on the interrelationship between the Brazilian military and the government from 1945 to 1968. Based on several years' study and field research on military organization in the country, it utilizes considerable archival material, including that of the Escola Superior de Guerra and of the late president, Humberto Castello Branco, as well as interviews with more than 200 officers. The book begins with an institutional analysis of the Brazilian military (mainly the army) and continues with a survey of the role of the military in recent Brazilian history, especially since 1964. Stepan adopts the concept of the military's

political role as that of "moderator" between various factions in the country (and in other nations as well) and proceeds to explain why, in his view, the moderating pattern broke down in the 1960s leading to the military *coup d'état* in Brazil in 1964. The final section of the book is devoted to a study of the nature of the military government in its first four years.

An important characteristic of the book is its consistent military-centered approach, possibly an artifact of his research design. This is valuable in sociologically analyzing the Brazilian army as an institution but creates some problems in historical analysis. While one should not, perhaps, fault an author for not extending his research beyond a self-selected point, it is still rather disappointing that Stepan with his exceptional sources of information should not have given us further glimpses into the history and development of the military in Brazil. The greatest weakness of the book, to me, is in its historical analysis. The concept of the "moderating power" of the military (and before that the emperor) is one of the favorite myths of Brazilian historiography and, like all myths, has a degree of truth. Certainly it is true that many of the military themselves in Latin America see this as their role. But the tremendous political impact of figures such as the Duque de Caxias, Floriano Peixoto, and Gois Monteiro, as well as the rise and fall of the dictatorship of Deodoro de Fonseca, is hard to explain on this basis alone. More helpful, to my mind, might be a study of factionalism within the military itself as well as a full analysis of the interrelationship between the military and other powerful institutions such as the great state governments with their own paramilitary organizations. It is in these matters, namely, the interrelationship between the military and other Brazilian institutions, that I find Stepan's work less useful than that of, for example, Thomas Skidmore (*Politics in Brazil 1930-1964* [New York: Oxford University Press, 1967]).

On the other hand, the work's great strength is its analysis of the Brazilian military itself, without much doubt the best that has ever been done. Chapters 1-3 form an institutional analysis of the size, organization, and social background of that organization which is unique and extraordinarily valuable as social data on the country. This kind of material done in such a systematic manner is very difficult to obtain in Brazil—a fact that is recognized in Stepan's final section on "Researching a Semi-closed Institution." The first three chapters alone make this book essential for anyone who seriously wishes to understand Brazil and its recent politics. In addition, Stepan's historical material, despite some limitations, offers extremely important insights into the operations and ideology of the Brazilian officers in recent years as well as the origins of the ideology and its changes through time (i.e., from the more "liberal" and U.S.-oriented Castello Branco presidency to the "hard line" military philosophy after 1968).

To sum up, I would say that this book is an essential study and reference book for anyone interested in contemporary Brazil, military sociology, and Latin America, but in some ways it falls short of being the definitive study of the Brazilian military.

Handbook of Military Institutions. Edited by Roger W. Little. Beverly Hills, Calif.: Sage Publications, 1971. Pp. 607. \$25.00.

Public Opinion and the Military Establishment. Edited by Charles C. Moskos, Jr. Beverly Hills, Calif.: Sage Publications, 1971. Pp. xvi+294. \$12.50 (cloth); \$7.50 (paper).

David R. Segal

University of Michigan

The essays in these books reflect the work of the Inter-University Seminar on Armed Forces and Society at the beginning of the second decade of its activities. Together, they indicate how far the field of military sociology has progressed since the seminar began operating in 1961 with a small group of sociologists as its total membership. More important, they indicate how far the field must still go to develop the kind of empirical base that characterizes more traditional areas of sociological inquiry. There is some promise in these pages that the seminar, now operating with a larger and cross-disciplinary membership, is attempting to fill the lacunae.

Roger Little's volume is intended to be a basic reference source, and Little sought to have the materials presented in the form of a propositional inventory. However, as the directorate of the Inter-University Seminar notes, "we are obviously dealing with an area in which theory has outrun empirical research" (p. 5), and the reader is left with little sense of the set of propositions that comprise the theory of military sociology, or the data that have been brought to bear on these propositions.

The contributions to the *Handbook* are grouped in four general parts: organizational structure; occupational socialization; organizational dynamics; political military functions. The placement of some of the essays seems arbitrary. Little's paper on "The Military Family, in part 2, and Amos Jordan's paper on "Troup Information and Indoctrination," in part 3, might well have been reversed, for example. Nevertheless, the *Handbook* does carry an aura of organization rather than merely being a collection.

In the first part, Morris Janowitz discusses the military institution and its constabulary role in an age of nuclear military technology. This is the clearest statement I have found of the sociological implications of the strategy of deterrence. Janowitz's discussion of models of the linkage between the military institution and social structure raises the empirical question of whether the United States fits a democratic model or the garrison state model.

Harold Wool, in "Military Manpower Procurement and Supply," reminds us that historically, most of America's military manpower needs have been filled through voluntary programs, and that a relatively small proportion have been conscripts. He also systematically presents the information available on the relationships between such variables as education, race, regional and socioeconomic origins, and military enlistment and reenlistment. While these relationships cannot be assumed to hold constant in the future, the

essay should shed light on the usually data-free speculation on the volunteer army.

Paul Nelson's paper on "Personnel Performance Prediction" will be of greater interests to psychologists than to sociologists. Indeed, his discussion of the marginal man in the military diverges greatly from the conventional sociological use of the term and makes it difficult to place his observation in the matrix of sociological theory.

Albert Biderman, in "The Retired Military," discusses the problems related to having large numbers of career military men retiring from the military at relatively young ages and entering civilian society and the civilian labor force. In addition to discussing the issue of second careers, including those as civilian employees of defense contractors, he reminds us that military retirement pay is a large portion of the defense budget, and that defense-related expenditures cannot be expected to disappear proportional to post-Vietnam demobilization.

Part 2 also begins with an essay by Janowitz. In "Basic Education and Youth Socialization in the Armed Forces" he points out that while using the military as an educational institution is probably less efficient than using schools as educational institutions, many people, particularly from lower socioeconomic backgrounds, who did not complete their secondary educations in civilian institutions, have been able to do so in the military. Dealing with the higher levels of the military hierarchy, Amos Jordan describes the institutions that provide the in-service education of our commissioned officers. While Janowitz points out that military training and education do not make our enlisted men authoritarian and Jordan argues that officers coming out of our command and staff colleges and war colleges "have played a major role in deterring nuclear war" (p. 241), we have to note that relatively few enlisted men and fewer officers refused to serve in an Asian war that the American nation seems to want no part of.

Little's paper on "The Military Family" confronts a set of sociological issues regarding the family in the context of the military and notes that the family is becoming more important to the military. As the military comes under increasing sociological scrutiny, the family should appear as one of the most common units of analysis.

Charles Mosko's discussion of "Minority Groups in Military Organization" deals primarily with the distribution of and relations between black and white soldiers in the years prior to our recent racial tensions in the military. Moskos notes that research is needed on other racial and ethnic minority groups and on women.

Of the three papers in part 3, Alexander George's discussion of "Primary Groups, Organization, and Military Performance" is the most sociologically relevant. In addition to having an important body of literature on the American army to discuss, George deals extensively with his own studies of the Chinese Communist army, thus casting the work in a comparative perspective that is too frequently lacking in this volume. A history of "Psychiatric and Social Work Services" in the army, by Bernard Wiest

and Donald Devis, that is interesting as history but frequently confuses sociology with social work, and Jordan's paper on troop information, which argues that most soldiers cannot be motivated to fight through ideological indoctrination, complete this section of the book.

In part 4, William Kornhauser presents a useful typology of revolutions. Franklin Osanka's essay on "Social Dynamics of Revolutionary Guerrilla Warfare" and Konrad Kellen's paper on "Psychological Warfare" are more like tactical descriptions than sociological analyses of unconventional warfare, although the latter author has interesting observations on the adversary relationship between the psychological warrior and the conventional soldier. Paul Blackstock and Martin Blumenson present interesting histories of "Covert Military Operations" and "The Function of the Military in Civil Disorders," respectively.

The book is uneven with regard both to the materials included and excluded. The most notable absence is a general treatment of social problems, and particularly the drug problem, in the military. The book is nevertheless a useful reference work for military sociology, and its utility is enhanced by a general bibliography and 22 pages of appended manpower statistics. Indeed, my major complaint is that given its utility, the book deserves a more careful proofreading than it received. The unevenness of the *Handbook* is a fair reflection of the field with which it deals.

The contents of Mosko's volume will surprise the reader who expects analyses of public opinion about the military. Only one paper, by Robert B. Smith, deals extensively with public opinion data. Smith compares popular reactions to World War II, Korea, and Vietnam, and demonstrates that in all three cases, the incumbent president increased his popularity through militancy. During the Korean and Vietnamese engagements, however, in contrast to the World War II experience, disaffection increased as the war dragged on.

More generally, Mosko's book deals with the interface of the civilian and military sectors of American society, and specifically with the appropriate role of the military in a societal context. In part 1, "Military Education and Civilian Values," Laurence Radway analyzes trends in the American service academies, Nona Glazer-Malbin and William Lucas present independent analyses of the ROTC, and Peter Karsten compares academy and ROTC students. These studies throw some light on the degree to which officer recruitment can affect the input of civilian sensibilities to the military, or, conversely, the bifurcations of military and civilian values. Unfortunately, Karsten's analyses seem to be predicated on the assumption that a volunteer army will necessarily be a professional army. In a similar vein, Glazer-Malbin discusses, and seems to bemoan, the low career-retention rate of officers recruited from nontraditional backgrounds. While such retention is clearly problematic, discussion of it should take place in the context of an estimate of the retention level that is optimal in terms of continual turnover at the junior officer level so that the officer corps does not become cluttered with older senior officers with no vacancies or career opportunities for younger personnel.

Part 2, "Civilian Response to Military Roles," includes Smith's contribution and three essays on the societal role of military institutions. Bernard Beck discusses the welfare functions of the military, David Sutton the minimal success achieved in fighting discrimination in host communities adjoining military installations, and Leon Bramson the functioning of the examining station as an interface institution that, among other activities, brings into the military people who do not want to be there and screens out people who do.

In Part 4, "The Emergent Military Establishment," Janowitz and Moskos independently arrive at projections of greater isolation and dissimilarity between the civilian and military sectors of American society. Janowitz, like Karsten, sees the volunteer army being made up predominantly of military professionals.

This book is the first in an annual series on war, revolution, and peace-keeping. It sets a standard that succeeding volumes will do well to meet.

Population Economics: Selected Essays of Joseph J. Spengler. Compiled by Robert S. Smith, Frank T. de Vyver, and William R. Allen. Durham, N.C.: Duke University Press, 1972. Pp. vi+536. \$17.50.

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Relatively few economists pay very much attention to sociologists. Particularly in his efforts to analyze the decision by couples whether to have additional children but also in other aspects of his research, Joseph J. Spengler is one of the few prominent economists who have tried to integrate their own work with that of sociologists. A past president of the American Economic Association and also of the Population Association of America, Spengler is best known for his work on population, economic development, and the history of economic thought.

The present volume represents a collection of Spengler's essays published in various journals from 1944 to 1966. The volume contains 17 works; three of these are contained in a section entitled "History of Thought," nine in a section entitled "Theory," and five in a section entitled "Development." At the end of the book is a list of all of Spengler's 239 publications.

A long essay which may be of particular interest to sociologists is entitled "Pareto on Population" and was first published in 1944. Spengler reviews all of Vilfredo Pareto's work to bring together his views on the interrelationships between population and other types of variables. This is a valuable contribution because those sociologists whose knowledge of Pareto is derived solely from Talcott Parson's *Structure of Social Action* (Glencoe, Ill.: Free Press, 1949) will not be familiar with this aspect of his thought. Spengler describes Pareto's use of existing data to show the substantial impact of economic forces upon mortality, nuptiality, and

fertility. However, since in Pareto's view of the social system economic factors were mutually interdependent with noneconomic factors, he did not believe that the relation between economic variables and population was asymmetric. Furthermore, he stressed noneconomic factors as important in fertility change. He maintained that an increase in the residue he termed "persistence of aggregates" tended to increase fertility, whereas an increase in the instinct for combinations tended to reduce it. Pareto was also interested in defining optimum population size for various desiderata. He maintained that the population-size optimum for individual welfare was smaller than that for military power. Spengler also describes Pareto's beliefs that substantial population growth was to the interest of the wealthy and that the rate of social mobility was greatest when the rate of childbearing among the wealthy was least.

Spengler has achieved renown for his work with respect to optimum population size and the effect of different population growth rates of economic development. The majority of essays in this volume are devoted to these themes and are written in technical language which unfortunately will be difficult for the average sociologist to understand.

Since the span of time in which they were written is so large, some of them are concerned with what was once considered to be a social problem for the developed nations—the too-low birth rate and the prospect of population decline. For example, "Some Economic Aspects of the Subsidization by the State of the Formation of 'Human Capital,'" first published in 1955, is concerned with the relative effect of subsidies for bearing children in the form of family allowance versus stamps granted to those having more than a certain minimum number of children and redeemable for a discount only on the purchase of goods necessary for child rearing. Spengler makes use of indifference-curve analysis to demonstrate the likelihood that more resources would actually be devoted to child rearing under the latter scheme than the former.

"Measures of Population Maladjustment," first presented at the 14th International Congress of Sociology and published in 1951, defends the thesis that one can ascertain whether a nation's population size is converging toward or diverging away from its optimum for per capita income despite the fact that it may be difficult to estimate the exact difference between its actual and optimum population. Spengler's proposed measure to ascertain whether a nation is or is not approaching its optimum population size makes use of changes in net output imputable to employees in industries subject to increasing returns to scale, decreasing returns to scale, and no changes in productivity with change in scale. Spengler maintains that a nation is diverging from its optimum population size when the weighted percentage change in output per employee in industries subject to increasing returns to scale is less than the weighted percentage change in output per employee in industries subject to declining returns after each has been adjusted for the increase in productivity of industries not subject to influence from change in scale, and where the weights are the proportion of all employees in the particular type of industry.

The editors are responsible for one feature of this book which is non-optimal—they indicated the date at which each essay was first published only in the foreword and not at the beginning of each piece. Moreover, in the foreword the dates are not easy to find since the titles of the essays are arranged not by page number but by name of publisher.

Aging and Modernization. Edited by Donald O. Cowgill and Lowell D. Holmes. New York: Appleton-Century-Crofts, 1972. Pp. xii+331. \$12.95.

Ethel Shanas

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Aging and Modernization is a collection of original chapters dealing with the aged and aging in a variety of societies ranging from Austria to the South African Bantu. The editors solicited these contributions in order to relate them to a middle-range theory of aging developed by Cowgill. Briefly, Cowgill believes that some aspects of aging and the situation of the aged in different societies are universals; other aspects of aging are relative to individual social and cultural contexts. The key element in the social and cultural context which affects the aged he calls "modernization," defined as the level of technology, degree of urbanization, rate of social change, and degree of Westernization of a society. Cowgill believes that, in general, Western societies emphasize individualistic achievement, and that such value systems place the older individual at a disadvantage compared with less Westernized societies in which the individual, aged, or not, is submerged into the group.

Unfortunately it is not possible to test this theory with the material in this volume since the various authors stress different aspects of aging in the societies studied, some considering the role of the aged, others giving general census-type descriptions, etc. Further, some authors make value judgments about the aged in Western societies which conflict with fact. For example: "Marked senility was rare among the Bantu of Southern Africa. Such as we witnessed from 1939 through 1967 was the direct result of drunkenness, drug addiction, or some organic brain damage. Senility such as is found among civilized people, where the aged of good health and sound mind lapse into states of self-pity, melancholy and moroseness, escaping into the past, and playing to the full the role of a useless, helpless unwanted appendage of society, did not appear in my observation of the Bantu in southern Africa" (p. 68). Or "Compare this [Samoa] with the often cited dilemma of the aged in our society who find themselves isolated from human contact by virtue of the fact that they must live in small boardinghouse rooms or in small flats in large impersonal apartment buildings" (p. 76). Such gratuitous and erroneous presentation of value judgments as fact makes one question the validity and reliability of the authors' reports of the non-Western cultures they are

describing. Can they be viewing these cultures, too, through distorting glasses?

In all fairness, it must be stated that many of the reports in this volume are well done, and some are exceptionally good. David Plath's paper on the aged in Japan is outstanding in its evaluation of both the Japanese culture and the evidence presented on the status of the aged; Walter McKain's report on old people in the USSR is balanced and thought provoking.

Finally, what of Cowgill's theory which states that there are some universals about aging in every culture and some aspects of aging which vary with modernization? Cowgill finds more variations among the situation of the aged than universals. In every culture, however, some people are classified as old and are treated differently because of this classification. In every society "the mores prescribe some mutual responsibility between old people and their adult children." The status of the aged is highest, however, where there are few aged, and it appears to be highest in preliterate societies not greatly affected by social change. The relationship between aged parents and adult children changes with modernization, with the state taking a greater role in providing economic support. In our contemporary world, preliterate societies not greatly affected by social change have almost vanished. Given Cowgill's insight into the situation of old people, it is unfortunate that he has not speculated more on the role of old people in contemporary societies following the suggestion of Plath, who says, "Modern society, in Japan as in many nations, has bestowed longevity. It has turned people loose into new lifespan territory. But it has equipped them only with medieval maps, full of freaks and monsters and imaginary harbors. The aged are among the true pioneers of our time, and pioneer life is notoriously brutal."

Information and Its Users: A Review with Special Reference to the Social Sciences. By J. M. Brittain. New York: John Wiley & Sons, 1971. Pp. xii +208. \$11.50.

Mark Oromaner

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The primary purpose of this work is to present a review of studies of information use, requirements, and needs of social scientists. Its author, Michael Brittain, is a psychologist presently associated with a group of social scientists and librarians at Bath University of Technology. While the "publication explosion" has received much attention in the form of discussion, Brittain could find no more than 18 empirical "user" studies in the social sciences. (User studies are studies of the use, demand, or need for information.) This, however, is an improvement over the findings of a 1965 review in which no studies of this type could be found. The most extensive studies of the social sciences are those concerned with psy-

chology. Only one of the 18 studies deals with sociology. This is a study of British sociologists and their interests by Carter. While the work of a sociologist, Herbert Menzel, is among the most influential in the field of scientific communication, the relative neglect by sociologists of the communication and information structure of their own discipline is quite surprising. This is particularly so when one thinks of the contributions made by sociologists to the study of mass communications. Brittain's review leads him to conclude that "there is no indication that user studies will proliferate in the social sciences as they have done in science and technology" (p. 162). The discrepancy between the social sciences and science and technology is, in part, due to the fact that "there has been nothing in the social sciences to match the interest in, and financial support of, information systems and services in science and technology by the United States Government" (p. 51). It is of course possible that this situation will change. Even in the absence of a shift in government interest the present introspective concerns of sociologists may increase the number of such studies in their discipline. A number of recent articles in the *American Sociologist* have made direct, if not indirect, contributions to this area.

Sociologists who are concerned with the present "crisis" in their disciplines may benefit from an examination of Brittain's work and in particular his chapter 4—"The Systematic Approach: Studies of Communication Artifacts." This chapter reviews studies on "the growth, size, obsolescence rate of social science literature, reference scattering and bibliographic coupling." Brittain correctly points out that a number of such studies exist in sociology as well as in the other social sciences. Once again readers of the *American Sociologist* are aware of the increasing publication of such studies. These studies are significant, for they permit an examination of the structure of schools of thought and invisible colleges in sociology. A recent contribution to this area is Diana Crane's work, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* (Chicago: University of Chicago Press, 1972). If the theoretical, individual, and departmental power bases in sociology are changing, such changes should be reflected in the publications of sociologists.

The text of this book is only 162 pages plus an excellent 35-page bibliography. The brevity of the text is, in part, due to the paucity of existing studies. I, however, would have appreciated a more detailed account of a number of the studies reviewed. One of Brittain's major themes is that "user studies have been conducted, both in the physical and social sciences, in the absence of strong theoretical or conceptual frameworks" (p. 162). Had Brittain presented the reviewed works in greater detail he may then have felt it necessary to at least attempt to correct this deficiency.

Thus, while this work does not go beyond a review of the field it will provide sociologists with an introduction to studies of information and communication in the sciences. It will, also, expose them to tools such as bibliographic coupling which may aid in an understanding of the changing structure of sociology itself.

III scoala sociologica de la Bucuresti. Bucuresti: Editura Stiintifica, 1971. Pp. 473. Lei 7.50.

Sociologica romaneasca azi. Bucuresti: Editura Stiintifica, 1971. Pp. 201. Lei 5.

Resurse umane ale intreprinderii. By Mihail Cernea, Maria Popescu, and Haralambie Ene. Bucuresti: Editura Politica, 1971. Pp. 288. Lei 8.25.

Sociologie pedagogica. By Leon Topa and Tiberiu Truter. Bucuresti: Editura Stiintifica, 1971. Pp. 139. Lei 8.50.

Teoria reglarilor sistemelor sociale: Elemente introductive. By Haralambie Ene. Bucuresti: Editura Academiei Republicii Socialiste Romania, 1972. Pp. 202. Lei 8.50.

A.B.C.-ul investigatiei sociologice. Vol. 1.: *Prolegomene epistemologice.* By Achim Miha. Cluj: Editura Dacia, 1971. Pp. 275. Lei 7.

Jiri Kolaja

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Before World War II Romania was almost 80% rural—today it is only 49% rural; it was a prewar kingdom but today is ruled by the Communist party; and now Romania has almost four times as many students at the universities as it had before World War II.

Because of the studies D. Gusti and his followers have made of rural communities Romanian sociology has made a name for itself in Eastern Europe. Gusti involved practitioners of many different disciplines among his co-workers: physicians, geographers, and others; in my judgment his studies are best described as being close to cultural anthropologists. In *Scoala sociologica de la Bucuresti*, students of Gusti, H. H. Stahl and T. Herseni, have written methodological and theoretical treatises on his work. The regional monograph used by Gusti is discussed by Herseni, and H. H. Stahl examines his approach to population analysis, economic diagnosis, health-reporting methods, cultural typology of Romanian communities, styles of rural architecture, and identification of peasant economy.

One needs to be reminded that sociology was not favored in the years after World War II; hence, Gusti with his organismic and cooperation theory on rural communities tended to be ignored. In *Soziologia romaneasca azi*, D. Gusti receives the recognition he has long deserved. In this same book, I. Matei writes about territorial developments and R. Moldovan discusses planning.

In the two previous books, sociology is conceived within a more historical perspective; in *Scoala sociologica de la Bucuresti* O. Neamtu as well as A. Golopenta have chapters that deal with the application of sociology in regard to future developments of Romanian villages. For example,

Neamtu exemplifies the application of the motion picture to facilitate change.

Not only the problem of the movement of the rural population to urban centers but also the related problems of industrialization and education have attracted the attention of Romanian sociologists. *Resurse umane ale intreprinderii* is one of several industrial sociology books produced at the present time in Romania. In M. Cernea's chapter, one is informed that the productivity of the Romanian worker is only about one-third that of workers in advanced countries. M. Popescu, influenced by W. Landecker's classification of integration, openly discusses poor productive performance of previous farm workers. Likewise, H. Ene's report on graduate engineers frankly explains that their absence of innovativeness is because of poor remuneration. Moreover, the engineers, in the two enterprises investigated, felt that much of their daily work could have been performed by less educated persons. One should notice that this book openly discusses dysfunctional factors in Romanian enterprises. Certainly, a more open discussion of deficiencies contributes to the possibility of improvements. It is worthwhile to observe, however, that there was no reference made to any professional organization such as a labor union.

The significance to be attached to education in Romania today is captured by keeping in mind that 160,000 students is about four times the pre-World War II university population. Characteristically in *Sociologie pedagogica*, Topa and Truter refer several times to Durkheim's idea of education, as well as contemporary sociologists such as W. B. Brookover, H. Schelski, R. Girod, etc. One also notes a skillful combination of references to Durkheim, Weber, and most other Western sociologists. As I have observed, in other books published today in Romania, Romanian sociologists more or less skillfully focus on common elements in Marxist and Western non-Marxist thinkers. For example, in this book sociometric research data is integrated with a discussion of possible relations, such as between teachers and pupils, among teachers themselves, and between teachers and parents. In reading this I was reminded of the system of different social relations as elaborated, for example, by von Wiese.

Though urbanization, industrialization, and education are problems that justifiably occupy the attention of Romanian sociologists, a historic, major change—the rule of the Communist party—has with one exception (which is described in a small investigation of political training) produced no empirical studies of the political behavior in Romania.

The last two books deal with sociological theory. H. Ene in his *Teoria reglarilor sistemelor sociale* is aware of the increased usage of the concept, "system," and deals with such authors on the subject as Ludwig von Bertalanffy, A. Rapaport, and N. Wiener, as well as sociologists such as W. Buckley, A. Etzioni, and J. Stoetzel. Referring to 35 different concepts of system, he defines structure as an essential part of the concept of system. Self-regulation as well as diachronic phenomena attract his attention.

The most original among the Romanian books listed in this review appears to me to be A. Miha's *A.B.C.-ul investigatiei sociologice*. First, one

should list structuralism or, as Mihiu terms it, "megastructure." He stresses that parts in certain respects are units of a category, while from another viewpoint are made up of smaller subunits or are units of a larger whole. He thinks that Western structuralism, especially American, tends to be static. He is therefore eager to point out advantages of a genetic structuralism.

Another epistemological problem is the grounding of social action in the human person. This is manifested not only in one level of action, but in several levels. The person, for example, transcends particular actions, the action is multilevel. To illustrate this he discusses instances of four sociological phenomena: social personality, interpersonal relations, group, and community (pp. 127-37). To explain his concept of social personality, called by Mihiu "*socius-ul*," he clarifies it by reference to K. Lewin's notion of personality which is conceived as a phenomena that is not sufficiently defined by references to social roles.

I do not feel that Mihiu has resolved the problems of the human person, or as he calls it, factor, satisfactorily. If we admit that each scientific discipline introduces a certain degree of abstraction, we can wonder if Mihiu has not moved on to some other fields such as psychology or ethics. Moreover, though Mihiu mentions on several occasions that Marxism has helped him to overcome certain limits of structuralism, I think that existentialism could just as well have been listed, if it was not in fact a stronger influence.

Considering Mihiu's system further, I would point out that we are presented with a theory of action that has a more extensive set of categories than those developed by Parsons. For example, the "object" is subdivided into several categories such as scope, motivation, values, norms, and energy. Thus, Mihiu indicates some possibilities that the general theory of action has not been exhausted. His analytical differentiation between an act itself, its logical or qualitative aspect, and its temporal and ecological dimensions undoubtedly opens other classificatory possibilities. I have found his diagonal table of projection of action and its realization quite helpful (p. 98).

It appears that Dr. Mihiu, who was in the United States for a year, not only studied the general theory of action, but also several critiques of action theory. Mihiu's system is definitely less closed logically than Parsons's. As Parsons used his concepts, they are more logically integrated. They form a system. Mihiu's system of analytical categories is certainly more empirically conceived but, unfortunately, his categories do not define each other and do not form a "system." In other words, his categories are insightful but their degree of systemization is low.

Reflecting on the six Romanian books, we can summarily point out that the delayed and posthumous recognition to D. Gusti, thus far the most outstanding Romanian sociologist, indicates an increasing maturity and a capacity for scientific objectivity in the Romanian sociology.

The attention given by Romanian sociologists to urbanization, industrialization, and education is founded in the social change occurring in that country today. Maturity is evident in that Romanian authors tend

to report openly the negative aspects of their research works. Their major problem is the study of productivity. I would say that the Romanian interest corresponds somewhat to Frederick Taylor's interest. They can also be criticized for overlooking other variables in their industrial output studies.

The Denominational Society: A Sociological Approach to Religion in America. By Andrew M. Greeley. Glenview, Ill.: Scott, Foresman & Co., 1972. Pp. iii+266. \$6.95.

David E. Payne

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Professor Andrew Greeley indicates in the preface of this book that "it does not pretend to be a comprehensive textbook on the sociology of religion" (p. ii). It does intend to provide beginning students and interested laymen with one orientation for interpreting American religious behavior. The thesis Greeley is explicating is that the unusual vitality and resilience of American denominations can be understood primarily by examining one function he believes they provide, namely, belonging. For various reasons this function is not as prominent in most other countries of the world.

The first three chapters constitute the author's introduction to the book. First, traditional views of the nature of religion are summarized. Next, relevant theorists are grouped into three schools, based on the function of religion they emphasize (meaning, comfort, or belonging), and their contributions are reviewed. Finally, the church-sect typology with its historical variations is discussed. These three chapters are generally well organized and accurate. They do contain a rather large proportion of quoted material, however.

Chapter 4 is devoted to explaining the denominational composition of the United States and indicating the socioeconomic structure and general orientation of each denomination.

Chapter 5 forms the heart of the support for Greeley's basic thesis. He proposes that American denominations are ethnically oriented. They are middle-level *gemeinschaft* organizations with which immigrants can identify in the *gesellschaft* American society into which they move. This is, he maintains, as true for subgroups within Catholicism as it is for denominations within Protestantism.

Chapter 6 reviews data which indicate that religious denominations are retaining their influence despite declining immigration rates and growing secular knowledge. Greeley maintains that this is comprehensible because of their stress on belonging rather than on meaning function. One can thus see why "people can go to church more and simultaneously say that religion is less important" (p. 138).

The next three chapters seem to be only slightly related to the underlying thesis of the book. Large portions of these chapters are again quoted

from various other sources. In chapter 7, Americanism as a national religion is discussed; in chapter 8, the diversity of American denominationism is explored; in chapter 9, the conditions under which religious variables lead to conflict and prejudice are commented upon. It is further suggested that since denominational society will continue in America for some time, some conflict is also likely to continue.

Chapter 10 is designated as a summary chapter. Greeley shows how the American child acquires a sense of the American national religion. The child then acquires a sense of individuality and belonging from his particular denomination. In the final chapter, findings relating to several aspects of the thesis are discussed, and several hypotheses which follow logically from it are proposed for testing.

Since the book is designed for the beginning student and the interested layman, the very brief methodological section and the nontechnical application of statistics are not inappropriate. One might, however, wonder about the comparability of data from different surveys representing different time periods, different samples, and different locations.

The vast majority of the data as well as the theoretical and bibliographic materials in the book come from the late fifties and early sixties. The reader with a background in the sociology of religion may get the feeling that the book itself comes from that era.

The basic idea of the book is theoretically interesting and quite clearly developed in chapter 5. The other chapters are related to the thesis with considerably less directness. While these other chapters are interesting and informative for the intended audience, they leave the book without continuity and integration.

In short, the book will provoke the student and interested laymen to thought about the thesis. It will also provide them with general information about religion in America and the sociology of religion. It probably will provide the sociologist with background in the area with methodological questions but the feeling he is covering something very familiar—from the past.

The Sociology of Education: An Introduction. By Ivor Morrish. New York: Barnes & Noble, 1972. Pp. 304. \$14.50.

Patricia Cayo Sexton

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Reviewing a textbook is somewhat like reviewing sociological abstracts or a journal of book reviews. Most texts, after all, are essentially critical summaries of a field, rather than records of original research or thought. Textbook writers do, however, select and interpret, as well as review and abstract, a vast body of knowledge in their disciplines. In education, that body is so enormous as to be almost beyond the digestive capacity of mortal scholars. So, some credit should go to anyone who writes "the sociology of education."

Ivor Morrish's new text deserves a good deal more credit than that. For "An Introduction," and such a small one at that (304 pages), it is surprisingly comprehensive. It deals with most major concepts in the field and offers extensive references for those who wish to go beyond the text. Among its subjects are the relation of education to culture, social change, politics and the state, the economy, stratification and mobility, the family, socialization and the development of self—as well as internal matters of school organization, teacher roles, classroom dynamics. It also introduces discussions of curriculum and social pathology, subjects not commonly dealt with in such texts.

It does all this without sounding too abstract or abbreviated. Its style—lucid and even relaxed—seems quite British, and appropriately so since Mr. Morrish is on the faculty of La Sainte Union College of Education at Southampton, England. But its Britishness is both a large asset and a limitation. In the study of social institutions, such as schools, few perspectives are more illuminating than the comparative one, for it offers contrasts against which we may see our own institutions. Morrish's book gives us a British perspective, mixed with many enriching examples from both the United States and Russia. Especially interesting are his few references to the impact of political parties on education, as seen, for example, in the role played by Labour in the development of comprehensive schools and the Open University.

No book about "other" schools, however, can be right on target if we are aiming at "our own." We cannot, for instance, say much about American schools, or about sociological analysis of them, without dealing, initially and basically, with the difficult subject of race. Yet race is not that central to British education, so Morrish treats the subjects only in passing and does not mention the word "Negro," or its equivalents, at all.

In the United States the "growing edge" of sociological inquiry into education has been concerned with the impact of schools on academic learning (or any kind of learning); job performance; equality of opportunity (in competition for jobs and other rewards); and equality in the society (mainly in the distribution of wealth and income). The conclusion, or implication, in much of this discussion is that the schools have little or no impact (therefore are not really *vital* institutions), and that heredity, intelligence, parents—even chance—count for more. Both "left" and "right" converge on the same policy conclusion, the extreme statement of which is "abolish the public school system." The left's view is: schools don't matter much compared to "revolution" and "basic change in the social system." The right's view is: school don't matter much compared to hereditary intelligence and family background. However misshapen the research, and the conclusions from it, such is the frame of reference in the discussion of American schools. It is not exactly the frame of Morrish's book, although he does come to it occasionally: "The home and social class influences are stronger than those of the school and are in fact taken into the school" (p. 136).

In the United States there is also more stress on the disjunctures be-

tween school and the outside world, perhaps because so much time is spent in schools here. Those who question the goodness-of-fit between school and life, and those who search for ways to get students out of classrooms and award academic credit for "life experience," would be inclined to question Morrish's view that "the school is not simply a training ground for life and the larger society; it is life itself, and social living" (p. 118).

Also, perhaps because he could not include everything, he has neglected two other significant subjects. One is the relation between education and religion/ideology. The word "religion" is mentioned often enough, but little systematic treatment is given to the "roots" of most public education, which are deeply imbedded in religious and ideological instruction. This is a strange oversight in view of the fact that Morrish himself has had considerable theological instruction.

A second neglected subject is one that interests me, if not him; that is, the feminine culture of the school. He has, however, titled one chapter "The Teacher and *His* Role" (my italics). This suggestive male pronoun might have offered us another useful contrast with American schools. A third omission was, I trust, a publisher's accident. The chapter billed "The Family and Education" in the table of contents was completely missing in the copy of the book I received. None of these omissions detracts in the least from the generally high quality and usefulness of Morrish's text.

The Family. By Robert O. Blood. New York: Free Press, 1972. Pp. vii+694. \$10.95.

Elise Boulding

University of Colorado

Robert Blood's innovative approach in using materials from many cultures not only for teaching about family structure but also about family process makes *The Family* a unique textbook. In the days that the family was treated as a social institution, the comparative and historical approach was extensively used, usually to show that the family was evolving to a new higher form in the 20th century. Carle Zimmerman's *Family and Civilization* (1947) was a landmark in sophisticated historical research that demolished simplistic notions of family evolution. In the fifties the trend was toward treating the family as process, with increasing emphasis on courtship, marital interaction, and socialization processes. The historical and comparative aspect of the family were confined to an introductory chapter or two. The incorporation into these texts of an increasing volume of research on family processes, often utilizing families of college students meant that the picture of the family presented to students was that of the middle-class white American. With the spotlight on the modern American family, an implicit evolutionary model was creeping in again.

William Goode's cross-cultural study of *World Revolution and Family Patterns* was hurled like a thunderbolt into the midst of these cozy presen-

tations of the American family. Just as Zimmerman used history with much more sophistication than his contemporaries, so Goode dealt with evolutionary theory in a much more sophisticated way than his contemporaries and used a much broader cultural base. With an increased awareness of modern cultural forms in other parts of the world has come an increase in awareness of a diversity of subcultures in the United States. The net result is that family textbooks are now increasingly dealing with American minority subcultures, including the commune culture, but the international dimension has slipped out of focus again.

Blood may help to rescue us from this new, more sophisticated ethnocentrism. He combines the best of cross-cultural structural analysis with the best of cross-cultural process analysis in his presentation of research on the family. Because most teachers are not accustomed to dealing with such a wealth of cross-cultural material, they will need to allow some time for familiarizing themselves with the material before using it in class.

To summarize what the book does, Blood sets out "to understand . . . why the family is the way it is under varying social conditions and to make sense out of the internal structures and processes involved in family life." He does this in two parts, first by examining the diverse cultural forms of the family in terms of the institutional environments they find themselves in around the world, and second by examining diverse ways of organization and functioning of the family as a small group, independently of specific cultural milieus. In a way the book is a huge data bank, consisting of research reports from a number of cultures which answer the whys of diversity. One is guided through this research by a simple commonsense outline dealing first with external systems as they impinge on the family: economic, stratification, political, religious, educational, community, and kinship systems. The second part uses the language of social organization and control to deal with family patterning and process, treating mate selection as personnel recruitment, marriage as organization enactment, the addition of children as organization expansion, variations on the two-parent family as leadership patterns, and so on.

The simplicity and parsimoniousness of these concepts are a great aid to the student confronted with a massive amount of information about families. No other family textbook comes anywhere near this one in comprehensiveness of treatment of the 20th-century family. I regret that he did not include summaries after each section, however, and an overall summary chapter at the end to help the student (and the teacher!) integrate the great range of material covered.

The book largely assumes knowledge of the larger macrosystem trends of the 20th century as they are delineated, for example, in Goode. One could argue that it is not necessary to take account of those trends if one is simply focusing on the family in its present functioning. However, since different cultural regions of the world are in very different situations with regard to urbanization, industrialization, and population growth, it is really necessary to know something about that in order to make sense of cross-cultural comparisons which include African and Asian as well as Latin

American, North American, and European families. I hope that in a future edition of this book Blood will include some macrosocial data to help the student develop adequate mental images of societies in various parts of the world.

There is not much attention to the presentation of conceptual frameworks for viewing the family. The implicit structural-functional approach, and the language of social organization, is helpful but does not deal adequately with conceptual problems. The student does need help in finding ways to think about the information he is given—and ways to code and organize that information in his own mind. Rather than emphasizing this as a weakness, however, I would suggest that it offers the opportunity for creative exercises in conceptualization that any professor could initiate with his or her class in the course of teaching with this book.

The plight and the strengths of the relatively isolated conjugal family struggling to meet the needs of its members in the absence of extended family and community support of a kind available in earlier eras of less mobility come out very vividly in the cross-cultural presentations. The emphasis on love and personal choice in mate selection, which Blood, Goode, and most writers on the modern family treat as basic phenomena in understanding the modern family, struck me as particularly anachronistic when it receives this extra cross-cultural emphasis. Anachronistic, because this question is never raised: "Why, in a world that sets great value on interpersonal competence, does free choice, and commitment to expression of personal affection, lead to a decline in interpersonal competence in the family as a group?" I am suggesting that the rising divorce rate might be treated as a decline in interpersonal competence and that the doctrine of the love-based marriage choice as leading to more emotionally supportive family groups may need some reexamination. One strength of Blood's book is that such issues can be reexamined with the data he presents and discussed in a variety of ways in the classroom.

It is a temptation to complain of informative overload after reading this book, but it is a temptation I will resist because the real problem is rather in the other direction: how to draw on enough different sources of information about the family to develop an adequate global image of family structure and process. There seems to be a convergence of opinion among scholars active in the Committee on Family Research of the International Sociological Association that research published in the English language substantially reflects all significant research on the family. William Goode makes this point in his introduction to the monumental *Social Systems and Family Patterns*, and Rene Konig reaffirms it in his summary reflections in *Families in East and West*. There is no need to belittle the tremendous advances in knowledge made possible by the work of these scholars and the women and men associated with them in the ISA Committee. I do, however, question the correctness of the assumption concerning the adequacy of the research in covering significant knowledge about the family. A UNESCO conference on social change back in the 1950s provided an eloquent documentation of cultural bias in social science re-

search that has greatly impeded planning and development in nonindustrial societies. These findings have been confirmed again and again in recent years as specific failures to identify actual practices of planning, resource, and role allocation in both families and in communities have been uncovered through dramatic modernization failures. The problem was not that the relevant knowledge was not available, but that Western-trained social scientists were not paying attention to it. They wrote up their own observations and read only each other's work.

I see no reason to believe that family sociologists are doing better, but their work has not been exposed to the glare of large-scale planning failures. More careful attention to research not conceived in the major universities of the West is probably called for. By amassing so much of the Western-oriented research around the world Blood makes it possible to see more clearly that we are casting all of our knowledge about the family in a few well-tried molds. Space prohibits documenting this, but assumptions about kin structures and their relationships to property, social mobility, and achievement motivation are all cast into doubt by Eberhard's discoveries that many Turkish villages assumed to be of ancient vintage are recently (within the last 100 years) settled nomadic tribes that had no traditions of private property. Similarly, analyses of family structure in terms of women's roles are cast into doubt by work such as Ester Boserup's *Woman's Role in Economic Development*, Mary Beard's *Woman as a Force in History*, Barbara Ward's *Women of the New Asia*, and Harriet Holter's *Sex Roles and Social Structure*.

It is hard to write a textbook in a time of rapid social change. Recent work on the relationship between sex roles and social structure makes obsolete much of the language used by Blood in referring to family role structure. Equating "breadwinner," "head of household," with "husband" and describing this person as the family's major contact with the outside world, which Blood does throughout his book, won't do any more. Neither will those old chestnuts of Zelditch, the instrumental (male) and expressive (female) roles as family universals—another frequently used pair of concepts. The male's-eye view of sex in marriage, drawing heavily on Kinsey and the old stereotypes of female passivity, will inevitably meet a lot of criticism. The analysis of alternate family forms in terms of Caribbean societies and the ex-slave society pathology approach fails to do justice to the viability of a variety of household forms and to the increasing frequency of alternative family form in American society. Blood does point out, and correctly, that we don't have much research information on these phenomena yet.

Blood and I are of the same generation and share certain common problems of our generation. In spite of a real commitment to personal openness and sensitivity to the new experiences of new generations, we have a certain Anglo middle-class stubbornness about what the world is like that makes it hard for us to give an adequate presentation of trends which challenge that world view.

The strengths of the book as a teaching instrument are substantial. Any

sociologist who takes seriously the task of helping students understand social phenomena in transcultural contexts will find this book indispensable to his personal library. As a tool for gathering new understandings about the family, it offers a challenge and opportunity to teachers who like to work with a broad canvas.

Community Studies: An Introduction to the Sociology of the Local Community. By Colin Bell and Howard Newby. New York: Praeger Publishers, 1972. Pp. 262. \$8.95.

Charles M. Bonjean

University of Texas, Austin

From several standpoints this is an attractive addition to the rather small universe of textbooks designed for undergraduate courses on the community. Its chief merits are a straightforward and logical organization enabling a succinct yet fairly comprehensive review of the most relevant literature and a lively style focusing upon both continuity and disagreement in this substantive area. In addition, the attempt by Bell and Newby, both at the University of Essex, to point up the similarities and differences of North-American and European community studies takes the book beyond those dealing primarily with the American community.

The introductory chapter previews the issues to be treated in later chapters, discusses the criteria used in their selection of studies to be reviewed (those most important studies which raise most of the major theoretical and methodological problems and are concerned with the study of the interrelationships of institutions in a locality), and make explicit their own theoretical perspective—sociology of knowledge.

"Theories of Community" are treated first. An attempt is made to relate the works of some of the giants (e.g., Comte, Durkheim, Maine, Tonnies, and Parsons) to a concern with community. Although the attempt is admirable and some of the summaries are illuminating, in many cases the reader is likely to conclude that the degree to which sociological theory has impinged on the concept of community equals the degree to which the authors occasionally seem to show this relationship. After reviewing attempts to define community (and not surprisingly declining to offer a definition themselves), they identify and describe five approaches to community study: the ecological approach, communities as organizations, communities as microcosms, community study as method, and communities as types.

"Community Study as a Method of Empirical Investigation" stresses the eclectic techniques of community researchers, but focuses primarily on participant observation and on how both the investigator and his informants influence the findings of community studies. A number of the epistemological points made in this chapter are illustrated in the four substantive chapters which follow.

The two longest chapters comprising the central substantive aspect of the book deal with "The American Studies" and "The European Studies." The former describes the two Middletown volumes, the Chicago school (with perhaps an exaggerated emphasis on Zorbaugh to illustrate some of their own views), Yankee City, Springdale, and others. The European studies summarized are those available in English dealing with communities in England, Ireland, Wales, France, the Netherlands, and Southern Italy. Although the coverage is generally good, perceived errors of omission are likely to vary by reader. On the North American side, I believe some reference to Hollingshead would have been in order and at least a paragraph on the original Plainville study and its restudy would seem to have been called for. On the European side, it is regrettable that Booth's pioneer study is overlooked.

Separate chapters are offered on those aspects of community structure and process that appear to be central in the literature—"Local Social Stratification" and "... Community Power and Community Conflict." Much of the chapter on stratification summarizes the work of Warner, Rex and Moore, and Pahl. The former study is criticized (mainly by summarizing the criticisms of others), while the latter are discussed in a more favorable context in part to make the basic point that "it is unlikely that there will be a consensus of 'local subjective' models of stratification" (pp. 207-8). Both the elitist and the pluralist approaches to the study of community power are described and criticized, but the emphasis is on the debate which raged between the two schools in the early sixties: "So convinced are the proponents of their own righteousness and of the ideological and methodological iniquities of their opponents, that hardly ever does it seem to be contemplated that perhaps communities differ" (p. 241). Overlooked by the authors are those outstanding works which have combined aspects of the reputational and decisional methods (e.g., Aggar, Goldrich, and Swanson) and the increasing number of comparative studies attempting to relate variation in power structure to other community characteristics (e.g., research by Clark, Walton, Aiken, Gilbert, and others). The result is a somewhat outdated view of the study of community power in the United States.

While the sociology of knowledge approach taken by the authors is fruitful more often than not, it may have a few drawbacks. Its major weakness, intended or not, is that it has apparently led Bell and Newby to implicitly advocate an atheoretical (if not antitheoretical) perspective at points in their discussion. The authors suggest that theoretical perspectives are "blinkers" which limit the field of vision (p. 63) and thus(?) more often than not the studies which are applauded are those with no theoretical perspective. Rather, the heuristic value of holistic studies is emphasized and the conceptual or theoretical contributions occasionally seem slighted. A related problem is their search for "the tensions and strains of the field, particularly the conflict which has been one of its central themes" (p. 250). This perspective, too, may act as "a pair of blinkers" and perhaps explains, for example, the focus in the chapter on

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The Coup d'Etat in Theory and Practice—Wells

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IN THIS ISSUE

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A Test of Lindesmith's Theory of Addiction: The Frequency of Euphoria among Long-Term Addicts¹

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Lindesmith and others claim that once physical dependence is established addicts do not experience euphoria. Consequently, euphoria cannot explain chronic addiction. Data are presented to show that, contrary to this view, long-term addicts experience euphoria frequently, crave it, and act to obtain it. "Lack of money" is the most important reason addicts give for not experiencing euphoria more often. Based on success in achieving euphoria, two classes of addicts are identified. The sources of income of addicts who experience euphoria most often correspond to those of types described by others as highest in prestige. Analysis suggests an addict stratification system founded on the two major psychopharmacological phenomena of opiates: withdrawal and euphoria. Addicts who barely succeed at tending to their daily need to avoid withdrawal are lowest in prestige. In the higher prestige ranges, addicts are stratified by their success in achieving euphoria. Thus, the social as well as the value system of addicts owes much to success at achieving what are universally considered, at the individual level, to be the most fundamental reinforcers. Since these reinforcers operate at the individual level, our analysis reveals the addict social system as a microcosm of broader theoretical interest, with transitions between physiological, psychological, cultural, economic, and sociological phenomena in plain view.

INTRODUCTION

The hold that opiates exert over the user has long puzzled observers. At one time, it was believed that intensely pleasurable sensations, in addition to physical dependence and the distress of withdrawal symptoms, were operative in sustaining chronic opiate use. For some 30 years, however, the

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dominant sociological account of opiate addiction (which has found acceptance far beyond the boundaries of sociology) has maintained that pleasurable effects are important only in the early stages of opiate use, after which the need to avoid pain predominates. It is obvious that there are important differences between a theory of addiction based at least in part on pleasure and one based entirely on the avoidance of pain. On the practical side, moreover, the chronic addict who enjoys opiates may be a quite different epidemiological entity from the unfortunate who pays for an earlier self-indulgence with a perpetual, joyless struggle against the agony of withdrawal. Clearly, current proposals to combat addiction with outpatient programs that provide addicts with heroin make it important to be correct about the relation between opiates and pleasure.

Lindesmith's Theory

The major sociological theory of opiate addiction is Lindesmith's (1938, 1947, 1965, 1968). This article is based on its latest revision (1968), although in all major respects the essentials of the theory have changed little over the years. Since it first appeared, Lindesmith's theory has been one of the most comprehensive and well-integrated analyses of addiction available in any literature. Although a few sociologists (e.g., Duster 1970; Robinson 1951; Turner 1953) have been critical of some formal and conceptual aspects of this theory, they have not challenged its empirical foundation. Ausubel (1958) and Scher (1966) have questioned Lindesmith's treatment of the role of euphoria, but neither offered any empirical evidence to support his objections. Although there are other major works on opiate addiction, such as that by Chein et al. (1964), which treat topics not considered by him, Lindesmith's theory currently stands virtually uncontested among sociologists (see, for example, Grupp 1969). In this paper, our concern will be mainly with his treatment of the two most fundamental psychopharmacological effects of opiates: euphoria and withdrawal sickness.

Euphoria and Withdrawal Sickness in Lindesmith's Theory

According to Lindesmith (1968, pp. 24-45), initial doses of opiates are "usually but not always pleasurable." After the first few doses any unpleasant effects disappear, and the person enters what he calls the "honeymoon period," which lasts several weeks (Lindesmith and Strauss 1968, p. 195). During this time, the person "increases the size of his dose and for a time experiences a more intense euphoria." Repeated daily use for several weeks results in physiological dependence, marked by acutely unpleasant physical symptoms when the drug is not used. These symptoms are known as "withdrawal sickness."

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Once physiological dependence develops, according to the theory (1968, p. 31), a "reversal of effects" occurs. Lindesmith describes this reversal as follows: "In the beginning phases of addiction, the pleasurable effects of drugs, other than those that occur at the time of injection, tend to diminish and vanish. As this occurs, and withdrawal increases, the psychological significance of the doses changes. Whereas they at first produced pleasure, their primary function becomes that of avoiding . . . withdrawal." In a key passage he further argues that an explanation of addiction cannot be based on the positive effects of the drug "if these effects are reversed or vanish when addiction is established" (1968, p. 33). As a consequence of this reasoning, he has tended to rely heavily, if not exclusively, upon the avoidance of withdrawal symptoms as the motivation for continued addiction: "The craving for drugs . . . is fixed by negative rather than positive reinforcement, by relief and avoidance of discomfort and pain rather than by positive pleasure" (p. 95).

Most writers accept Lindesmith's view concerning euphoria (see, for example, Akers, Burgess, and Johnson 1968; Chein et al. 1964, pp. 14, 248; Duster 1970, p. 59; Grupp 1969; Lingeman 1969, p. 105; Martindale and Martindale 1971, p. 270; O'Donnell 1969, p. 256; Rubington 1968, p. 306; Schur 1965, p. 122; Tardola 1970, p. 50; Wikler 1965, p. 87; Zinberg 1973). In fact, Nyswander (1959) has reported that British physicians define a drug addict as someone who feels normal on drugs.

However, the theoretical rationale for the use of various chemotherapies, such as methadone maintenance and naloxone, hinges on the ability of these drugs to *block the euphoric effects of opiates* (Dole, Nyswander, and Kreek 1966; Hammond 1971). A major goal of this paper is to resolve this apparent inconsistency by determining the true facts concerning euphoria. We shall demonstrate that despite the development of tolerance *chronic opiate addicts do experience euphoria following injections*, and that their desire for euphoria appears to be a major factor in the explanation of their behavior.

METHODOLOGY

Recruitment of Respondents

This report draws on two surveys of addicts in Baltimore. Since all of the quantitative data in the report are taken from one of these surveys, that sample is the one described here. Our interview excerpts, however, are drawn from the second sample, which resembles the first, and it will be described elsewhere (McAuliffe, Gordon, and Doering 1973).

According to Nurco and Balter's (1969) ecological analysis of narcotic addiction in Baltimore, black addicts are concentrated most heavily in the western half of the center of the city, while white addicts are dispersed in

small concentrations in all four quadrants of the city. By employing interviewers who had gained special access to loosely knit addict groups or "copping communities" (Hughes and Jaffe 1971), we were able to sample this addict population widely while maintaining a high degree of rapport between interviewer and respondent. Between November 1970 and June 1972 we succeeded in recruiting 64 respondents from natural addict groups in all of these areas.

Twenty-two of our addicts were recruited from four separate groups by a participant observer who had been mingling with many groups of Baltimore street addicts almost daily for more than five years.² Sixteen more were recruited from still another natural addict group by a second participant observer.³ The remaining 26 were recruited and interviewed by a college-educated ex-addict, known to be reliable (since he was a longtime friend of one of the authors). Seventeen of these last addicts were members of the interviewer's own heroin-using social network; the other nine were parolees whom he met through a heroin-using friend. Thus, our samples were quite dispersed.

Respondent Characteristics

Sixty of the respondents were male and four were female. Their ages ranged from 17 to 42 years, with a mean of 24.7 years and a standard deviation of 4.86. Thirty-six were Protestant, 19 were Catholic, and seven were Jewish.⁴ Fifty-one held blue-collar occupations and 11 white-collar occupations when employed. The mean Real Prestige Score (based on occupation) for the sample was 32.1 (Siegel, Rossi, and Hodge, forthcoming).⁵ At the time of interview, 24 were employed. Forty-seven of the addicts in our sample were white and 17 were black.

When interviewed, all of the respondents but six were using illicitly obtained opiates exclusively.⁶ The average amount spent per day on opiates by all addicts was approximately \$40. Because our interviewers contacted

² For a detailed description of the groups from which these 22 addicts were recruited, see McAuliffe, Gordon, and Doering (1973).

³ For a journalistic description of this addict group, see Rozhon (1972).

⁴ Demographic data were unavailable for two respondents.

⁵ The prestige scores for this sample of addicts ranged from 52.4 (probation officer) down to 14.4 (busboy). The mean of 32.1 falls exactly at the score for truck driver, and the standard deviation of the scores was 10.6. The lowest white-collar occupation had a prestige score of 45.8 (secretary).

⁶ Fifty-one used heroin, four used dilaudid, and three were using street methadone. The six remaining were receiving legal methadone from maintenance rehabilitation programs, but only three of them were not using illegal drugs too. High rates of cheating among methadone program addicts have in fact been reported by Taylor, Bowling, and Mason (1971).

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the respondents through their natural groups, they were able to make sure by a variety of means that all the respondents were actively dependent on opiates at the time of the interview. Besides observing them buy and use opiate drugs, the interviewers determined dependence for each addict (1) by asking the other addicts in the group about the individual, (2) by inspecting the addict's body for extensive old and new scarring (which indicated both long-term and current use), and (3) by asking the addicts themselves. Only persons who could be confirmed as addicts by all of these methods were interviewed. Approximately half of the respondents were paid from \$3 to \$5 for their cooperation.

Drug-use characteristics of the respondents are presented in table 1. The

TABLE 1
DRUG-USE CHARACTERISTICS OF SAMPLED ADDICTS

Characteristic	Mean	Range	SD
Age of first opiate use (years)	17.4	13-29	2.91
Age first physically dependent (years)	19.0	14-31.5	2.91
Length of onset (years)	1.6	0.0-7.0	1.41
Gross length of addiction (years)	5.8	0.3-26	4.66
Periods of incarceration (months)	13.1	0.0-120	21.87
Periods of voluntary abstinence (months)	16.4	0.0-93	20.83
Net length of addiction (years)	3.5	0.2-18	3.06
Length of current dependency (years)	1.1	0.1-5.5	1.1
Money spent daily on opiates (dollars)	39.8	6-180	29.1

respondents first tried an opiate at an average age of 17.4, and approximately 1.6 years later, at age 19, they realized they were physiologically dependent.⁷ This realization had occurred an average of 5.8 years prior to our interview. With corrections introduced for periods of abstinence and incarceration, the average net length of time the addicts were actually dependent on opiates is reduced to 3.5 years. All had relapsed at least once,

⁷ This period of onset was considerably longer than the several weeks specified by Lindesmith (Lindesmith and Strauss 1968, p. 195). However, Lindesmith (1968, p. 106) himself describes cases in which opiate use prior to dependence was prolonged. In addition, other studies have reported long periods of onset as typical (e.g., Willis 1969, p. 312; Chein et al. 1964, p. 128; Ball 1969, p. 121; Ellinwood, Smith, and Vaillant 1966). This duration is an extremely important fact which differentiates the drug-reinforcement histories of urban heroin addicts from persons who become physically dependent upon opiates through medical treatment, and it may well explain why physical dependency in this latter group does not ordinarily lead to chronic addiction. Moreover, the animals used in experimental addiction studies (e.g., Nichols 1965) do not usually have comparable drug-reinforcement histories. In contrast, the typical heroin addict has perhaps been rewarded by opiate euphoria hundreds of times before he actually becomes physically dependent, and it is no wonder that animals who become dependent after only a few weeks of constant exposure to opiates do not always behave like human heroin addicts.

and the most recent drug-free period ended for our addicts 1.1 years prior to the interview, on the average. The shortest run of continuous dependency prior to the interview was three weeks, and the next shortest was six weeks. These two cases have been retained in order to provide data points in the lowermost range of this potentially important variable. (In the light of our later analysis, it is noteworthy that neither of these addicts experienced euphoria at a high frequency.) Thus, even if we consider each cycle of dependency as a new addiction, which Lindesmith does not, all but one of our respondents would still be well beyond the several-week period after which Lindesmith's "reversal of effects" supposedly occurs. In terms of Lindesmith's own criteria (1968, pp. 64-67), all were, without any question, addicts.

The Definition and Measurement of Euphoria

Defining euphoria.—It is essential that our discussion of euphoria be as unambiguous as possible. Opiate euphoria is defined here as a subjectively pleasurable feeling produced by taking an opiate drug. We consider this euphoria to be the conscious concomitant of the positive reinforcement produced by the arousal-changing effect (Berlyne 1967) of the pharmacological action of the drug, and the presence of euphoria may be taken as an indicator that positive reinforcement has occurred.

Operationally, experiencing opiate euphoria is defined in this study as "feeling or getting high." Participant observation work among addicts in Baltimore and Saint Louis has convinced us that this definition accords with the meaning of "high" as the addicts in this study use it. Although euphoric reactions to opiates are by no means confined to persons involved in an addict subculture, since physician addicts also report euphoric effects (Winick 1961), we have operationally defined the concept in terms appropriate to the population we were studying. Other researchers (e.g., Chain et al. 1964, p. 229) have operationalized the concept in this way when studying subcultural addict populations, and Lingeman (1969), in his dictionary of drug terms, defines "high" as a state of drug euphoria (p. 109).

An excerpt from one of our tape-recorded interviews with an addict will indicate the meaning of being high for this population:

Respondent: I like being high.

Interviewer: What do you like about being high?

R: I like the rush when you fire and I like the nod.

I: Why do you like being high?

R: It's just like when people like to get drunk, you know, they do it because they feel good.

I: What do you mean, "They feel good"?

R: They just feel good, that's all. Just like most people like to ball [have intercourse with] a girl to get the climax because it feels good.

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I: So, when you're high, when you say you feel good, what's that mean to you?

R: I'm happy and content.

I: Anything else?

R: No, just satisfied.

Euphoria and the impact effect.—One aspect of this addict's comments that may require explanation is the distinction he draws between the two euphoric effects encompassed by the term "high." The first effect, which he calls the "rush," is the intensely pleasurable sensation first felt following an intravenous injection. This effect has been described as the "pharmacogenic orgasm" by Chessick (1960), and Lindesmith refers to it as the "impact effect." We shall use Lindesmith's term. The second euphoric effect is the more prolonged sensation which this addict terms "the nod" and which we shall refer to as the "continuing effect." Lindesmith considers only this second effect to be "euphoria," but we question this restriction.

Although the impact effect of a dose of opiates is most distinct from the continuing effect when the drug is injected intravenously, and it may vary widely in its intensity, there is every reason to believe that the initial arousal-change is present and positively reinforcing in one degree or another, regardless of the mode of administration and regardless of whether the user is physiologically dependent. The intensity of the effect, and consequently the magnitude of reinforcement produced, appears to depend mainly on the rapidity and amount of absorption into the bloodstream. O'Donnell and Jones (1968), for example, quote one addict as saying that the intravenous mode of administration did not become widespread until heroin began to be diluted—"you didn't need no vein until they cut it." More generally, however, they attribute the original diffusion of the "I.V. route" to the discovery of its increased pleurability when a vein was accidentally struck by hypodermic-using, long-term addicts. Kolb (1925) has remarked that, historically, the reason addicts turned to the intravenous method was that the impact effect obtained from other methods of administration became less distinct due to the development of tolerance. Within the addiction literature, the tendency of writers to refer to the continuing effect alone as "euphoria" is quite likely due to the fact that the impact effect is most salient under intravenous administration, and this method has become popular only since the 1930s (O'Donnell and Jones 1968).

In view of these considerations and the important testimony of addicts themselves (Chessick 1960), we feel justified in regarding the impact effect as one of the euphoric effects of opiates, as does Goldstein (1972). Therefore, unless otherwise specified, our use of the term "euphoria" in this paper will subsume both impact effects and continuing effects. Elsewhere (McAuliffe 1973), we present evidence from two factor analyses that bears out the pleasurable value of the impact effect as distinct from the relief of

withdrawal discomfort, with which Lindesmith tries to equate it. By disassociating impact effects from euphoria and from positive reinforcement, Lindesmith has freed himself from the necessity for dealing with the pleasurability of impact effects while discussing positive reinforcement, even though he himself elsewhere acknowledges that they are pleasurable (1968, pp. 33-34).⁸ Obviously, an adequate treatment of euphoria must take both of these effects into account. In this paper we show that the two euphoric effects can also be measured separately (e.g., table 6).

Measuring euphoria.—Self-reports of "being high" and "getting a rush" are usually accepted as indications of what we mean by experiencing euphoria. However, some writers might question the validity of such measures. Lindesmith (1968, pp. 34-39), for one, has insisted that addicts' reports of their sensations are not valid measures of drug effects. In support of his contention, he cites a few examples of addicts' having been deceived when undergoing the gradual-reduction method of withdrawing drugs and of cases in which addicts did not recognize the effects of an opiate when it was administered disguised as a different drug. Lindesmith also claims that little research on the placebo effect has been done with addicts or with opiates.

Lindesmith is quite wrong on this matter. Although it is possible under very special conditions to deceive addicts with respect to the effects of various drugs, the conditions in question seldom occur naturally. Addicts are, in fact, conspicuous as nonreactors to placebos (Haertzen 1966, p. 183; Lasagna, von Felsinger, and Beecher 1955, p. 1012; Fraser and Isbell 1952, p. 499; Martin and Fraser 1961, p. 390). Furthermore, many pharmacological researchers (e.g., Fraser et al. 1961, p. 385; Martin and Fraser 1961, pp. 390, 394; Lasagna et al., 1955) have been impressed by the ability of addicts to distinguish opiates from placebos and other drugs in single-dose and experimental addiction studies. For example, Lasagna et al. (1955) stated: "Many of these persons are pharmacological sophisticates, i.e., they can not only accurately distinguish between a potent drug and a placebo but can identify certain drugs with amazing accuracy, regardless of the route of administration" (p. 1019).

All of the preceding studies show that addicts can validly determine that they are experiencing subjective effects when given opiates. Evidence that these subjective effects are indeed euphoric ones is demonstrated by dose-effect relationships that show the reported strength of euphoria to be a

⁸ This becomes especially significant when we consider his interpretation of a series of experiments by Beach (1957a) and, indeed, when we consider Beach's interpretation of his own results. It is essential to examine Beach's experiments carefully because Lindesmith (1968) cites them and other experimental studies as evidence that "attachment to morphine . . . in rats . . . depended upon negative reinforcement involved in the relief of withdrawal distress and not upon the positive effects of the drug" (p. 126). For an examination of these experiments by Beach and others see Appendix.

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direct function of the amount of drug taken. For example, Haertzen's study (1966, table 7) shows an increase in euphoric responses from "no drug" and "placebo" conditions through two levels of morphine dosage (see the scale MBG), and Martin and Fraser's study (1961, fig. 1, and p. 396, which refers to fig. 1 mistakenly as "table 2") shows a similar increase across four dosage levels. In light of this substantial body of research (see also Beecher 1959), there is little reason to doubt that the subjective effects of opiates can be reliably and validly measured.

The Use of an Interview Methodology

The purpose of our study is not to establish that it is possible for physiologically dependent subjects to experience euphoria following an opiate injection. This has already been established under controlled laboratory conditions (Martin and Fraser 1961). Nor are we suggesting that tolerance to euphoric effects does not develop under certain conditions (Seevers and Deneau 1963, p. 576). Instead, our research is designed to determine whether chronic addicts are actually experiencing euphoria under the conditions of the current, natural addict milieu. Given what we know could be occurring, on the basis of experimental studies, it is important to investigate what actually does occur out in the addict world. If street addicts do not experience euphoria, then a demonstration in the laboratory that euphoria is an effective reinforcer would be irrelevant for understanding the present addiction problem.⁹ Addicts can report the experience of euphoria, and since other studies have shown that information obtained in interviews with addicts can be reliable and valid (Ball 1967; Robins and Murphy 1967; Stephens 1972), this is the method we have employed.

RESULTS

Our results are presented in four sections. The first establishes that chronic addicts do experience euphoria and, what is more, experience it frequently. Two types of addicts are distinguished on the basis of the frequency with which they experience euphoria, and these types are followed throughout the remainder of the paper. The next two sections trace the theoretical links from euphoric reactions to the behavior they seem to produce. The second section shows that a stated desire for euphoria is an important conscious motivation for addicts. The third shows that addicts not only desire

⁹ Wikler's (1965) research on relapse serves as an excellent case in point. Although Wikler has had some success in the laboratory demonstrating that relapse may be induced by conditioned withdrawal sickness, to our knowledge he has never tried to determine how often this effect occurs under natural conditions. We have interviewed 60 addicts concerning their many relapses, and we could find only one who had ever responded to conditioned withdrawal symptoms by relapsing.

euphoria, but in fact take positive action to attain it by choosing opiates with superior euphorogenic properties and by using a greater quantity of drugs. The fourth section accounts for some of the times in which—despite their desires—addicts fail to achieve euphoric reactions.

Evidence That Euphoria Is Experienced, and Its Frequency

To determine whether or not long-standing addicts experience euphoria, our respondents were asked, "These days, during the course of an average week (month), how often do you in fact get high?" The responses to this question are shown in table 2. Of the 64 respondents, 42% said they got

TABLE 2
FREQUENCY OF EXPERIENCING EUPHORIA

Frequency (Days per Month)	Percentage of Addict Respondents (N = 64)
28-30	42
24-27	2
20-23	0
16-19	5
12-15	17
8-11	20
4-7	9
0-3	5
Total	100

high *at least once daily*. (One, an admitted drug dealer, claimed he experienced euphoria every time he injected drugs.) The responses of the rest of the sample clustered around two to three times per week (8-15 times per month), but two of the respondents said they got high only twice a month. Only one said he never got high anymore. The mean frequency of experiencing euphoria for the entire sample was 18.6 times per month, and the standard deviation was 10.6. Thus, we learn that, with but one exception, all of the addict respondents experience euphoria, and, what is more, many of them experience it quite frequently.

In examining the distribution of these responses, we discovered that there appear to be two fairly distinct modal patterns of getting high. In one pattern the addict experiences euphoria every day; in the other euphoria is obtained a few days a week—perhaps on weekends only. Practically no addicts fell between these modes. It will be convenient to refer to these two groups as "hardcore" addicts and "weekenders," but we must emphasize that *all* of these respondents are physically dependent and *use opiates every*

day. In the sections that follow, we show that this typology, present in every one of the six groups sampled (McAuliffe 1973, p. 133), may constitute a significant discovery, with each type representing a basic pattern in the addict subculture around which other important aspects of the addicts' lives tend to be organized. It should perhaps be emphasized that it is not definitively established that the latter group reserves getting high exclusively for weekends, but some of our interview material suggests this tendency, and it will be seen that this characterization of the type that gets high just a few times per week has heuristic value for comprehending their overall pattern. The casual use of the term "hard-core" by other authors to refer to true addicts should not be confused with our use, and our "week-enders" should not be mistaken for nonaddicted users known as "chippers."

The one respondent who claimed he no longer experiences euphoria at all is, of course, of particular interest. He was white, 18 years old, and an unemployed carpenter's helper. He first realized he was physically dependent on opiates at age 15, and his last period of abstinence ended eight months prior to the interview. For the first four months of this latest period of addiction he was getting high every day, but then he reduced the amount of drugs he was using from \$30 to \$8 daily. He explained that the change was largely due to his decision to eventually stop using drugs. He was interviewed a few minutes before he contacted a social agency for help in stopping his addiction. Thus, in this one case the complete absence of euphoria was closely associated with seeking abstinence.

While existing theory might be able to stand in the face of scattered reports of euphoria from a small percentage of chronic addicts, the fact that 98% of a sample of 64 gave this response is totally at variance with what Lindesmith's theory seems to claim, and all the more so when the heterogeneity of our sample is fully appreciated.

In his more cautious passages Lindesmith asserts that the euphoria from opiates diminishes or vanishes once dependency occurs. It is quite possible, then, that Lindesmith did interview some addicts who said they experienced euphoria intermittently. However, because Lindesmith formulated his theory in the early 1930s prior to the discovery of the importance of intermittent rewards by Skinner (1938), and because of his "deterministic rather than statistical" methodological orientation (Lindesmith 1968, p. 13), he probably regarded these addicts' reports as inconsequential for a causal explanation of addiction. Today, we are in a position to recognize that these data represent meaningful patterns or schedules of positive reinforcement (Deese and Hulse 1967), and laboratory research with animals has demonstrated that intermittent drug reinforcements have effects similar to those produced using other kinds of reinforcers (Thompson and Pickens 1969, p. 187). Up to now, however, in the face of prevailing opinion regarding euphoria, none of the many published reinforcement-theory anal-

yses of opiate addiction (e.g., Akers et al. 1968; Harris and Balster 1970; Nichols 1965; Vaillant 1969; Wikler 1965) has ventured to posit intermittent, positive reinforcement as a factor in explaining chronic opiate use. Nevertheless, the evidence we have just presented suggests that opiate use by addicts is generally reinforced by a combination of negative and intermittent positive effects.

It is possible, of course, that our respondents overestimated the frequency with which they experienced euphoria. To alter any of the basic conclusions of this paper, however, the addicts' estimates would have to be exaggerated grossly, and there are several reasons to believe that this was not the case. First, the interviewers had known most of these respondents for years, and our interviewers were extremely knowledgeable about the "street scene." Second, the frequencies reported are by no means extraordinarily high. More than half of the sample admitted that they did not get high every day, even though they did use drugs at least once a day, thus indicating that the daily relief from withdrawal is not equated by them with getting high. Of those who did claim to get high daily, we asked whether or not they got high every time they injected. As we have noted, only one respondent claimed to get high after every single injection. The remaining members of the group that got high every day did so on the average of 52% of the times they injected themselves. By contrast, the addicts who got high less than once a day, the group we are calling "weekenders," experienced euphoria only 24% of the time. The third indication that the addicts' reports are not exaggerated is that the measure appears to have construct validity (Cronbach and Meehl 1955; Ebel 1961). That is, the reported frequency of euphoria is related to variables that one would theoretically expect to be related to this measure.

For example, the frequency of euphoria is positively related to the amount of drugs the addict consumes ($r = .30$, $P < .02$); the amount of drugs used in excess of the amount needed only to feel normal ($r = .30$, $P < .02$); a preference for euphorogenic drugs over drugs that simply relieve withdrawal symptoms ($r = .51$, $P < .01$); selling drugs or "dealing" ($r = .29$, $P < .05$); and negatively related to being employed ($r = -.22$, one-tailed $P < .05$). It also proves to be unrelated to the race of the respondent ($r = .05$, nonsignificant); the age at which he first began using opiates ($r = .01$); his height ($r = .05$); and his weight ($r = .16$, nonsignificant).¹⁰

That all but one of these respondents experienced euphoria establishes one of the necessary conditions for showing that euphoria is one of the

¹⁰ The last two correlations with height and weight were calculated for only the 17 respondents for whom these data were available. A correlation of .48 would be required to reach significance at the .05 level with these few cases. The other correlations were based on data from the entire sample.

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causes of drug taking and of related behavior by addicts. Nevertheless, it remains to be shown exactly how this finding could be of consequence to a theory of addiction. It is logically possible, for example, that euphoria is merely a pleasant psychological side effect that sometimes occurs as an unintended consequence when opiates are used for other purposes, such as relief from withdrawal sickness, depression, or anxiety, in which case the frequency of experiencing euphoria might not of itself produce any notable changes in drug taking. We have, of course, pointed out that experimental work with rats and monkeys (Beach 1957*a*, 1957*b*; Deneau 1969; Woods and Schuster 1968) demonstrates that the positively reinforcing pharmacological effects of opiates are alone sufficient to maintain self-administration of the drug. The purpose of our remaining analysis, then, will be to show that the desire for euphoria is independent of the desire for relief from withdrawal sickness, that it serves as a major conscious goal for addicts, and that, furthermore, addicts deliberately engage in a variety of activities in order to attain it.

Evidence That Euphoria Is Consciously Desired

To test whether or not euphoria is a consciously desired goal for these addicts, we asked them to choose from a set of alternative responses to the question, "If you could get what you wanted, how much time would you spend being high?" The response alternatives and the addicts' choices are presented in table 3. All of the respondents desired euphoria, and three-fifths

TABLE 3
IF YOU COULD GET WHAT YOU WANTED, HOW MUCH TIME WOULD
YOU SPEND BEING HIGH?

RESPONSE	PERCENTAGE		
	Hardcore (N = 26)	Weekenders (N = 37)	Total (N = 63)
All of the time	69	54	60
Most of every day	19	19	19
At least part of every day	12	16	14
One or two days a week	0	8	5
Once or twice a month	0	3	2
Never	0	0	0
Total	100	100	100

of them wanted to be euphoric "all of the time." Of the entire sample, 93% desired to be euphoric at least once each day. Although only the hardcore addicts are currently able to achieve this ideal goal, the weekenders seem to desire euphoria almost as much as the hardcore group. The difference

between the two groups on this measure was not significant; the correlation between the time the addict would spend being high and the frequency of experiencing euphoria was, however, weakly positive ($r = .18$, nonsignificant).

Even if an addict states, when asked, that he would like to be euphoric a large part of the time, it is quite possible that he seldom actually thinks of that as a goal. If this were the case, a desire for euphoria could conceivably play only a minor role in the addict's motivational system. Therefore, respondents were asked a separate question to gauge the extent to which the desire for euphoria actually occupied their thoughts. The question was, "How often do you think about wanting to get high?" The response alternatives available and the actual choices the addicts made are presented in table 4. As can be seen, all of the respondents thought about wanting to

TABLE 4
HOW OFTEN DO YOU THINK ABOUT WANTING TO GET HIGH?

RESPONSE	PERCENTAGE		
	Hardcore (<i>N</i> = 26)	Weekenders (<i>N</i> = 37)	Total (<i>N</i> = 63)
Whenever I am not high	81	60	68
At least once a day	19	24	22
About once or twice a week	0	16	10
Only once or twice a month	0	0	0
Never	0	0	0
Total	100	100	100

get high at least once a week, and 90% thought about it every day. The correlation between this measure and the frequency of experiencing euphoria was .36 ($P < .01$).

The data from these two questions suggest strongly, therefore, that the craving or hunger addicts have for drugs is to an important degree a desire for their pleasurable effects. This is borne out further by the relations between the variables described in tables 3 and 4 and the amount of money spent on drugs. The amount of time an addict wishes to spend being high and the frequency with which he thinks of euphoria are both positively correlated with the amount of money spent ($r = .22$, one-tailed $P < .05$; and $r = .26$, $P < .05$, respectively).

Evidence That Euphoria Is a Goal of Behavior Typically Associated with Addiction

Up to this point, we have shown that addicts actually experience euphoria regularly and that they desire it a great deal. We have also shown that

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these two variables are positively related to each other and to the amount of drugs consumed. A second paper (McAuliffe et al. 1973) presents evidence that addicts regard euphoria as one of the most important reasons for continuing to use opiates. In this paper, however, we want to show that euphoria is not only a key variable for explaining chronic opiate use per se, but it is also essential for explaining many behavior patterns typically associated with addiction. In this section, therefore, we present data showing that addicts prefer using certain drugs such as heroin and dilaudid because these drugs are particularly euphorogenic, and that addicts use more drugs than would be required simply to relieve withdrawal sickness in order to obtain euphoria.

Although neither of these hypotheses is startling or even especially novel, these phenomena were chosen for analysis because Lindesmith's theory does not appear to handle the first one easily and because he has offered a quite different explanation of the second phenomenon. We take this opportunity, therefore, to compare the two competing explanations.

Drug preferences.—The drug most frequently used by the contemporary urban opiate addict is heroin, despite the fact that methadone is more readily available, less expensive, and equally potent for suppressing withdrawal (Blachly 1965–66, table 2). If they use methadone at all, most addicts use it only when heroin is unavailable, and even those addicts who are maintained free of charge on methadone programs frequently cheat by using illicit and expensive heroin. Taylor, Bowling, and Mason (1971) found that 92% of the addicts on one such program cheated in this way during a one-month period. Since the duration of methadone is longer (Blachly 1965–66, table 1), and it is thus more effective than heroin in preventing withdrawal symptoms, this strong preference for heroin is paradoxical in terms of Lindesmith's theory.

Although it is probably true that heroin possesses a special subcultural value that might lead addicts to prefer it, there are a number of facts which suggest that its superior euphorogenic properties are responsible for its being the drug of choice for most addicts. First of all, Martin and Fraser (1961) have verified that heroin is actually more euphorogenic than even morphine. In their study, physically dependent addicts preferred heroin over morphine when the two drugs were administered intravenously under double-blind laboratory conditions. After a heroin injection, these addicts were more likely to describe their sensations in terms associated with euphoric effects ("buzz," "rush," "a pleasant feeling in the stomach," "high," and "nodding"). And, of course, part of the rationale for methadone is that it minimizes euphoric reactions, particularly the "rush" or impact effect when administered orally (Bazell 1973, p. 774). Further evidence stems from the fact that the diffusion of heroin occurred in subcultural groups that were *already established around the use of other opiates for*

pleasure (O'Donnell and Jones 1968). Against this background, it seems more plausible that heroin is preferred because it is so enjoyable, rather than that it is enjoyed because it is so preferred in the subculture.

In this section, we will show that the preference for heroin within sub-cultural groups is indeed associated with its superior euphorogenic qualities and that this preference is actually a function of the degree to which euphoria is desired. In other words, we will show not only that addicts prefer heroin, but also that variation within the addict group in the preferred drug is associated with variation in the desire for euphoria.

Fifty-seven of our addicts were asked to state their preference between dolophine (methadone) and heroin (in that order). Eighty-one percent chose heroin. At a much later point in the interview, the addicts were presented with another choice between drugs. Here, however, instead of using the names "dolophine" and "heroin," the interviewer described merely the reputed characteristics of the drugs:

Which would you prefer using:

1. A drug which would *not* make you high, but would keep your sickness away all day? or
2. A drug which *would* make you high, but would keep your sickness away only a few hours?

When the choice was posed abstractly in this way, the preferences shifted toward withdrawal reduction, so that only 43% preferred the euphorogenic alternative. While this shift indicates that the abstract characterization of the drugs' properties did not entirely capture the meaning of the addicts' preference for heroin over dolophine, responses to the two questions were significantly correlated ($\phi = .25$; one-tailed $P < .05$; the maximum value ϕ can attain here is only .57; ϕ 's between items even of standard batteries seldom range above .25-.35). Of the 24 respondents who chose the abstract euphorogenic drug, 92% also chose heroin. The percentage preferring heroin was substantially less, 72%, among the 32 who preferred an abstract drug featuring only protection against withdrawal.¹¹ Thus, the overwhelming popularity of heroin is associated with its ability to give pleasure.

This interpretation was tested further by examining the relationship between these drug-preference items and the two items dealing with the desire for euphoria. Phi correlations between the preference for heroin over dolophine and the amount of time the addict wanted to spend being high ($\phi =$

¹¹ Preference for heroin independent of its euphoric potential was found primarily among the weekenders. That is, of the 23 who switched from heroin to the noneuphoric drug, 17 were weekenders. These weekenders may prefer heroin because its symbolic value bolsters their standing as addicts in the face of their failure to realize more often one of the main addict values—to get high. Moreover, some of the change between the two questions may also have been due to the emphasis in the second version on the brevity of the interval during which withdrawal would be prevented ("a few hours").

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.25, one-tailed $P < .05$), and the amount of time he thought about being high ($\phi = .35$, $P < .02$), were both significant. The ϕ correlations between the more abstract euphorogenic choice and each of these desire items were .18 (not significant) and .26 (one-tailed $P < .05$), respectively. Thus, all of the correlations were positive, and three out of four were significant. The greater the addict's desire for euphoria, the more likely he was to prefer drugs that were euphorogenic.

A euphorogenic drug preference is associated not only with a general desire for euphoria, but also with the actual frequency of experiencing euphoria, as reflected in our hardcore and weekender typology. This is shown in table 5, where it is evident that the hardcore addicts, who

TABLE 5
PERCENTAGE OF ADDICTS PREFERRING EUPHOROGENIC DRUG OVER DRUG FEATURING
EXTENDED AVOIDANCE OF WITHDRAWAL SYMPTOMS

DRUG PREFERENCES	PERCENTAGE		
	Hardcore ($N = 23$)	Weekenders ($N = 33$)	Total ($N = 56$)
Preferred heroin and the euphorogenic drug ..	65	21	39
Mixed preferences	35	52	45
Preferred dolophine and the withdrawal-avoidance drug	00	27	16
Total	100	100	100

experience euphoria daily, reveal a considerably greater preference for euphorogenic drugs over drugs offering security from withdrawal. Looked at in greater detail, 21 out of 23 hardcore addicts preferred heroin over methadone, and 17 out of 23 preferred the abstract drug featuring euphoria. In contrast, weekenders were more likely to prefer methadone and the drug that would keep them normal all day long. There was, consequently, a strong association between preferring euphorogenic drugs and qualifying as a hardcore addict by experiencing euphoria daily ($\chi^2 = 9.23$, $df = 1$, $P < .01$, $Q = .75$). With the introduction of this last result, it can be seen, therefore, that there is marked consistency; the stronger an addict's desire for euphoria, the more likely he is to prefer euphorogenic drugs, and, in turn, the more frequently he experiences euphoria.

These findings are not consistent, however, with Lindesmith's assertion that the addicts' craving for opiates stems primarily from the withdrawal-suppressing qualities of these drugs. Addicts *need* a drug that prevents withdrawal sickness, but they *crave* a drug that makes them high. Obscuring the latter by emphasizing the former, as Lindesmith does, leaves unexplained results such as those presented here.

The deluxe ratio.—In addition to expressing attitudes that are consistent with behavioral preferences for euphoric drugs, there are many other overt actions that addicts may take to attain the euphoria they desire. Increasing the amount they use in a day is an important example. After asking our respondents how much they were spending on opiates each day, we asked, "How much dope, in dollars, do you need each day *just* to take your sickness away but not to make you high?" This question established a different baseline for each individual, and thereby acts as a rough control for different levels of tolerance to the withdrawal suppressant action of the drug. Every respondent, except the one who was not experiencing euphoria at all, responded with a figure that, when compared with actual daily costs, showed he used considerably more than he needed. Thus a desire simply to suppress withdrawal symptoms cannot in any sense explain completely the use of drugs by these addicts. The mean ratio of amount actually spent over amount needed, which we shall call the "deluxe ratio," was 2.4. That is, the addicts were using an average of two and one-half times as much as they thought they needed to prevent withdrawal sickness.

Increasing the size of an intravenous injection of heroin has two important consequences. One is to intensify the effects of the drug. For the present sample, this is borne out by the fact that the larger his deluxe ratio was, the more often an addict experienced euphoria ($r = .30$, $P < .05$). The other consequence is to prolong the euphoric and the withdrawal-suppressant actions of the drug. According to Lindesmith, the prolongation of the suppression of withdrawal is the addict's goal when increasing his dose. According to our viewpoint, euphoria is the more salient objective in motivating this behavior.

In order to test which of these interpretations corresponds more closely to the addicts' intentions, a set of carefully selected possible reasons for the deluxe ratio was read to them, and they were instructed to specify whether each reason was "not important," "slightly important," "medium important," or "very important." After completing the list, the addicts were then asked to indicate which reason was "most important." A mean importance score for each reason was calculated by assigning 0, 1, 2, and 3 to these four degrees of importance, from "not important" through "very important."¹² The "most important" responses were analyzed separately. The reasons, and their average importance scores, are presented in table 6.¹³

¹² Labovitz (1970) has shown that the correlations between two variables are relatively invariant over all but the most special monotonic transformations of their scales.

¹³ A brief comment on the validity of these responses may be in order. The respondent was asked to explain his behavior by evaluating the relative importance of a set of reasons. Although it is *possible* for data of this sort to be misleading, in the present case two types of evidence suggest that our addicts have insight into their own motives. First, the respondents themselves considered the questions carefully and felt that they understood the phenomenon being addressed. Only five addicts out of 63 indicated to

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TABLE 6

REASONS FOR DELUXE RATIO AND THEIR IMPORTANCE

REASON	IMPORTANCE SCORE MEAN		
	Hardcore (N = 27)	Weekenders (N = 36)	Total Sample (N = 63)
1. To get high	2.9	2.4	2.6
2. For the feeling when the drug first comes on, the "flash" or "call"	2.3	2.0	2.1
3. Just because you had it on hand	1.6	1.3	1.4
4. To be doubly sure you won't get sick	1.4	2.0	1.7
5. So that you do not have to fire as often	0.7	0.9	0.9
6. To feel more secure	1.6	1.8	1.7
7. For the hell of it	0.7	0.3	0.5
8. Because it is warm outside	0.04	0.06	0.05
9. For some other reason (specify)	0.6	0.7	0.7
10. Don't know	0.3	0.6	0.5

NOTE.—After ascertaining and calling attention to the fact that an addict used a deluxe ratio, the reasons were introduced as follows: "Here are some reasons why someone might use more drugs in a day than he needs just to take his sickness away. I want you to tell me how important each of these is to you, as a reason for using the amount of drugs that you do." One addict did not use a deluxe ratio, and the questions naturally were not posed to him. Within the table, most reasons are listed in order of their importance to the total sample. In the interviews, the reasons favorable to Lindesmith's hypothesis were presented first.

To interpret the results in table 6, it must be recognized that reasons 2, 4, 5, and 6 were included to reflect aspects of Lindesmith's explanation of the deluxe ratio (1968, p. 91). Lindesmith has proposed that the addict becomes hypersensitive to withdrawal symptoms and consequently shortens the time between doses. Unfortunately, this more frequent use reduces the impact effect (described in reason 2), which, according to Lindesmith's analysis, has become primarily a symbol to the addict of security from withdrawal sickness (reasons 4 and 6). Therefore, in order to feel his shot and thus be definitely assured that a potent, lasting dose of withdrawal-relieving drugs has entered his body, the addict must increase the size of his dose. Reason 5, "So that you do not have to fire [inject drugs] as often," was included to measure the addict's desire to lengthen the amount of time the injection will be effective in preventing withdrawal sickness. According to Lindesmith's analysis, then, the addict's motive for the deluxe dose is not to obtain euphoria but to feel security from withdrawal. High mean importance scores for reasons 2, 4, 5, and 6 would be consistent with his hypothesis.

In contrast to Lindesmith's explanation, we have hypothesized that the excess amount of drugs is taken whenever it is available (reason 3) in order

any degree that they didn't know (reason 10 in table 6) why they use a deluxe ratio. In addition, giving reason 7, "For the hell of it," would suggest a lack of definite purpose in their behavior, but the low importance scores for that item show that lack of definite purpose plays an inconsequential role. Clearly, the addicts themselves feel

to produce pleasurable effects. These effects include the "high" or "nod" (reason 1) and the impact effect (reason 2). Inclusion of the impact effect in our hypothesis, however, raises a difficulty in interpretation. Although at one point Lindesmith (1968) described the impact effect as intensely pleasurable (pp. 33, 40), elsewhere he interpreted the effect as a "symbol of security from withdrawal" (p. 91). Which of these it is will determine which explanation to credit according to the importance attached by addicts to reason 2. We have already argued that the impact effect should be regarded as a euphoric effect, and here we digress briefly to present further evidence in support of this interpretation.

Is a desire for the impact effect a desire for pleasure or for a sign of relief? If a desire for pleasure, the importance of the impact effect (reason 2) should be more closely associated with the importance of euphoria (reason 1); if a sign of relief, it should be more closely associated with the importance of security from withdrawal (reason 4). A factor analysis of these and other items has shown (McAuliffe 1973) that the items clearly group themselves into two distinct orthogonal factors—one of which reflects an orientation toward euphoria (reasons 1–3) and the other an orientation toward withdrawal sickness (reasons 4–6). In that analysis, the impact effect loaded positively on the euphoria factor and negatively on the withdrawal-sickness factor, thus clearly establishing its meaning. For now, it is sufficient to show that for the entire sample the importance of the impact effect is unrelated to the importance of security from withdrawal ($r = -.01$, nonsignificant) and is positively related to the importance of euphoria ($r = .37$, $P < .01$), and the second correlation is significantly larger than the first ($t = 2.04$, $df = 59$, one-tailed $P < .025$). Even if we consider only the weekenders, who experience euphoria less often, the observed relationship between the importance of the impact effect and concern over withdrawal remains negligible ($r = -.08$, nonsignificant), and the association

that they have definite goals for using the extra amounts of drugs. Moreover, reason 8, "Because it is warm outside," was included in the list to provide a neutral baseline for interpreting the magnitudes of the other responses and their validity. Only two respondents chose it, and neither regarded it as very important. Second, it can be shown that motives given by the addicts often result in empirically confirmed predictions that are not dependent upon interpretive introspection. For example, in the present case reason 3, "Just because you had it on hand," appears to measure the inability of the addicts to resist using all the drugs in their possession. (See McAuliffe [1973] for a more detailed discussion of the interpretation of this item.) This reason was reported to be of some importance by 29 addicts. But one would expect this reason to be most important for drug dealers, those addicts who are most likely to *have extra drugs on hand*. Twenty-three of the addicts were currently getting at least part of their money for drugs by selling opiates; as expected, they used larger amounts of drugs than the rest of the sample ($t = 2.06$, $df = 61$, $P < .05$) and regarded reason 3 as more important than the other addicts did ($\chi^2 = 2.91$, $df = 1$, one-tailed $P < .05$, $Q = 48$). In this kind of way, we shall try to show as we proceed that the addicts' responses have validity.

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with the importance of euphoria remains positive ($r = .43, P < .01$). The second correlation is again significantly larger than the first ($t = 2.01, df = 32, \text{one-tailed } P < .05$). It appears quite justified, therefore, to regard the impact effect as a desire for pleasure, and, in light of the factor analysis, to regard pleasure as quite separate from the mere relief of withdrawal distress.

We may now return to table 6 to consider the evidence there for the two rival explanations of the deluxe dose. Reasons 1 and 2, both euphoria reasons, received the highest mean importance scores (2.6 and 2.1) for the entire sample. The items receiving the next highest importance scores were reasons 4 and 6 (1.7 for both). These two items measure a concern with security from withdrawal sickness. *Thus, a combination-of-effects explanation appears to summarize the data better than one based on withdrawal alone.*

The relative importance of the reasons involved here, however, varies according to the frequency with which the addict experiences euphoria. The hardcore addicts, who experience euphoria daily, give greater emphasis to reason 1, "To get high," than do the addicts who experience euphoria less often ($\chi^2 = 5.89, df = 2, \text{one-tailed } P < .05$). Conversely, the weekenders or addicts who get high less frequently tend to put greater emphasis upon reason 4, "To be doubly sure you won't get sick," than does the euphoria-daily group ($\chi^2 = 5.75, df = 2, \text{one-tailed } P < .05$). This same pattern (but to a reduced degree) is also reflected in the mean importance scores of reasons 2 and 3 and of reasons 5 and 6. Consequently, it appears that the two kinds of effect combine in different proportions, depending upon which category of our addict typology is under consideration. However, euphoria reasons tend to rank first as explanations of the deluxe ratio for *both* types of addicts.

Examination of the addicts' choices of the "most important" reason reveals similar results. Reason 1 was chosen by the largest number of addicts (21), and reason 4 was second (18 addicts). The hardcore addicts, as before, gave greater emphasis to the euphoria items, while the weekenders stressed both kinds of items equally; again, euphoria held top rank for both groups.

Reasons for Variation in the Frequency of Euphoria within the Addict Subculture

Thus far, we have shown that 98% of our addicts experienced euphoria; 93% would spend at least a part of each day being high (table 3); 90% think about getting high at least once a day (table 4); 81% preferred euphorogenic heroin over withdrawal-satisfying methadone; and all but one currently used more drugs than needed to prevent withdrawal sickness. Clearly, euphoria is of major importance to chronic addicts.

However, there was a well-defined bimodal distribution in the frequency with which euphoria was experienced (table 2) that led us to divide our sample into two types, and all of the succeeding analyses showed that these types were somewhat different from each other on key variables (tables 3–6). These consistent differences suggest that our typology, which was initially based only on the frequency of experiencing euphoria, reflects a more general difference in orientation toward drug use. The following two excerpts from longer, tape-recorded interviews with Baltimore addicts will serve to bring out the nature of this difference in orientation between what we have called the hardcore and weekender types.

The first is from an interview with a 23-year-old hardcore addict, who first realized he was physically dependent at 18. He is using between \$30 and \$35 worth of dilaudid daily, and he gets high at least once each day:

- I: When you fire [inject opiates], are you usually just trying to feel normal, or are you firing to get high in a way that makes you feel better than normal?
- R: I think I am firing to get high and feel *better* than normal.
- I: How many caps of good smack [heroin] do you need a day to just keep your sickness away? That is, just enough for you to feel normal all day?
- R: Right now I need four all day to keep me going. I could space them but I don't like it. I did two and one-half this morning, and that will hold me until late tonight.
- I: Then you'll do two more?
- R: Yeah, I'll do two, maybe more.
- I: Suppose you had twice that many, and it was the same good stuff, how many would you cook up and fire at one time, assuming that you have money to cop [buy drugs] for the next day? Suppose you had eight?
- R: I'd do all of them. Not for one shot though. I'd throw in [to the cooker] about three.
- I: Why would you fire that many?
- R: To get high.

The second excerpt is from a 23-year-old "weekender" who uses only about \$15 worth of opiates per day and actually gets high only about twice a week.

- I: When you fire, are you usually just trying to feel normal, or are you firing to get high in a way that makes you feel better than normal?
- R: Well now, it's just to make myself feel normal, because I really don't get high much any more.
- I: Why is that?
- R: Well, I don't know if it's because the dope isn't good or because it takes more than I can get, but I am satisfied with just feeling normal.
- I: What about last night?
- R: Last night was something else. Then there was enough around to really get nice.
- I: What do you mean?

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R: Like, I didn't have to go out and hustle for money. It was right there, you know. It was given to me, and the person who was giving it had a large enough supply of it. But it is not like that all the time. It's some rare thing that happens.

I: How many caps of good smack do you need a day just to keep your sickness away? That is, just enough for you to feel normal all day?

R: About two bags.

I: How would you fire those two bags?

R: Oh, I'd fire one at 12 or one o'clock. Then, I'd fire the other one at about eight or nine o'clock. I'd be all right until the next morning.

I: Suppose you had twice that many, and it was the same good stuff, how many would you cook up and fire at one time, assuming that you have enough money to cop for the next day?

R: Probably what I would do would be to fire all four of them and then cop again that day.

I: Fire all four of them at once?

R: Yeah.

I: Put them all in?

R: Yeah.

I: You'd do them all and then go out and cop more?

R: Yeah.

I: Why would you fire that many?

R: So I could get high and get nice.

I: In the past year, how often have you done this?

R: A whole lot of times . . . about 90 times.

I: How much [would you shoot], for example?

R: Oh, maybe six or seven bags a day. Something like that.

The expectations of these two addicts are quite different. Although the first recognizes that he could space his shots one cap at a time and easily maintain his "habit," he has no intention of doing so. He desires a euphoric feeling, and therefore, he shot two and one-half caps that morning and planned to do at least two more the same night. The second addict expects to get high upon occasion, whenever a good supply of drugs becomes available. Otherwise, he is completely satisfied to feel only normal.¹⁴

What accounts for this variation between addicts in the frequency of euphoria? The usual explanation for the disappearance of euphoria (which is implicit in Lindesmith's analysis) invokes the concept of tolerance. As daily use continues, physiological tolerance to the effects of opiates builds progressively, thus constantly compelling the addict to increase the amount

¹⁴ It is worth noting, in his case, that if the questioning had been more casual, and had stopped, say, immediately after his reply to the interviewer's first or second questions, this addict would have left the impression that he never gets high, and fires only to feel normal. In contrast to his earlier behavior and experience, this probably seems to him to be a reasonable characterization of his present situation. However, upon closer examination, it turns out that he is still quite capable of feeling euphoria, and in fact did so the previous night, and that this capability extends back over the entire past year.

of drugs he consumes in order to obtain the same euphoric effect. Eventually, the addict reaches the limit of his financial resources and becomes unable to get high at all on amounts of drugs that are still within his means.

Our data suggest, however, that length of continuous use does not adequately explain the difference in frequency of euphoria between the hardcore and weekender types.

As expected, tolerance apparently does develop in these addicts, since the longer an addict has used opiates continuously, the larger was the amount of drugs he felt he needed just to feel normal ($r = .35$, $P < .01$). However, the addicts also appear able to counter this successfully by increasing proportionately the amount of drugs they consume as continuous use progresses ($r = .42$, $P < .001$). By adjusting their consumption, they maintain their own individual frequency of getting high. This can be seen in table 7, where

TABLE 7
FREQUENCY OF EUPHORIA AS FUNCTION OF LENGTH OF CURRENT RUN

Length of Current Run (Months)	<i>N</i>	Mean Frequency of Euphoria (Times per Month)	<i>SD</i>
1-4	15	20.0	8.38
5-8	15	18.1	10.59
9-12	18	18.2	11.40
13-66	16	19.4	13.85

there is practically no change at all in the mean frequency of experiencing euphoria as a function of the length of continuous use ($r = .07$). Furthermore, the sharp jump observed in table 2 between the weekender and hardcore addicts in their frequency of euphoria is not what one would expect if tolerance accounted for this difference. One would expect, instead, a gradual transition. The average length of the current run was also greater for the hardcore addicts than for the weekenders—15 versus 11 months. If anything, the hardcore addicts should exhibit the greater tolerance.

What is more, it is important to realize that the development and disappearance of tolerance depend on a variety of factors (Seever and Deneau 1963, pp. 577-79) and that addicts can control their level of tolerance rather easily in order to bring it in line with their current economic resources (Scher 1966). The following excerpt illustrates just how quickly an addict can reduce the quantity of drugs he feels he needs, when faced with financial restrictions:

I: On the average, what would you say your habit costs per day?

R: I'd say about \$25 a day.

I: What about when you were dealing?

R: I figure about \$75 a day.

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I: How long ago was that?

R: About a week and a half ago.

I: How did you get down to \$25?

R: By not having no money and doing one bag here and one bag there. That cuts you down. You don't feel high or nothing. You're just barely on it, just barely feeling good. You just can't make it, you know, to hustle the rest of the money.

Another plausible explanation for the differences in the frequency of experiencing euphoria is maturation (Winick 1962), which holds that addiction tends to remit as the addict grows older. Thus, the lower frequency of getting high by our weekenders may simply represent an early stage in the remission process. However, the maturation hypothesis does not seem to apply here. There is no difference at all in the average age of the weekenders and the hardcore addicts. In fact, the hardcore addicts have actually been dependent on opiates (net length of addiction) slightly longer than the weekenders (3.8 years to 3.2 years), and there is no difference in their distributions according to gross length of addiction ($\chi^2 = 1.2$, $df = 3$, nonsignificant).¹⁵ Both the lack of difference in age and the direction of the difference in net length of addiction are inconsistent with maturation as the explanation.

What evidence we have suggests that the frequency of experiencing euphoria is not explained by differences in the addicts' histories of drug use. Rather, the frequency of euphoria appears to be indicative of an overall difference in commitment to a drug-oriented life-style. The hardcore addicts became more deeply involved in drug use from the very start. While both the hardcore and the weekenders began using opiates at the same age, the weekenders took 50% longer to become physically dependent (1.8 years as compared with 1.2 years for the hardcore). In addition, the weekenders have been abstinent a greater percentage of the time since originally becoming dependent (33% versus 26%). These periods of abstinence, furthermore, tend to have been voluntarily sought more often by weekenders, whereas the hardcore addicts have spent a disproportionate amount of these drug-free periods under incarceration.

The difference in life-style is revealed even more clearly by examining the ways the addicts get money for drugs. Half of the weekenders were gainfully employed, but only one-fifth of the hardcore. An addict who wants to remain employed ordinarily must be able to moderate his hedonistic urges, as the following excerpt illustrates: "The job that I have now, I'd like to keep it. It's in a convalescent home, and I wouldn't want to cause any harm on the patients. I have to be lifting them and taking care of

¹⁵ The cutting points used for gross length of addiction were the same as Brotman and Freedman's (1968, p. 127). The curvilinear relationship between adaptation type and gross length of addiction which they reported is not corroborated by our data, although our types clearly resemble at least two of theirs.

them. So I wouldn't want to be nodding and lifting up a man or lady, you know." Instead of working, the hardcore addicts were much more likely to depend on selling drugs (52% compared with 22% of the weekenders) and other illegal activities. Almost all the hardcore addicts (96%) were regularly involved in some illegal activities, whereas one-fourth of the weekenders claimed no illegal activities whatsoever.

These differences are especially meaningful on two counts. First, they indicate that addict criminality does not result primarily from a desperate need to relieve withdrawal sickness, as the popular conception would have it, but rather from the desire for euphoria. This conclusion is strengthened by the fact that withdrawal distress is rarely experienced by addicts; in our sample, actual daily consumption of opiates exceeded the minimal amount needed by a factor of 2.4. Second, the differences indicate a potentially important link between our typology and rehabilitation outcomes, since the continued use of euphoric drugs is a chronic problem for methadone programs, and since the extent to which an addict was legally employed and not involved in criminal activities has been one of the best predictors of successful rehabilitation (Babst, Chambers, and Warner 1971; Ball and Snarr 1969; Blum and Associates 1972, pp. 220-21; Brotman and Freedman 1968, p. 132; Duvall, Locke, and Brill 1963; DeFleur, Ball, and Snarr 1969; Inciardi and Babst 1971; Stephens and Cottrell 1972; Waldorf 1970; Vaillant 1966).¹⁶

It seems apparent, then, that the hardcore and the weekender types represent markedly different points on a continuum of commitment to a conventional life-style. The weekenders are addicts who have retained some of their most important ties with conventional society, and, in doing so, let loose only periodically—much like other workingmen. The hardcore addicts, in contrast, have gone over more fully to a deviant orientation which values pleasure from drugs above all else.¹⁷

¹⁶ It might be pointed out that, contrary to popular belief, a substantial proportion of addicts hold jobs while addicted. In a review of 16 studies, we found that 39% of a total of 5,194 addicts worked, according to various criteria. The median percentage was 29.4. In our two surveys, 37% and 33% of the addicts were working when interviewed. The studies reviewed were Alksne et al. (1959, table 46); Blum and Associates (1972, p. 220); Brill and Lieberman (1969, p. 297); Brotman and Freedman (1968, p. 121); Chambers, Cuskey, and Moffett (1970, pp. 203, 208); Chambers, Cuskey, and Wieland (1970, p. 45); Chambers, Hinesley, and Moldestad (1970, pp. 260, 263); Chambers and Moffett (1970, p. 192), males only; Ellinwood et al. (1966, p. 42); Flohr and Lerner (1971, p. 152); Glaser, Lander, and Abbott (1971, p. 514); Nurco and Balter (1969, pp. 34, 78); O'Donnell (1969, p. 127); Stimson and Ogborne (1970, p. 16); Vaillant (1966, p. 1286); Wurmser (1970, table 1).

¹⁷ Brotman and Freedman (1968) have reported similar observations, but they argue that conventionality and criminality form two dimensions rather than opposite ends of one. Consequently, they distinguish four addict types where we distinguish two. Elsewhere (McAuliffe 1973, pp. 170-77), we have explored a similar breakdown, but in view of the strong association ($Q = .67$) between noncriminality and conventionality

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This interpretation is further substantiated when the addicts themselves are asked to explain why they sometimes fail to get high. The relevant instrument item and response alternatives are presented in table 8. The

TABLE 8
REASONS FOR NOT EXPERIENCING EUPHORIA, BY FREQUENCY OF
EXPERIENCING EUPHORIA

"IF YOU DO NOT GET HIGH, WHEN YOU FIRE, IT IS SOMETIMES BECAUSE":	MEAN RESPONSE (%)			
	Hardcore (N = 25)	Weekenders (N = 37)	Total Sample (N = 62)	SD (N = 62)
Nonvolitional reasons:				
1. You cannot get enough money together	29	30	29	25.4
2. You got burned (cheated)	12	9	10	12.3
3. You cannot find the dealer you normally go to	11	9	10	10.9
4. You cannot get a hit (get the needle in a vein)	13	4	7	12.8
5. Of where you are	5	4	4	9.5
6. Of whom you are with	3	2	3	6.0
Subtotal	73	58	63	...
Volitional reasons:				
7. You only want to feel normal, and you do not want to get high	7	19	14	24.3
8. You want to keep the size of your habit down*	4	11	8	11.0
9. You have something important to do ...	7	5	6	12.5
10. Of how you feel	6	6	6	7.7
11. You are worried about something	5	3	4	8.6
Subtotal	29	44	38	...

* The N's for this item were 22 for the hardcore addicts and 34 for the weekenders.

addicts were requested to respond in percentages, "For example, if about half of the times you actually did not get high in the past few months it was because you did not want to, then when I read that reason, you say '50%.'"¹⁸

For analysis, the reasons have been classified into two broad categories, "nonvolitional" and "volitional." The nonvolitional category includes such reasons as the lack of financial resources which prevent an addict from

(i.e., employment status) in our sample, we prefer the one-dimensional interpretation in this paper.

¹⁸ The list of alternative reasons was not exhaustive, and so it would be reasonable for the percentages given by an addict to add to less than 100. However, many of the addicts gave responses which totaled more than 100%. Consequently, all of the responses were adjusted so that they did total 100% for every respondent. The numbers should be regarded, therefore, simply as measures of relative importance.

achieving euphoria despite his desire to try for it. While both groups of addicts reported the lack of money as most important (thus signifying the value of euphoria to addicts generally), the hardcore addicts tended to place greater emphasis on the other nonvolitional reasons than did the weekenders. This was especially true for reason 4 ("You cannot get a hit").¹⁹

The volitional reasons in table 8 include a variety of items which carry the implication that the addict himself has decided not to try to get high. The weekenders stress these volitional reasons significantly more than the hardcore addicts (whereas the hardcore addicts stress the nonvolitional reasons), as can be seen by the sums of the average percentages of the reasons in each classification ($t = 2.20$, $df = 60$, $P < .025$). This difference is due entirely to responses to reasons 7 and 8, which indicate a flat rejection of euphoria as a goal, and a concern for restricting the level of tolerance. Thus, the weekenders often do not wish to be high, and they also tend to be careful to keep their level of consumption under control (by not getting high some of the time). In contrast, the hardcore addicts are less inclined to allow considerations under their own control to take precedence over their desire for euphoria.

The strong emphasis on the nonvolitional reason 1 by both types of addict indicates that money is the primary constraint on getting high more often. This was also borne out in our second survey of 60 addicts from these same neighborhoods. The subset of addicts in that survey who said that they were usually *not* trying to get high but were just trying to stay normal in response to a particular question (and who are thus roughly equivalent to the weekenders here), explicitly mentioned money more often than any other consideration in accounting for their failing to get high

¹⁹ The following excerpt illustrates how this item typifies the hardcore perspective:

I: Do you skin-pop [subcutaneous injection] or mainline [intravenous injection]?

R: Well, I can mainline, but I usually skin-pop. Uh, too much trouble . . . all the veins are collapsed.

I: You used to mainline though?

R: Well, I still do once in a while, but lately . . .

I: You've been switching over to skin-popping. Is that what you're . . .

R: Right. When I am sick. Yeah, I gotta get on. I might have, like see, if I had eight bags, I might hurry up and shoot four—skin-pop four, that is. And then turn around and take my time, trying to get on [i.e., get high]. You know, find the place. It might take a couple of hours or something.

I: Which would you rather do?

R: I'd rather mainline.

This addict valued the sensation that he received from an intravenous injection so much that he would take a maintenance dose to keep from becoming sick during the time he needed to probe under his skin searching for a vein into which he could inject the remainder of his drugs. With the additional drugs he would feel the impact effect and then would be high for a number of hours. The behavior exhibited here, by the way, is quite inconsistent with Lindesmith's equating of the impact effect to the mere relief of withdrawal.

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more often. Moreover, money was often implied in the reasons given by other such respondents.

If we examine this situation more closely, it appears to hold the key to the main difference between our two types of addict. The ultimate in commitment to the pleasure of opiates is probably realized in getting high every day. Although one could conceivably exceed this by not only getting high every day, but by also getting high on every single injection, in practice this would leave little time for attending to other daily business unimpeded by "nodding." (On the effects of opiates on mental and physical efficiency, see Smith, Semke, and Beecher [1962]; Fraser et al. [1963].) In particular, it would leave little time for the business of financing one's pleasure. Only one of our addicts claimed to get high this often, and he was a drug dealer. Therefore, enjoying euphoria one or more times each day, but not necessarily on every injection, represents the practicable upper bound for extracting pleasure from opiates. A sizable proportion of our addicts operate at this upper bound.

Now, what about the lower bound? Clearly, there is no physically determined lower bound; a person could sustain a physical dependence indefinitely without ever enjoying much pleasure from it. But this would be expensive, and since almost no addicts do it, it is obviously not a profitable position. By the same reasoning, if euphoria were experienced only rarely, the addict would have very little return in the way of pleasure for the exorbitant "overhead" of his habit. The question naturally arises: is there some stopping point, short of total involvement in daily euphoria, that yields a meaningful frequency of euphoria in return for the overhead of carrying an opiate habit?

On the time scale of human affairs, pleasure on at least a weekly basis appears to be a schedule of reinforcement meaningful enough to sustain daily striving. Our days of rest and recreation are spaced so; many wage earners are paid weekly; and research (Kinsey, Pomeroy, and Martin 1949, p. 256; Rainwater 1965, p. 101) indicates that the average frequency of sexual intercourse among married couples is about two to three times per week—just about the same as the modal frequency of euphoria among our weekend addict. To enjoy euphoria less often than a couple of times a week strikes us as rather meager return on one's daily maintenance investment, and it probably strikes addicts the same way. Addicts who are getting high much less often than once a week probably want to terminate their current run altogether, although they may be reluctant to undergo the unpleasantness of the withdrawal period. Only 5% of our addicts (table 2) got high less than four days per month, and we know that at least one of these was definitely intending to quit.

If this argument concerning a theoretical lower bound is correct, it still

remains to show that the economics of getting high every couple of days is sufficiently different from getting high every single day to represent a fairly stable reinforcement position in its own right. Let us examine the average weekly costs faced by our two types of addict, excluding those who are known to rely heavily on methadone, which has a different price range than other opiates. For 30 weekenders, the average weekly cost is \$249, and for 26 hardcore addicts it is \$357. This indicates that it costs \$108 more per week—or 44% more—to move from being a weekender to the position maximally rewarding to an addict. Since more weekenders rely heavily on the considerably less expensive methadone, its use could well be regarded as an intrinsic aspect of the weekender pattern; counting all addicts, the weekly costs are \$227 and \$349 for the two types, respectively—a 54% difference.

These differences of either \$108 or \$122 per week involve substantial amounts of money for working-class persons. They are substantial even for the addict who raises money by illegal means because they translate into having to work much harder and into taking greater risks, either as the result of more continuous exposure or as the result of shifting to a riskier but more lucrative hustle.

Since 50% of our weekenders are legally employed, it is clear that many weekenders often pay for part of their habits with legally obtained funds, making up the difference between earnings and the high cost of their habits by low-risk hustles (e.g., copping for others or begging shots) and brief forays into medium-risk endeavors (e.g., some stealing). In this position, the weekender's legal resources, including support from family members, are probably fairly strained, if not fully so. However, many weekenders are still able to enjoy the rewards of a conventional life-style. They can maintain a job, share the responsibilities of marriage, raise a family, accumulate possessions, and even have a fairly normal sex life. Thus, they can still feel entitled to conventional self-respect, which many addicts in our samples claimed to value.

These inferences are substantiated in table 9, which shows the relationship between being a hardcore or weekender addict in our sample and obtaining income from progressively riskier and more deviant categories of sources. Ten addicts obtained money solely from legal sources (category 1 in table 9). All were working full time, and some supplemented their incomes with money obtained from family or friends. These 10 spent the least amount of money for drugs, and they experienced euphoria less often, on the average, than addicts who obtained their income from sources that are illegal (categories 2–4). Only one addict in category 1 got high every day. Supplementing one's legitimate income by hustling (category 2) leads to an increased frequency of euphoria, but still only two of the eight addicts who both work and hustle were able to achieve the hardcore frequency

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TABLE 9

SOURCE OF INCOME, FREQUENCY OF EUPHORIA, AND SIZE OF HABIT

Source of Income	Percentage Who Are Hardcore	Average Frequency of Euphoria (per Month)	Average Cost of Habit (\$ per Day)	N*
1. Legal only	10.0%	13.6	20.8	10
2. Employed plus hustling (except dealing)	25.0	16.1	37.6	8
3. Hustling only (except dealing)	40.9	17.3	35.0	22
4. Dealing	60.9	22.5	54.0	23
Outcome of χ^2 test	$\chi^2 = 8.39$ $df = 3$ $P < .05$
Outcome of one-way analysis of variance	$F = 2.16$ $df = 3, 59$ N.S.	$F = 3.84$ $df = 3, 59$ $P < .025$...

* Source of income was not determined in one case.

of euphoria. However, movement into full-time hustling (category 3) results in a substantial increase in the number of hardcore addicts. The weekenders in category 3 actually got high less frequently (8.5 times per month) than the employed weekenders in categories 1 and 2 (11.7 times per month), which suggests that, rather than being weekenders by choice, they may represent examples of the pathetic individuals known scornfully among addicts as "lames" or "hope-friends," who have overreached themselves and who can barely finance the overhead of their habits. Moreover, since category 4 contains the greatest concentration of hardcore addicts, the data are consistent with our argument that most addicts must turn to risky but lucrative activities such as selling drugs on at least a part-time basis in order to attain the maximum frequency of euphoria.²⁰

However, in order to obtain the significant increment in income required to sustain the hardcore addict's frequency of euphoria, a working weekender would have to commit himself much more fully to illegal means. This would probably necessitate giving up his job and compromising all of his other investments in conventional sources of satisfaction, including his family relationships. Although he may need to raise only 54% more money, his risk increases more than that, for illegal activities probably provided only a part of his income before, whereas now they may provide all of it, and be qualitatively riskier as well (e.g., dealing in drugs). Furthermore, as a

²⁰ The proportion of addicts involved in dealing in our sample is not unusual. Blum and Associates (1972, pp. 216-17) found a similar proportion. They also give habit costs for working, hustling, and dealing addicts that stand in relation to each other much as the values for corresponding types in our sample.

weekender, the addict could "nurse" his habit so that the development of tolerance does not proceed too rapidly. As a hardcore addict, taking large doses of opiates every day, he is embarking on a course guaranteed to accelerate the trend of rising costs over the long run. These considerations lend stability to the weekender position on the high side and account for the quantum jump between the frequency of euphoria modes.

The following excerpt illustrates the stability of the weekender position over long periods of time:

- I: When you fire, are you usually just trying to feel normal, or are you firing to get high in a way that makes you feel better than normal?
- R: I would say for the past 10 months—that 90% of the time has just been to feel normal. Now, there may be times that I say, "Yeah, I'm going to get loaded today," you know? I feel like I want to get loaded today. And if I got the bread [money], like, I just go on with it, you know?
- I: What about the times before the last 10 months?
- R: Even with them, even after I was hooked [three years prior], 50% of the time was to feel normal. But the other 50%, I wanted to get *high*, you know?

Immediately following this, the addict explained that he injects drugs three times each day—once in the morning, once at midday, and once before bedtime. We can infer, therefore, that he experiences euphoria between two and three times a week (10% of 21 shots), which makes him a weekender who was stable over this entire 10-month period. There was no indication that he intended increasing or decreasing this frequency of experiencing euphoria.

This passage also brings out another extremely important point. Even a hardcore addict experiencing euphoria daily probably injects himself at least twice a day (in one of our surveys, 77% fired more than once a day). One of these injections is almost certainly merely a maintenance dose. In the above example, our addict was probably getting high daily during the earlier period in which he reports that he got high on 50% of his injections. This corresponds almost exactly to the 52% rate of getting high per injection reported by our hardcore addicts, excluding the one dealer who said he got high on every single injection. Thus, even the hardcore addicts are very much aware that a large fraction of their injections goes for "overhead," rather than for euphoria. In their minds, this evidently contrasts sharply with their recollection of the "honeymoon" period, during which there was no overhead because physical dependence had not yet developed. During that period, all of their shots were volitional, and therefore taken with the intention of getting high, and no shots were taken just to feel normal. Since addicts essentially pay by the shot, it is reasonable to assume that they count by the shot rather than by the day unless specifically asked to do otherwise. Hence, even the hardcore addict who gets high every single day

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is close to being able to report that he is "firing mainly to keep normal," injectionwise. This contrasts with the original state of affairs when he never had to shoot to feel normal. In the case of a stable weekender, as above, who gets high on the average of about three times per week, most of his injections go for overhead. Hence, he is even more apt to claim that he is "firing mainly to keep normal." In the above example, this would have meant 90% of his shots during his current weekender period. Thus, the claim heard frequently from addicts that they are mainly trying to stay normal is actually a complaint about their overhead rather than a full description of the amount of positive reinforcement they receive, for on closer inquiry it often turns out that their frequency of euphoria, on a monthly basis, remains exactly what it was during their predependency stage—or is even higher.

In view of our analysis, the two modes appear to represent the only two relatively stable positions in the range. Very few addicts fell between the two (see table 2), and this distribution does not appear to be due to any artifacts stemming from stereotyped ways of reporting frequencies. The large difference in cost between the two modes demands that one make a profound adjustment in the manner of obtaining income and succeed at it in order to move to the hardcore position. In addition, the hardcore addict role implies a much greater overall allotment of time to drug-centered activity. Therefore, if one is already a stable weekender, the commitment to experience euphoria at the upper-bound frequency represents a change in life-style that is more fundamental than that which sociologists normally associate with a radical change in occupation (say, drastic downward mobility). One's willingness and ability, or lack of either, to make this change must determine whether a successful neophyte becomes a weekender or a hardcore addict—that is, at which of the two stable positions he comes to rest.

Since the weekender status is to some extent a marginal position that often reflects an attachment to elements of a conventional life-style and an exercise of self-control, it is probable that the weekenders represent the more promising candidates for rehabilitation, as the findings linking employment and remission suggested. On the other hand, the unrestrained hedonism of the addicts who obtain euphoria every day and their willingness to sacrifice conventional commitments in order to devote themselves more completely to the pursuit of pleasure, by full-time criminality if need be, suggest that they are the poorer candidates for rehabilitation.

By attaining the ideal of the addict who is able to maximize his enjoyment from drugs, and thus realizing the aspiration common to most addicts, these individuals personify the most important norm of a pure addict subculture. Their willingness to fully embrace criminal activity to achieve this ideal accounts for the strong criminalistic component in addict norms,

and the success and resourcefulness of these addicts in the face of the attendant dangers no doubt contribute to their prestige among addicts. Such heroic exertions, however, would be relatively meaningless, if not absurd, if they were directed solely toward reducing withdrawal symptoms, which could be accomplished with much less effort, as every addict knows. The high prestige reported in the literature for types of addict that appear to resemble what we are calling hardcore addicts (Blum and Associates 1972, p. 217; Fiddle 1967, pp. 55-58; Sutter 1966, 1969, p. 822) makes sense only when the importance of euphoria is recognized.²¹ *For it is the frequency of euphoria, more than anything else, that stratifies the addict social system.* The degree of success in staving off withdrawal symptoms differentiates individuals mainly at the bottom of the prestige range. As in conventional society, those who cannot attend to their own basic needs are lowest in status. Beyond these minimum requirements, however, individuals are ranked by their success in achieving euphoria, with persons who do not have to exert themselves at all being envied perhaps, but denied top status (the "idle rich"). Thus, the distribution of prestige among addicts parallels what occurs in small groups generally, with greatest prestige reserved for those who most fully and consistently embody the highest values (Homans 1950). In this sense, hardcore addicts are the true elite, and the addict stratification system itself points to the fundamental importance of euphoria.

CONCLUSION

We have reviewed Lindesmith's arguments, including the experimental studies which he cites in their support (see n. 8), and found that they do not warrant the exclusion of positive reinforcement from the explanation of chronic addiction. We have cited other experimental studies showing that euphoric effects are able to sustain opiate use and that opiate-dependent persons can in fact experience euphoria. We have also cited evidence to show that addicts are quite able to judge and report the effects of the drugs they receive and that an interview methodology was therefore appropriate for addressing whether chronic addicts experience euphoria.

²¹ When prestige among addicts is discussed by writers on addiction, they tend to focus mainly on the more salient aspects of the hardcore role without realizing that these aspects take on their significance among addicts mainly as the result of their relation to producing euphoria. Thus, Sutter (1966) writes: "Prestige in the hierarchy of a dope fiend's world is allocated by the size of a person's habit and his success as a hustler" (p. 200). But both of these are related to the frequency of euphoria (see table 9)! Sutter's failure to attach more importance to euphoria appears to derive from the influence of the Lindesmith school (Sutter 1969, p. 822). Feldman (in manuscript) has elaborated a prestige hierarchy among users of various drugs based on the degree of *risk* attached to each drug, with heroin at the top. We would suggest that heroin places first in status also because of its pleurability, and that without this the risk would be pointless.

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In our own results, we showed that all but one of our long-term addicts experience euphoria from opiates, most of them at frequencies high enough to be of great theoretical interest. We have also traced some of the links that tie the experiencing of euphoria to the repeated use of opiates: addicts come to crave these euphoric effects, they therefore use drugs that produce euphoria, and they purposely take the large amounts of opiates needed in order to obtain it. Lack of money was the main reason given by all addicts for not getting high more often.

Elsewhere, by factor analysis, we have also shown that the internal consistency among our measures of the effects of opiates, and between them and other variables, bears out our contention that there are at least two reinforcement dimensions of importance to chronic addicts, and that particular effects, such as the impact effect, indeed have the meaning which addicts appear to attach to them (McAuliffe 1973). The orthogonality of the two factors bears out our contention that both dimensions contribute independently to addict behavior and that a combination-of-effects hypothesis is superior to a single-effect hypothesis.

Lindesmith's theory is called into question by these findings because of the trivial role it assigns to euphoria. It might well be asked what are the theoretical implications of allowing euphoria to assume greater importance. The remainder of our paper represents a brief answer to this question.

Elsewhere, we have shown that addicts view the two dimensions of reinforcement, euphoria and withdrawal, as the most important reasons for using opiates (McAuliffe 1973). According to our theory, the response tendency that is addiction starts to gain strength at the very beginning of opiate use and continues to grow incrementally with each of the many positive reinforcements experienced during the "honeymoon" period (see n. 7). With the onset of physical dependence, euphoria and withdrawal sickness combine in various proportions to yield a complex schedule of reinforcement for the typical long-term opiate addict. The exact weighting of each in the reinforcement schedule may vary from time to time within a given individual and from addict to addict.

At the time of injection, an addict who is sick from withdrawal and who has only a maintenance dose on hand is obviously satisfied to respond to just one component of his schedule. With a larger supply on hand, he almost always opts to respond to both components by reducing his sickness and enjoying euphoric effects too. Oftentimes, having done so, he will take another dose soon afterward, to produce even more intense euphoria. Having already attended to his withdrawal needs, this time his response is solely to the euphoric component. The weighting of these components across addicts ranges from one extreme, exemplified by our one addict who never experiences euphoria, to the other, exemplified by our dealer who said he got

high on every injection. At any given time, most addicts are distributed in intermediate positions, where they avoid withdrawal and receive intermittent positive rewards. *It is the history of reinforcement gained from using drugs in all of these ways that accounts for an individual's overall drug-derived motivation for being an addict.*

Although this total reinforcement from both sources may enter into the motivation to use drugs at any given time, there would appear to be some advantage to recognizing that it is the relation of the two components to each other that accounts for the unique hold of opiates over the individual. Were it not for the promise of euphoria, heroin addicts would be much less motivated to remain within the severely demanding schedule dictated by the abstinence syndrome.

While it must be appreciated that the physical dependency associated with certain drugs does introduce novel features into the total reinforcement schedule, it should also be emphasized that recognizing the contribution of hedonic effects to the total schedule brings opiate use and the persistent use of other drugs that do not lead to physical dependency together under the same broad classification of phenomena. In this sense, the theoretical discontinuity between the two types of chronic drug use that is implicit in Lindesmith's theory no longer obtains.

Clearer recognition of withdrawal sickness as but another potent source of reinforcement should also dispel some of the controversy as to whether "addiction" is defined as a physical phenomenon or as a psychological phenomenon and thus also clarify the related issue of whether drugs that do not entail physical dependency are "addicting." The distinction between the two conditions is certainly a valuable one, since one adds a potent reinforcer that the other lacks, but the decision to regard one or the other state as addiction proper is, from a theoretical standpoint, basically arbitrary. From a public relations standpoint, on the other hand, deciding one way or the other does confer certain advantages, according to one's purposes. At the present time, the adoption of one or the other viewpoint tends to be perceived as siding for or against certain theoretical positions, such as Lindesmith's, and hence what is mainly a semantic (and propaganda) issue receives more attention from scientists than it may deserve.

Of special significance to social scientists, perhaps, is the fact that physical dependence and the withdrawal-sickness phenomenon activate the addict's conception of himself as being legitimately in the sick role, with all of its attendant claims for special consideration and exemption from normal social responsibility (Parsons 1951, pp. 312, 440). Not only do addicts view themselves this way when it is to their advantage to do so, but so do important segments of the nonaddict population (Levine and Stephens 1971, pp. 5-6). The utility of this self-conception to the notoriously manipulative

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and extractive addict should not pass unnoticed, and persons who find themselves in therapeutic or personal relations with opiate addicts might well find it productive to unmask this claim early. The constant availability of such a claim as a self-pitying rationalization for continued use of drugs for pleasure is certainly a unique feature of the drugs that induce withdrawal distress. It is not hard to see that a theory which denies the pleasurable of opiates plays into this self-conception quite readily and hinders unmasking. Such misconceptions also feed the popular stereotype which attributes addict criminality mainly to a desperate need to avoid withdrawal, when in fact criminality is greatest among those addicts who are most devoted to euphoria (table 9).

Considering the broader range of addiction phenomena, we would suggest that the ability of almost all addicts to display their enjoyment of euphoria accounts in part for the ease with which large numbers of recruits are motivated to try opiates and to continue to use them despite any initial unpleasant reactions. Their own enjoyment of euphoria, in turn, leads these recruits to continue use until they themselves become physically dependent. Once dependent, the promise of euphoria holds the addict to his habit, and the pursuit of it drives up his tolerance, increases the high overhead cost of his addiction, and depletes his legitimate resources. If he remains determined to pursue euphoria at a maximum level in the face of these developments, rather than contenting himself merely with avoiding the abstinence syndrome, he must commit himself more completely to the life-style of the criminal addict. As such a hardcore addict, he is less likely to seek relief voluntarily and to respond to attempts at rehabilitation.

Finally, evidence from our own study and from the studies of others has indicated that success at obtaining euphoria is fundamental, not only to the social psychology of addiction, but also to its sociology, in that it constitutes a major basis for social stratification among addicts themselves.

Our analysis of the economics of addiction, as it interacts with the schedules of reinforcement peculiar to addicts, accounts for the appearance of two modal points (or major social classes of addicts) in the prestige hierarchy. We have termed these categories of addict "weekenders" and "hardcore addicts." Euphoria is prominent as a normative goal defining both of these conditions, but its greater emphasis in one, above all else, accounts for the fact that one impacts on society as far more deviant than the other.

This recognition of the central role of euphoria within the social system of the chronic addict opens up many interesting possibilities for further psychological, social psychological, and sociological analysis. Rarely can social scientists find such a remarkably convenient microcosm in which fundamental reinforcers are so clearly specified, in which the economics are

just complex enough to be interesting, and in which the phenomena of special interest to various disciplines shade into each other so visibly at every level.

In order to underscore the possibility for advancing our understanding in many theoretical directions by viewing addiction as a miniature social system in its own right, let us review certain basic observations. Our analysis pointed to an addict stratification system built around the two major psychopharmacological phenomena of opiates: withdrawal and euphoria. Addicts who can barely succeed at tending to their daily need to avoid withdrawal occupy the lowest prestige positions. In the higher prestige ranges, addicts are stratified by success in achieving euphoria. Apparently, much of the prestige attached to addict occupations (the "boss" hustler) derives from the significance of their relation to the achievement of euphoria. Thus, the social system and the value system of addicts are closely related to the hedonics of addiction at the individual level. Since the reinforcers that are universally considered, within the subculture, to be most fundamental operate at the individual, psychopharmacological level and their hedonic effects seem rather robust in the face of social influence,²² it would appear that the major values of this subculture emerge from these personal values, rather than vice versa. This should serve as a needed reminder that cultural values are not necessarily the autonomous, ultimate origin of all that they contain, while the relation between competence as an addict and success at achieving euphoria provides an important demonstration of the extent to which competence may underly stratification systems generally, deriving its legitimacy from consensus on personal values and from direct personal familiarity on everyone's part with the demands of the task in hand.

APPENDIX

In his first experiment, Beach gave rats morphine just prior to running them through a Y-maze during a 12-day training period. The prior morphine injections were always associated with running to one goal box rather than the other. Prior to runs to the other goal box, saline injections were given instead of morphine. Later, when given a choice of which way to run, the rats ran significantly more often into the goal box associated with the morphine injections. Beach and Lindesmith both interpret this as evidence for the negative-reinforcement, withdrawal-reduction effect of opiates.

²² Witness the double-blind, placebo experiments that employ street-level dosages of opiates, in which both addicts and nonaddicts experience euphoria, and the discomfort of withdrawal even when accompanied by intense social support (e.g., Yablonsky 1965, pp. 196-99), or when manifested by laboratory animals (e.g., Wei, Loh, and Way 1972).

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However, it is not recognized in experiments of this sort (including others cited by Lindesmith in this connection; by Nichols [1965]; Wikler [1965]; Weeks [1964]; and Beach [1957*b*]), that the impact effect and the withdrawal-reduction effect are completely confounded here, and that the rats' learning could have been reinforced by a combination of the two.

Each of these two effects is empirically independent of the other, since users who are not physiologically dependent can experience the impact effect (thereby calling into question any attempt to equate this simply with the rapid reduction of withdrawal distress), and addicts who are dependent can experience relief from withdrawal symptoms although the accompanying impact effects may vary greatly in their intensity. Some addicts, for example, operating under the urgency of oncoming withdrawal symptoms, will relieve their withdrawal discomfort first with a subcutaneous injection so that they can then take their time finding a vein in order to enjoy a stronger impact effect. Just such a case is presented in this paper (see n. 19).

It is true, however, that when dependency does exist the two effects may usually be experienced in such proximity to each other as to be conducive to confusion as to whether there are two effects or just one. For example, although Lindesmith himself acknowledges the pleasurable impact effect (1968, pp. 33-34), only a few pages further on (pp. 40-41) he suggests that the addict really mistakes the relief from withdrawal distress for a pleasurable impact. For example, he states: "The satisfaction of the addict's craving for drugs may itself be called a pleasure. The relief from withdrawal distress which an injection gives may also be so designated. So considered, the assertion that an addict uses drugs because he obtains pleasure or satisfaction from them is merely a tautology" (p. 176).

More refined analysis of reinforcement is, however, possible. Both theoretically and practically, not to mention subjectively, a distinction between positive and negative effects can be maintained, even in the case of addiction. In recent physiological psychology, Berlyne (1967) has traced these different effects to hypothetical arousal changes within different systems of the brain, with some complex feedback relations between the systems. Some confusion concerning the ultimate difference between positive and negative reinforcement in the case of opiate addiction becomes quite understandable, however, in light of Berlyne's more abstract analysis, in which he observes that "the most intense satisfactions may well come when the reward system is subjected simultaneously to arousal . . . and . . . to immediately prior relief of aversion" (p. 94). This is consistent with the well-known phenomenon of hedonic contrast (Beebe-Center 1932), where "the judged pleasantness of a pleasant stimulus was found to be higher when it immediately followed an unpleasant stimulus, and vice versa" (Berlyne 1967, p. 85). Therefore, in having their withdrawal discomfort relieved, concurrently with experiencing the pleasurable impact of morphine, Beach's

rats may well have been receiving positive reinforcement in the most exquisite form of all. This would also apply to the human addict. Nevertheless, even in view of this proximity of effects, it is unwarranted to treat them as a tautology.

Beach also designed an experiment to test the positively reinforcing effect, or what he called the "euphoric effect," of morphine. He recognized that in his prior experiment the rats had been permitted to remain in the goal box at the end of the maze for an hour at the end of each training run, and therefore euphoria may have reinforced their later preference for this goal box in the test runs. Therefore, he trained a new group of initially nonaddicted rats to prefer one goal box over the other, using morphine just as before, but this time they remained in their home cage for 20 minutes before being shunted through the maze during the training period. During later test runs, when given a choice, they, too, chose the morphine-associated goal box significantly more often. Since any relief of withdrawal must have taken place while waiting the 20 minutes in their home cage, Beach reasoned, correctly it would seem, that euphoria had reinforced their learning during the hour they spent in the goal box at the end of each training run. Although Beach did not make the distinction, it should be obvious that by euphoria he had in mind only what we have called the continuing effect.

In a third experiment, Beach tested the persistence of the rats' habits after administration of morphine had ceased and all withdrawal symptoms had disappeared. This occurred after the rats had been withdrawn from all contact with morphine and with the training apparatus for three weeks. Rats that had been initially reinforced by what Beach construed as relief of withdrawal, but by what we construe as a combination of negative and positive reinforcement, were compared with rats that had been reinforced by what Beach construed as euphoria only and what we construe as the continuing effect only (as in the second experiment). When again tested on the apparatus, the rats in the former category continued to exhibit a significant preference for the morphine-associated goal box, whereas the rats in the latter category showed only a weak preference that did not reach significance.

It is this last result, in particular, that both Beach and Lindesmith interpret as evidence that euphoric effects do not produce a lasting change in drug-seeking behavior, whereas withdrawal effects do. However, it should be apparent from our account that they are crediting withdrawal effects with the potential effects of one of the two major aspects of euphoria, and that in considering "euphoria" they are in reality considering only the continuing effect and are completely ignoring the potential contribution to persistent drug-seeking behavior of the impact effect. Furthermore, there were substantial differences in these experiments in the dosages and lengths of time during which the rats in the key groups were reinforced, as well as in the

manner of injection, all of which may have put the euphoria-only rats at a learning disadvantage.

Other studies cited by Lindesmith (1968, p. 126) that show a strong effect for withdrawal reduction also overlook its confounding with the impact effect. One striking example is contained in an article by Weeks (1964), who describes the impact effect in rats: "Some of the rats went into a sort of trance immediately on receiving the injection, sometimes resting on the peddle for about a minute" (p. 48), and then goes on in the next paragraph to dismiss its possible importance as a pleasurable effect in favor of an explanation based solely on the relief of withdrawal symptoms: "One might be tempted to assume at this point that the rat 'liked' the morphine, but it is important not to read human reactions and emotions into an animal's behavior. Moreover, although human morphine addicts say they 'like' the drug, even in humans it is not clear to what extent the drug is a positive pleasure and to what extent it simply brings relief from the rigors of abstinence. The fact is that the rat may not 'like' the morphine at all but has learned that pressing the pedal stops the punishment of early abstinence." Although Lindesmith claims that experimental studies by Nichols (1965), Wikler (1965), Weeks (1964) and Beach (1957*a*, 1957*b*) support his position that it is withdrawal distress and not euphoria that is the motivating factor in opiate addiction, these studies do not in any way rule out the importance of euphoria. All they show is merely that withdrawal symptoms constitute extremely effective reinforcement.

Comments by Nichols himself, whom Lindesmith cites especially, indicate that his own rejection of euphoria stems from an *a priori* conception of the nature of reinforcement (it can consist only of drive reduction) rather than from his own experimental evidence. He regards euphoria as "no more than an epiphenomenon of the reinforcement process," and concludes with the statement, "Finally, the most cogent reason for rejecting the euphoria hypothesis is that the data are essentially subjective and difficult to use in a scientific framework" (Nichols, in Wikler 1968, p. 306). These comments were elicited in response to a question which took the work of James Olds as a model for positive reinforcement. Olds, of course, has shown that animals will work in return for electrical stimulation delivered to certain areas of the brain ("the pleasure center"), even to the point of exhaustion, and that this work is not related to the reduction of any known drive (Olds 1955). The tendency of some experimenters to discount the importance of euphoria seems to be related to their behavioristic rejection of what they regard as "subjective" effects when working with animals and to their reliance upon Lindesmith for information concerning human addicts (see, for example, Nichols 1965; Nichols in Wikler 1968, p. 306; and the quotation from Weeks, above).

Lindesmith's claims notwithstanding, there is substantial evidence that

drug taking can be positively reinforced by opiates. We have already pointed out that Beach (1957a) has shown such an effect for euphoria. We have also commented upon aspects of his research that may have reduced the potency of his positive reinforcement. The two other experiments (Davis and Nichols 1962; Jones and Prada 1973) in which positive reinforcement was not observed were not designed mainly for this purpose, and consequently their procedures were inappropriate for inferring that positive reinforcement does not occur. For example, Davis and Nichols relied upon morphine concentrations that may well have been too low to elicit measurable positive effects in the number of trials allowed. In addition, oral administration confounded any positive drug effect with an aversive, bitter taste, and spread the dosages over the entire drinking period thus diluting any impact effect. Subsequent experimental work utilizing improved techniques that more closely resemble the features of human addiction has in fact succeeded in showing the effect of positive reinforcement in the absence of physiological dependence and social rewards (Claghorn, Ord, and Nagy 1965; Deneau 1969; Woods and Schuster 1968). If there is a lesson to be learned from pharmacological research, it is that when methods are crude or inappropriate one must not be too hasty in accepting negative findings (Wolf 1961).

Most recently, an experiment by Jones and Prada (1973) seems to show incidentally that dogs did not respond to positive reinforcement from opiates. However, in reporting their results, Jones and Prada averaged many dogs receiving extremely low dosages with comparatively few dogs receiving adequate dosages. This obscured the clear evidence of increased rates of responding by higher-dosage dogs during the second and third weeks of the three-week exposure period (see their fig. 4). During the first week the animals receiving the higher dosages showed the aversive reactions that often accompany initial exposure to opiates (vomiting). Nevertheless, the authors also averaged their low rates of responding from the first week in with those from the second and third weeks, when responding had increased. This, too, obscured the increases.

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Waiting, Exchange, and Power: The Distribution of Time in Social Systems¹

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So far as it limits productive uses of time, waiting generates distinct social and personal costs. The purpose of this paper is to explore the way these costs are distributed throughout a social structure and to identify the principles to which this allocation gives expression. The main proposition of our analysis is that the distribution of waiting time coincides with the distribution of power. This proposition is based on the assumption that an individual's power reflects the scarcity of the goods or skills he possesses; accordingly, the relationship between a server and client may be characterized in terms of organized dependency, for which waiting (under certain conditions) provides an accurate index. However, if delay is related to the client's position in a power network, then he may show deference to a server by an expressed willingness to wait, or a server may confirm or enhance his own status by deliberately causing him to wait. Secondary interactional modes thus come to subserve a relationship originally grounded in a supply-demand structure. The broader implications of this correlation allow us to characterize stratification systems in terms of the apportionment of time as well as the distribution of other kinds of resources.

Delay and congestion are relevant to the analysis of social systems because they undermine the efficiency with which these systems conduct their business. Indeed, one Russian economist (Lieberman 1968-69) recently observed that because of its enormous cost in terms of more productive activities foregone, delay in waiting rooms and queues merit the status of a social problem (pp. 12-16). A gross estimate of the dimensions of this problem is furnished by Orlov, who reports that the Soviet population wastes about 30 billion hours a year waiting during their shopping tours alone. This is the equivalent of a year's work for no less than 15 million men (*New York Times*, May 13, 1969, p. 17). Another study shows that monthly queuing for the payment of rent and utilities wastes at least 20 million man-hours a year in Moscow alone (*New York Times*, June 25, 1972, p. 23). If figures like these were aggregated for the entire service sector of the labor force, social inefficiency occasioned by clients' waiting would stand out even more dramatically.

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The problem of delay may be more acute in some societies than in others; however, no modern society can claim immunity in this respect. Every social system must "decide" not only how much different members are to be given from a collective supply of goods and services; there must also be a decision as to the priority in which their needs are to be satisfied. Queuing for resources is in this sense a fundamental process of social organization, regardless of the specific level of its affluence. Indeed, though the amount of waiting time per unit consumption may be minimal in the richer, consumer-oriented societies, a higher volume of consumption leaves open the possibility that more time is lost in waiting under conditions of affluence than under conditions of scarcity.

On the other hand, it may be said that the social costs of waiting, no matter where they are incurred or what their absolute level may be, merely derive from the summation over an entire population of rather negligible individual losses. But this does not seem to be the case. As one American commentator (Bradford 1971) puts it: "None of us would think of throwing away the nickels and quarters and dimes that accumulate in our pockets. But almost all of us do throw away the small-change time—five minutes here, a quarter hour there—that accumulates in any ordinary day. I figure I probably threw away a full working day in the dentist's office this past year, flicking sightlessly through old magazines" (p. 82). Even in the more opulent of modern societies, then, waiting time creates significant deficits for the individual as well as the system. At issue, however, is (1) the way such cost is distributed throughout a social structure and (2) the principles which govern this distribution. These questions are the subject of the present inquiry.

We begin with the assumption that delay is immediately caused by the relations of supply and demand: when the number of arrivals in some time unit is less than the number an organization can accommodate, waiting time will be relatively brief; but if the arrival rate exceeds the service rate, a "bottleneck" is created and a longer waiting period results. Delay is in this sense occasioned by limitations of access to goods and services. However, this model does not explain socially patterned variations in waiting time. We must therefore explore the institutional constraints which sustain observable levels of scarcity and which organize the priorities granted to different groups of clients. These constraints are shown to be the expressions of existing power relations.

As we proceed, however, we discover that a purely structural model is tied to the very assumptions it seeks to extend, inasmuch as it takes objective scarcity as its point of departure. We then demonstrate how scarcities grounded in structured power relations may be deliberately magnified by the very people engaged in these relations. And so delay is found to be

partially independent of supply and demand, in whose relation this variable was originally thought to find its exclusive source.

The above argument is informed by the assumption that time is a generalized resource which may be expended productively or wastefully with respect to the acquisition of other, more concrete advantages. As such, our analysis will not only help clarify the way "productive time" and "idle time" are allocated in a social system; it will also show how this distribution affirms and ever reinforces that system's power arrangements.

WAITING, SCARCITY, AND POWER

When economic exchange involves relatively massive demand for specialized services, disturbances at the level of synchronization of supply and demand results in congestion. Waiting thus finds its organizational precondition in the scarcities occasioned by an advanced division of labor.

Waiting is related to scarcity in two respects. When the demand for a good or service exceeds its absolute supply, people may queue up before it is actually made available in order to ensure they will be accommodated. Others will wait with no guarantee of being served.² This latter condition is most widespread and conspicuous in the Soviet Union, where, according to Orlov (*New York Times*, May 13, 1969, p. 17), an average shopper must often wait in long lines at three to five stores in order to buy the item he wants. The same kind of problem arises in more consumer-oriented countries during periods of peak demand, for example, intercity transportation during holidays, theaters and restaurants on weekends, bargain days in department stores, important sporting events, etc.

Regardless of the scarcity of a good, however, organizations tend to minimize the employment of servers; in doing so they minimize labor costs and enhance profits. Similarly, those who sell their skills tend to create queues so as to minimize their idle time. A second condition of waiting, then, is the ratio of supply of servers to demand for the services which they are prepared to offer. The fewer the servers in relation to the number of clients they must accommodate, the greater will be the average client's waiting time. Moreover, the greater the scarcity of a service, and the more inelastic the demand for that service, the less is a server compelled to reduce the waiting time of clients. Urgency of need thus minimizes the probability of "balking," that is, refusing to enter the queue, and "reneging," or abandoning the queue after having entered.

Waiting is patterned by the distribution of power in a social system. This assertion hinges on the assumption that power is directly associated with an

² Others wait with no possibility of being served. For a very convincing explanation of this practice, see Mann and Taylor (1969).

individual's scarcity as a social resource and, thereby, with his value as a member of a social unit (see Blau 1964, p. 118). Accordingly, the person who desires a valued service generally cannot gain immediate access to its dispenser but must instead wait until others are accommodated.

However, it would probably be more precise to say that the capacity to make others wait is a property of roles and not their incumbents. The petty bureaucrat or cashier, for example, may himself possess little that is of value to others; however, he governs access to resources which are. As a result, he is able to keep people of great substance waiting for as long as he sees fit. Of course, when a server's power derives solely from his access to his employer's resources, that power can only be exercised over clients. In the absence of valued personal qualities his position in the organization itself will be a lowly one.

Waiting and Exchange

After a certain point, waiting becomes a source of irritation not only because it may in itself be wearisome, boring, and annoying, but also because it increases the investment a person must make in order to obtain a service, thereby increasing its cost and decreasing the profit to be derived from it. This loss to the waiter is related to the fact that time is a finite resource; its use in any particular way implies the renunciation of other rewards and opportunities. Put differently, in waiting, usable time becomes a resource that is typically nonusable. This transformation is mediated by the power relation between server and client: time, whose use is ordinarily governed only by the client—that is to say, expended for the sake of a benefit that he alone desires—is transformed during the waiting period into a resource that is governed only by the one whom the client attends.

However, the formal interactional properties of waiting are independent of vicissitudes in its personal cost. To be able to make a person wait is, above all, to possess the capacity to modify his conduct in a manner congruent with one's own interests. To be delayed is in this light to be dependent upon the disposition of the one whom one is waiting for. The latter, in turn, and by virtue of this dependency, finds himself further confirmed in his position of power. Looked at in a different way, it may be said that while having to wait may under certain conditions be negative and harmful to the interests of particular individuals, it often furthers the interests of those who keep them waiting. Waiting is therefore a negative condition only when we confine ourselves to the standpoint of the person who is delayed.

The one-sidedness of this statement may be balanced by two considerations which specify its applicability. First, the disadvantage of the waiter may be a detriment to the server as well. For, the benefits of waiting (such as respite from previous interaction and an opportunity to prepare for subsequent involvement) presumably shrink as the costs of activities foregone

increase; their intersection constitutes grounds for the waiter to renege from the waiting channel without being served. Correspondingly, the benefits a server receives by keeping a particular client waiting may initially be of greater value to him than time spent servicing that client; however, the declining value to the server of keeping that client waiting further and the rising cost entailed in delaying service also reach a point of intersection. This convergence constitutes grounds for offering service. The relationship between the first intersection and the second is crucial: if he is to stay in business, the server had better decide to serve before the client decides to leave. Although waiting represents an unfavorable exchange position for a client, the very principles which make it so subject the server to pressures which mitigate the extent of his delay. However, these pressures vary in terms of the scarcity and value of service, which suppress the probability of renegeing. This leads back to our initial point that his scarcity enhances the exchange position of a valued server who, while needing clients, needs no particular client; he can therefore take his time about serving any one of them.

The Stationary Server

The highly advantageous position of the server is intensified when viewed in terms of a stationary server/mobile client model. This arrangement not only affirms the power of the former (for the latter must expend resources to come to obtain the service he offers) but also works to his advantage in other ways. First, the stationary server has at hand sufficient opportunities for alternative involvement to offset the loss to which the tardiness of a client would otherwise subject him; second, he has the power to schedule and thereby control the sequence and pace of his activity.

The latter advantage is most conspicuously instanced by the widespread practice (particularly common among physicians with a large following) of overscheduling—setting up two or more appointments at very narrow intervals in order to ensure that possible delays on the part of clients, or a run of quick services, will not leave the server with idle time. Yet, even when confronted with an empty waiting room the server has at his disposal enough alternative involvement materials to minimize his loss. These may involve “secondary queues,” for example, paper work, checking of supplies, necessary calls to colleagues and clients, etc. On the other hand, the waiter is usually unable to transport enough supplies to keep himself maximally occupied, at least from a productive standpoint. Even the client who can bring his business to the waiting room in a briefcase may find himself unable to work comfortably in this strange and perhaps distracting setting. He is then cut off from queues which await *his* service.

Thus, by making the client wait the server may often impose a loss

without suffering one himself. On the other hand, by reneging, the client fails to impose a substantial loss upon a server, who may continue to operate productively; yet, the client subjects himself to loss in terms of time already invested in waiting. Moreover, even when he is forced into idleness the server may charge his clients for the time lost. This is especially true among psychiatrists, whose rigid 30- or 50-minute treatment sessions prevent them from overscheduling. On the other hand, when the popular server is delayed and forces his client into excessive idleness he does not consider it necessary to make compensation.³

Just as the mobile client may find himself at the mercy of a stationary server, the mobile server can be used hard by a stationary client. This server may have to wait until the client is ready to be seen and may get caught up in other peoples' queues, for instance, traffic jams that occur between visits to clients. Hence the increasing reluctance on the part of professionals to leave their offices for fieldwork and house calls. This practice redounds to their moral as well as material benefit, on the basis of the "if you want an audience, you come to *me*" principle (see Spencer 1886, p. 105). This seems to be confirmed by the fact that professionals generally do go to clients whose status exceeds their own. The doctor or lawyer who refuses to conduct business in the homes of the ordinary will more often than not rush to the ailing or wailing Mr. Big, taking care not to keep him waiting.

On the other hand, there are some servers who by the very nature of their work are forced to be mobile. These include insurance agents, door-to-door salesmen, delivery men, messengers, repair men, subcontractors, and the like. Of these, it is perhaps the latter who embody the clearest exception to the tendency for server mobility to be a disadvantage (Glaser 1972, pp. 90-107). Because the popular building subcontractor can take on more work than he can finish directly, and then keep the customer hooked by beginning a job he will only finish at his own convenience, this type of server can make a virtue out of the absolute necessity of his mobility (but of course only while the market is in his favor).

³ There are exceptions, however. Thus, "on some airlines, when there is a flight delay, the passengers . . . may get a meal, a long-distance call or even an overnight hotel room, all free of charge." But "with few exceptions, the availability of complimentary services is left to the initiative of those passengers who are knowledgeable enough to request them. . . . Passengers who are bashful or unaware of their rights may get nothing. . . . The Civil Aeronautics Board said four airlines indicate they will take the initiative in informing the passenger of such services. All other carriers will provide information as to services only upon the passenger's request" (*Chicago Daily News*, September 2-3, 1972, p. 27). The same is true with regard to the practice of "bumping." On some flights, airlines will sell more tickets than there are seats, expecting a certain percentage of those with reservations not to show up. If all do appear, the last ones to have made reservations are rejected and made to wait for the next flight. The CAB regulations entitle those so delayed to immediate compensation in the form of a fine equal to the price of the ticket (within a \$25-\$200 limit). Until recently, this right was honored only at the passenger's request (*Chicago Daily News*, October 10, 1972, p. 28).

Stratification of Waiting

Typical relationships obtain between the individual's position within a social system and the extent to which he waits for and is waited for by other members of the system. In general, the more powerful and important a person is, the more others' access to him must be regulated. Thus, the least powerful may almost always be approached at will; the most powerful are seen only "by appointment." Moreover, because of heavy demands on their time, important people are most likely to violate the terms of appointments and keep their clients waiting. It is also true that the powerful tend not to ask for appointments with their own subordinates; rather, the lowly are summoned—which is grounds for them to cancel their own arrangements so as not to "keep the boss waiting."

The lowly must not only wait for their appointments with superiors; they may also be called upon to wait during the appointment itself. This may be confirmed in innumerable ways. For one, consider everyday life in bureaucracies. When, in their offices, superordinates find themselves in the company of a subordinate, they may interrupt the business at hand to, say, take a phone call, causing the inferior to wait until the conversation is finished. Such interruption may be extremely discomfiting for the latter, who may wish not to be privy to the content of the conversation but, having no materials with which to express alternative involvement, must wait in this exposed state until his superior is ready to reengage him. The event becomes doubly disturbing when the superior is unable to recover from the distraction, loses his train of thought, and is unable to properly devote himself to the moment's business. Of course, the subordinate is demeaned not only by the objective features of this scene but also by his realization that for more important clients the superior would have placed an embargo on all incoming calls or visitors. He would have made others wait. The assumption that the client correctly makes is that his own worth is not sufficient to permit the superior to renounce other engagements; being unworthy of full engagement, he is seen, so to speak, between the superior's other appointments. In this way, the client is compelled to bear witness to the mortification of his own worthiness for proper social interaction.

While the derogatory implications for self are clear when the person must repeatedly step aside and wait until the superordinate decides that the granting of his time will not be excessively costly, debasement of self may be attenuated by the client's own consideration that his superior is, after all, in a position of responsibility and assailed by demands over which he may not exercise as much control as he would like. But even this comforting account may be unavailable when the server himself initiates the interruption. It is possible for him to make a call, for example, or to continue his work after the client enters, perhaps with the announcement that he will "be through in a minute."

It is especially mortifying when the superior initiates a wait when engagement is in progress. Thus, a subordinate, while strolling along a corridor in conversation with his superior may find himself utterly at a disadvantage when the latter encounters a colleague and breaks off the ongoing relationship in his favor. The subordinate (who may not do the same when encountering one of his peers) is compelled to defer by standing aside and waiting until the unanticipated conversation is finished. Nothing less than being expected by his superior, who, finding himself gaining less from the engagement than his inferior, assumes the right to delay or interrupt it at will, if more profitable opportunities should arise.

The immunity of the privileged.—The relationship between rank and accessibility implies that waiting is a process which mediates interchange between those who stand on different sides of a social boundary. The divisions and the rules of access which correspond to them are found in organizations which are themselves bounded with respect to the outside world. This fact raises the problem of access when outsiders or clients as well as insiders, that is, employees or co-workers) seek contact with persons situated at different points in a service hierarchy:

Low down on the scale are the men you can walk right up to. They are usually behind a counter waiting to serve you on the main floor, or at least on the lower floors. As you go up the bureaucracy you find people on the higher floors and in offices: first bullpens, then private offices, then private offices with secretaries—increasing with each step the accessibility and therefore the necessity for appointments and the opportunity to keep people waiting. Recently, for example, I had an experience with a credit card company. First, I went to the first floor where I gave my complaint to the girl at the desk. She couldn't help me and sent me to the eighth floor to talk to someone in a bullpen. He came out, after a suitable waiting time, to discuss my problem in the reception room. I thought that if I were to straighten this matter out I was going to have to find a vice-president in charge of something, who would keep me waiting the rest of the day. I didn't have time to wait so I took my chance with said clerk, who, of course, didn't come through. I'm still waiting for the time when I have an afternoon to waste to go back and find the vice-president to get my account straightened out.⁴

The above statement suggests that delaying a typical client may be a prerogative of important servers. However, we must also recognize that powerful clients are relatively immune from waiting. This remark accords with Tawney's (1931) emphasis on the asymmetry of power relationships. "Power," he writes, "may be defined as the capacity of an individual, or group of individuals, to modify the conduct of other individuals or groups in the manner which he desires, and to prevent his own conduct from being modified in the manner in which he does not" (p. 229; emphasis added).

⁴ Personal communication from Florence Levinsohn.

The relative immunity from waiting which the powerful enjoy is guaranteed because they have the resources to refuse to wait; that is, because they can often afford to go elsewhere for faster service or cause others, such as servants or employees, to wait in their places. Thus, while the relationship between privilege and the necessity of waiting cannot be generalized in any deterministic way, there appears nevertheless to be a relationship between the two, with the least-privileged clients compelled to do the most waiting. This general statement is consistent with Mann's (1969) more specific observations regarding the stratification of waiting in lined queues:

The relationship between cultural equality and public orderliness is attenuated in the area of queueing because waiting in line is not a habit of all social classes in Western society. It is reasonable to suppose that if Mrs. Gottrocks joined a theater or a football line in the United States, Australia, or England, she would not be treated differently than anyone else, but it would be a rare event for someone of Mrs. Gottrock's status to use a line. Ordinarily, in both class-conscious and relatively class-free societies, the privileged class circumvent the line altogether and get their tickets through agents or other contacts.⁵ Our point, then, is that queueing is confined largely to the less-privileged groups in society. [P. 353]

The privileged also wait less because they are least likely to tolerate its costs; they are more inclined to renege from as well as balk at entering congested waiting channels. On the other hand, the less advantaged may wait longer not only because of their lack of resources but also because their willingness to wait exceeds the readiness of those in higher strata. While they might have something else to do besides sitting and waiting, they might not have anything better to do. As a result, the least advantaged may pay less in profitable alternatives foregone and therefore suffer less than even those whose objective wait is shorter.

This relationship may be informed by another consideration, for which health-care delivery systems provide an example. Because of their scarcity, those who are able to pay for medical services are often forced to wait well beyond the time a server agreed to provide them. Yet there is some limit to the server's inconsiderateness, for, in principle at least, the client may decide that he has waited long enough and go elsewhere. On the other hand, those who are unable to pay for medical care may spend the better part of the day in outpatient waiting rooms, for consideration of the value of clients' time is far less imperative when these clients cannot take their business to someone else. In Britain's government-run maternity hospitals, for example, "a major complaint was that women dependent on the health service are treated offhandedly in hospitals and frequently have to wait

⁵ Other "contacts" include the radio, over which Saturday and Sunday morning waiting times at many metropolitan golf courses are broadcast. This service, which saves many players many long delays, is performed almost exclusively for the middle and upper-middle classes.

more than an hour for checkups at antenatal clinics. Women who paid up to \$700 for private treatment were dealt with speedily and efficiently" (*Chicago Tribune*, June 12, 1971, p. 10). Thus, while long, agonizing waiting periods may be avoided only if one is willing to settle for more expensive service, the poor may avoid waiting only if they are willing to settle for no service at all. (The frequency with which they do select this option is, of course, unknown—as is the consequence of the selection.)

The above principle may be further illustrated in other, altogether different connections. It is noticeable, for one example, that in the "best" of urban department stores a customer is met by a salesperson as soon as he enters; the customer makes a selection under his guidance and makes payment to him. In establishments which are a grade below the best, customers may have difficulty finding someone to serve them during busy periods but, when they do, are accompanied by him, that is, "waited on," until the transaction is consummated by payment. The lowest-grade stores, however, provide few servers; as a result, customers must for the most part wait on themselves, then line up behind others at a cashier counter in order to make payment.

The above patterns are to be observed within as well as among organizations. In the typical department store, customers surveying high-priced goods like furniture and appliances will typically be approached immediately by a salesperson. Those in the process of selecting a handkerchief or pair of socks will not be so quickly attended and, when they finally are, will be dealt with more quickly. Likewise, clients who show interest in very expensive jewelry will be served at once and at length; those who are fascinated with costume jewelry will wait.

In general, it may be said that establishments which cater to a relatively wealthy clientele must serve them quickly (if the clients desire) not only because of the objective or assumed value of clients' time but also because they have the means to take their business elsewhere if it is not respected. Commercial places which service the less wealthy are less constrained in this respect because they tend to deal with a larger and/or less independent clientele. Within organizations, clients who promise to bring the most profit to a server enjoy a competitive advantage; they wait the least, to the disadvantage of their lesser endowed brethren who can find no one to honor the value of their time.⁶

⁶ Even when circumstances make it necessary for the resourceful to wait, they suffer less than their inferiors. As a general rule, the wealthier the clientele, the more adequate the waiting accommodations. Thus, persons who can afford bail can await their trial (or, far more frequently, attorneys' bargaining on their behalf) in the free community. The poor must wait in jail. The same is true of facilities. In airports, for example, those who can afford it may simultaneously avoid contamination by the masses and engross themselves in a variety of activities, including fabulous eating and drinking, in "VIP lounges." The term "lounge" instead of the vulgar "waiting area" or "gate" is

Waiting and the Monopolization of Services

The above rule, however, rests on the assumption that faster alternative services are available to those who want and can pay for them. In fact, the availability of such alternatives is itself variable. Waiting is therefore affected not only by clients' resources and consequent ability to go elsewhere for service but also by the opportunity to do so.

It follows that establishments with many competitors are most likely to be concerned about the amount of time they keep clients waiting. Chicago Loop banks are among such organizations. In the words of one banking consultant, "The industry is too competitive to allow a dozen people waiting in line when they could just as easily take their business across the street where there is a teller at every window, a customer at every teller and waiting time is less than one minute" (*Chicago Tribune*, September 28, 1971, p. 7). However, organizations with few or no competitors are less obliged to reduce the waiting time of clients. (This condition makes waiting a national pastime in the Soviet Union, where most services are rendered by government-run establishments that are not subject to market forces.)

The enormous amounts of waiting time expended in dealings with public people-serving bureaucracies is directly related to monopolization of the various services which they offer or impose. Monopolization accords governmental units the power to maximize their efficiency of operation by minimizing service costs and, in so doing, maximizing client waiting. This "optimum solution" is exemplified by bureaus which distribute welfare benefits to long lines of disadvantaged people:

The number of Medicaid and public assistance applicants and recipients has become so great that [New York's] Department of Social Services is literally shutting its doors in their faces.

Many of the 45 social service centers close their doors early—12, 1 or 2 o'clock—rather than admit persons the workers realistically know cannot be seen that particular day.

The Medicaid office advises applicants to line up outside the doors before dawn. "You'd better get down here around 6:30 or 7 o'clock," said a person answering the telephone at the Medicaid office. . . . "We can only see 200 persons a day. If you want to be in the first 200 you better get here then—with your application filled out." The Medicaid office does not open until 3:30 A.M. . . .

Last week the department announced it had saved \$39 million by employing fewer case workers. [*New York Times*, November 21, 1971, p. 58]

However, the relatively wealthy as well as the poor are put to incon-

also applied to facilities set aside for those who travel a specified number of miles with (and pay a substantial sum of money to) a particular airline. In this as in many other settings, waiting locales for the poor and less rich lack the elaborate involvement supplies, pleasant decor, and other physical and psychological comforts that diminish the pain of waiting among those who are better off.

venience by having to wait in person for licenses, permits, visas, tickets, information and the like. Dealings with government-sponsored transportation facilities can also be cited as an example:

Before Amtrak took over, I would have had to call the Illinois Central to go to Miami. If I wanted to go to New York, I'd call the Penn Central. To go west, the Santa Fe. But now, under the streamlined, tax-supported Amtrak, one number, one central office, makes the reservations. They have computers and other modern devices the old system didn't have.

At 10 minutes after noon, I dialed the new Amtrak reservation number. The line was busy, so I hung up and waited a few minutes and dialed again. It was still busy. Five minutes later, I tried again. It was busy. By 1 o'clock I had tried 10 times, and had heard only busy signals.

Enough was enough. I phoned the Amtrak executive office, to ask what was wrong with their reservation number. A woman there put me on hold. I was on hold for seven minutes. Then when she finally took me off hold, she switched me to somebody's office, and a secretary laughed and said: "Oh, yes, our lines are very busy."

At 2 P.M. it finally happened. Instead of getting a busy signal, it rang. It actually rang. . . . It rang. And it rang. And it rang. For eight minutes it rang. . . . So I hung up, got another cup of coffee and tried again. That was a mistake, because I heard another busy signal.

Then at 2:47 it happened. It rang. And somebody answered. I listened closely to make sure it wasn't a recorded message. No, it was really somebody alive. After that it was easy. In about eight or nine minutes the reservations were made.

The clock said 3 P.M. So I have to congratulate Amtrak. It took me only two hours and 50 minutes to complete a telephone call and make reservations. It would have probably taken me at least 10 minutes more than that to take a cab to O'Hare, board a plane, fly to Miami, and get off the plane. [*Chicago Daily News*, June 9, 1972, p. 3]

This instance is an especially informative one, for it demonstrates that the amount of time clients of an organization are called upon to wait is in large measure determined by the broader competitive structure in which that organization is situated. Longitudinal and cross-sectional means are brought to bear in this assessment. By reference to the temporal barrier to access to rail service after centralization and monopolization, relative ease of access before the transformation is implicitly affirmed. And after documenting the lengthy waiting time required in a noncompetitive service market, we find explicit reference to the ready availability of service offered in highly competitive ones (airlines, in this case). In this double sense, the institutional grounding of waiting time is a conclusion warranted by the facts.

We now turn to public services which by their very nature admit of no alternatives and which at the same time are so organized as to constitute the most radical instance of the principle we are now discussing.

A day in court.—Discrepancy between demand for and supply of "authori-

tative judgment" is perhaps the most notorious source of waiting for both rich and poor. In fact, those who look forward to their "day in court," whether civil, criminal, or juvenile, very often find themselves spending their day in the courthouse corridor (many courts do not provide waiting rooms). In some courts, in fact, all parties whose cases are scheduled to be heard on a particular day are instructed to be present at its beginning when the judge arrives.⁷ This is a most pronounced manifestation of what we earlier referred to as "overscheduling," which in this case ensures that the judge (whose bench is separated from his office or working area) will not be left with idle time that cannot be put to productive use—a consideration which may help us understand the seemingly irrational practice of assembling together at the beginning of the day those who are to be served during its course. While this tactic guarantees that the judge's valuable time will not be wasted, it also ensures that most parties will be kept waiting for a substantial period of time; some, all day long. Indeed, because they have no means to retaliate against the judge's own tardiness or excessive lunch breaks, some individuals may not be served at all and must return on the next day to wait further. Clients' attorneys, incidentally, keep them company during much of this time—a service for which the former pay dearly.

All of this is not to say that the organization of justice profits. It must, on the contrary, pay a very high price for support of its *prima donnas*. As one juvenile-court officer puts it: "[W]aiting to be called into court . . . is the most serious problem. Just from an internal point of view this means that a probation counselor usually accomplishes nothing in the hour or more he often has to wait to get his case into court. Usually during this waiting period he sees no people, does no counselling, can't do dictation or other 'desk-work'—his wait is complete, unproductive waste. These same problems apply to other professional people: caseworkers from the Department of Social Services, school principals, lawyers, etc." (Fairfax County [Virginia] Juvenile and Domestic Relations Court, Memorandum, 1971, p. 1). While attorneys⁸ and other professionals are fortunate enough to claim a

⁷ A functional equivalent is found in the Soviet Union. "Aleksandr Y. Kabalkin and Vadim M. Khinchuk . . . describe what they termed 'classic cases' in everyday life in the Soviet Union, in which customers wait for the television repairman or for a messenger delivering a train or plane ticket that had been ordered by phone. To the question 'About what time can I expect you?' the stereotyped reply is, 'It can be any time during the day.' And people have to excuse themselves from work and wait—there is no other way out" (*New York Times*, November 7, 1971, p. 5).

⁸ It may not be assumed that all lawyers earn while they wait. For example, the *New York Times* (August 25, 1971, p. 24) recently reported: "A lawyer who specializes in prosecuting landlords' claims against tenants asked permission in Bronx Supreme Court yesterday to bring his cases there rather than in Civil Court because . . . he spent much time 'just sitting and waiting.' And consequently, he said, he was suffering 'financial loss' and felt he could not continue working in Civil Court."

fee for doing nothing in a professional way, others are often denied this luxury. Authorities who are mindful of civil security, for example, wisely find it more expedient to dismiss cases (particularly such misdemeanants as traffic violators) for lack of witnesses and evidence than to tie up a large sector of the police force for the better part of the day in a crowded corridor. In this particular sense, the police are too important—their time too valuable—to be kept waiting. On the other hand, it may be claimed that by tying up defendants all day long in these same corridors justice may be served—provided, of course, that the defendants are in fact guilty as charged. However, the situation is quite different in felony cases, where casual dismissals are less probable. Under these circumstances police wait as long as defendants. In the Chicago Gun Court, for example, “40 or 45 police are waiting to testify at 9:30 A.M., when court begins. Cases are not scheduled for specific times, so most of them wait and wait. One recent day 31 were still waiting around at 1 P.M. The next day 20 were there at 1 P.M. And 23 the following day.” The same conditions prevail at the Narcotics Court where police waiting time “translates on an annual basis to 13,000 police days lost and \$700,000 in expenses” (*Chicago Daily News*, August 21, 1973, p. 14).

Two observations emerge from and transcend the particular content of what has just been said. First, the assertion that clients may pay a high price, in terms of time, in their dealings with public bureaucracies means that a societal cost, expressed in terms of aggregate client time diverted from more productive activities, must be written into the usually implicit but sometimes explicit “optimum solution formulae” by which particular “public service” organizations maximize their own efficiency. Because of this factor, the real cost of governmental services is not to be obviated by budgetary considerations alone.

Second, minimization of a powerful server’s idle time may subtract from the productivity of the organization as well as its clients. This observation, which is merely grotesquely evident in court settings, reflects the general principle that increments in efficiency in one part of a social organization often entail malfunction in other sectors. Accordingly, just as high concentration of power in an organization may lend itself to societal inefficiency, indexed by more productive client-time foregone, so concentration of power and honor in an elevated server may render organizations ineffective by maximizing idle time of subordinated servers. The more general import of this statement is that it amends the overly simplistic scarcity theory of waiting, which fixates our attention upon server shortage as a condition of client delay. The present statement shows that the organization of services, as well as their volume, provides occasion for waiting.

An additional point is that some persons and groups are relatively exempt from waiting. If we turn our attention once more to the courtroom,

we find that the powerful are most likely to enjoy such advantage. In making up the docket, for example, resources are taken into account. Defendants who are represented by an attorney are very often scheduled before those who are not (in Chicago traffic courts, at least). And cases involving important and powerful contestants, witnesses, and/or lawyers may be scheduled at their convenience and not be delayed for long periods of time. Similarly, attorneys who enjoy favor with the court clerk are also able to avoid long waits because they are allowed to schedule their case early.⁹ Thus, while waiting time may be maximized by persons or in organizations which enjoy full or near monopoly on the services they offer, the relationship between the power and waiting time of their clients is probably attenuated rather than negated. For, while the powerful may lack the opportunity to take their business elsewhere, they nevertheless possess the resources to ensure that their needs will be accommodated before the needs of those with fewer means.

The resource-availability theory.—In summary, the relationship between servers' and clients' power in relation to waiting is asymmetrical. On the one hand, servers' holding power is contingent on clients' inability to frequent more distant and/or expensive servers; on the other, client autonomy requires the presence of alternative services. Despite their covariation, though, resources and alternatives seem to affect waiting time independently of one another. The resourceful wait less within both monopolistic and competitive organizations; regardless of clients' resources, however, waiting time tends to be longer in monopolistic settings. This "resource-availability theory" of waiting may also help explain the varying "optimum solutions" adopted by diverse organizations seeking the most profitable balance between losses due to keeping clients waiting and the expense of additional servers. While the theory predictably suggests that the balance arrived at reflects the relative power of organizations or individual servers, it also holds such resolutions to be "zero sum" in nature. This is most evident in monopolistic, "public service" bureaucracies or among charismatic officials who maximize the efficiency of their operation at great organizational and social cost, expressed in terms of productive time lost through waiting. The optimization of unit interests is thus often brought about at the expense of system interests.

However, the resource-availability theory must be qualified by recognizing existing limitations on monopolies' capacity to restrict service. For, despite their freedom from competition, monopolies must get their work

⁹ This is to say that, as a scarce commodity, time or priority of service routinely becomes the object of struggle. Recognizing this, a court intake officer writes in a memo to his supervisor: "Intake counselors should assume more control over the setting of cases on the docket, with a proportionate decrease in the control now exercised by clerks" (Fairfax County [Virginia] Juvenile and Domestic Relations Court, Memorandum, 1971, p. 1).

done, lest their managing personnel be subjected to pressures from those who have the power to exert them. While monopolistic enterprises may not be responsive to their clients' evaluations, they are often subject to officials, such as elected representatives, who are not free to ignore these clients. If this be so, then the greater the number of people who are dependent upon a monopolized service, the more effective will be the pressure for its public regulation. Thus, while the telephone company is a monopoly, it does not enjoy unlimited freedom to reduce service and delay callers. The same can be said of the postal service (which is in fact sometimes overstaffed because it is a nest for political patronage) and other public utilities. Waiting time, then, is affected by political as well as economic constraints.

SOCIAL PSYCHOLOGICAL ASPECTS OF DELAY

On the psychological level, what has been said may be recapitulated in the following terms. The person who is delayed is not merely in a condition of objective dependence and subordination; because his only duty is to attend the call of a server, the waiter feels dependent and subordinate. To be kept waiting—especially to be kept waiting an unusually long while—is to be the subject of an assertion that one's own time (and, therefore, one's social worth) is less valuable than the time and worth of the one who imposes the wait. This is why objections to being kept waiting sometimes take the form, "Why should I wait for him? My time is as valuable as his!" The actually inferior feeling that often gives rise to such a protest is especially common in such places as crowded waiting rooms, wherein each client, confronting multiple reflections of himself, is more pointedly made aware of his suppliant status and of how utterly insignificant he is as compared with the person for whom he waits. Of course, waiting does not create the sense of subordination but only accentuates an initial inferiority, which is often presupposed by the fact that one is waiting in the first place. It needs to be said that this same sentiment has its parallel on the other side of the relationship, for the server calls out in himself the responses that he elicits in the ones he keeps waiting, which enables him not only to be conscious of his own power—to see himself from the point of view of his clients—but also to feel within himself the independent power that he extracts from those who wait for him.

Waiting as a Determinant of the Value of Service

The above statement is one elaboration on a theme to which we have tried to adhere throughout this report, namely, that waiting presupposes and occurs within an established context of power relations and is to be understood in terms of these relations. Power, to repeat, entails among other

things the capacity to provide scarce services which people must wait to receive. The significance of the service for the individual and the social power of the dispenser therefore hinges on its desirability.

"The only relevant question apart from the direct enjoyment of things for their qualities," wrote Simmel (1971), "is the question of the way to them. As soon as this way is a long and difficult one, involving sacrifice in patience, disappointment, toil, inconvenience, feats of self-denial, etc., we call the object scarce. One can express this directly: things are not difficult to obtain because they are scarce, but they are scarce because they are difficult to obtain" (p. 68). Accordingly, if we regard waiting for a scarce service as an investment or sacrifice in return for a gain, we may measure part of the value of the gain by assessing the degree of sacrifice occasioned on its behalf. In Simmel's (1970) words, "Valuation arises from the fact that something must be paid for things: the patience of waiting . . . the renunciation of things otherwise desirable" (p. 23).

The subjective value of the gain is therefore given not only by the objective value of the service but also by the amount of time invested in its attainment. This being the case, one may wait for another not only because he is a source of value; the other's service becomes valuable (and he becomes powerful) precisely because he is waited for. While analytically distinct, the two parts of this phenomenon are empirically inseparable. What is more important is that they are functionally inseparable; this is to say that waiting subserves the distribution of power that it presupposes. It does so in two interrelated senses: a common willingness to wait for a service sustains its objective scarcity which, in turn, transforms itself (as we have seen) into a subjective value. This principle is particularly clear in its negative aspect: in the observation that services to which we have immediate access—which we can acquire without waiting—are of relatively little value to us. It is known, for example, that in seeking professional help from a person with whom he is unacquainted, the client does not always rejoice at being granted an immediate appointment, nor at finding an empty waiting room when he arrives for this appointment. Such ease of access may speak unfavorably of the server's scarcity as a social or economic resource; it may disconfirm the worth of his service. In contrast, those who confront obstacles to service tend to have more confidence in its value, once it is acquired.

The above principle holds within but not outside of specific time limits. Beyond their upper boundary, a desired and valued service may be considered unattainable, and an otherwise willing client might just give up and renege. Those who choose to wait it out beyond that limit may, in so doing, find their estimation of the service to be actually lowered. This is because persons tend not only to place a higher value on services for which they must wait; they also demand more in proportion as they wait. After a

certain point, the latter tendency may outweigh the former, raising expectations to such a level as to render their satisfaction impossible. The reward then cannot possibly be worth waiting for, let alone enhanced by waiting for it.

It follows that if services acquirable without waiting are of little value to us, those who wait to service us may be attributed a negative value; these servers become our subordinates. Simmel put this more generally by saying: "We perceive the specific value of something obtained without difficulty as a gift of fortune only on the grounds of the significance which things have for us that are hard to come by and measured by sacrifice. It is the same value, but with the negative sign" (1971, p. 54). The original meaning of the term "waiter" accords with this formula. The waiter is, in this earlier sense, one who stands by, alert to the call and ready to respond to the demand of a superordinate. What the waiter waits for, then, is a command; he is, as the French expression makes clear, an *attendant*: one who caters to the whims of the ascendant. This earlier, courtly reference to waiting as a form of subordination is found even today (as a "survival," so to speak) in southeastern parts of the United States where we observe the very common substitution of "waiting *on*" (someone or other) in place of "waiting *for*." The mere transformation of the linguistic meaning of waiting, from a readiness to serve to a readiness to be served, has therefore not fully negated its essential sociological property: to wait on others and to be kept waiting exhibit the common element of subordination.

We have digressed in order to demonstrate the inverse case of a principle to which we now return, namely, that waiting is not simply a barrier to service but is rather the very condition of its subjective value. This idea must be addressed in further detail because it appears to contradict our earlier assertion that waiting is inimical to profit in social exchange. It now seems that the reduction of waiting time would not necessarily increase profit for a client (in an exchange with a server) because the value of that which is attended is itself dependent, at least in part, upon the very length of attendance. But from this an absurd hypothesis is deduced: that persons faced with the alternative of, say, a long and a short queue will join the longer one in order to enhance the value of what they will receive at the end of it (much as an individual might extend his feet from under a blanket on a cold night in order to enjoy the warmth that its withdrawal will provide). Absurd as it appears, there is some truth in this; but only in the following, limited sense: that those who wait the longest tend to value what they receive the most. But this only means that the subjective value of the service, that is, its value for the waiter, is positively modified in the very act of waiting, even though waiting itself is not desired, or, more precisely, simply because it is not desired. Therefore, the contradiction between

this principle and the earlier one, which finds waiting to subtract from the profitability of an exchange with a server, is obviated by the term subjective value, to which the objective observer would be quite indifferent.

Making Others Wait

That waiting (within the limits referred to) will render a service more valuable, independently of its objective worth, seems to be an inherent feature of the psychology of social exchange. This property is perhaps made most intelligible by the principle of cognitive balance, which, according to Alexander and Simpson (1964, pp. 182-92); tends to equilibrate psychological investment and profit (for a more general statement, see Festinger [1957]). However, Simmel's was the first systematic discussion of this principle. His treatment is summarized in the observation that "even if [objects or services] possess no intrinsic . . . interest, a substitute for this is furnished by the mere difficulty of acquiring them: they are worth as much as they cost. It then comes to appear that they cost what they are worth."

We may turn to an important implication of this principle. Because the worth of a person is not independent of the amount of time others must wait for him, that person can maintain and dramatize his worth by purposely causing another to wait.

Of course, the imposition of a waiting period does not in itself make a person or his services valuable; it can only magnify existing positive evaluations or transform neutral feelings into positive ones. If these initial feelings are not favorable, or at least neutral, the waiting caused by a server may lower clients' estimations of his worth. Instead of a sought-after and important man, the server becomes an incompetent who cannot perform his job properly; thus is his initial inferiority confirmed. (This is why subordinates who know where they stand do not like to keep their superiors waiting.) Generally, the dramatization of ascendancy by keeping another waiting will do a server the most good when his social rank exceeds that of his client or when the difference between their ranks is ambiguous. In the latter case, ascendancy accrues to him who can best dramatize it; in the former, ascendancy may be dramatized by him to whom it already accrues.

Thus, just as authority is affirmed by the placement of social distance between super and subordinate, so temporal distance subserves the ascendancy of the person who imposes it. More precisely, the restriction of access to oneself by forcing another to "cool his heels" is instrumental to the cultivation of social distance. The importance of this point resides in its inconsistency with the assumption that waiting is primarily dependent upon the supply of servers and demand for their services. The kind of waiting

to which we now call attention is "ritual waiting," imposed without reference to scarcity of server time.

Now, ritual waiting is a form of mystification.

Waiting and Mystification

Causing another to wait is a form of "mystification" (see Goffman 1959, pp. 67-70) because self-imposed restriction on accessibility underscores a server's scarcity and social value, thereby promoting awe among those who wait for him. Notwithstanding our own attempt at elaboration and extension, however, this line of thought does not take us far enough. For, if the reverence in which a server is held is to be profoundly felt, it must rest not only upon the essentially negative capacity to regulate access to himself; the server must also display the more positive ability to satisfy needs or alleviate tension within the person waiting for him. In holding himself apart, then, the charismatic server must also "do something" for the client. Furthermore, if whatever is done is to dazzle the client, its efficacy must apparently derive from the very person of the server, independently of the particular substantive benefits he is capable of providing. The latter, it might seem to the client, flow from the status of the server, but not from his specific individuality.

This consideration enables us to see in the ability to make others wait an ideal resource for mystification. For, when, after waiting some time, a client's turn is finally called, the summons itself fulfills a need which, having been generated by the distress of waiting for a service, can have nothing to do with the need for the service itself. It may be argued that the distinction between these two sources of tension is merely of analytic worth; that, empirically, they merge insofar as the waiting period may exacerbate anxiety over the condition that requires servicing. But this objection only confirms the fact that by simply making himself available the server can display a remarkable personal capacity to alleviate suffering. Because he is so intensely waited for, his very appearance makes us feel better. Hence the impression of an inherent power to relieve stress. (In this regard, see Bettelheim [1960], p. 87.) Because he is explicitly defined as the one to wait for (with all the messianic implications of such definition), the tension attending the wait can be relieved in no way other than through his appearance.

Lest it appear that the delay creates rather than enhances attraction, we should stress that the above statements are preconditioned by a performer's initial appeal. In this connection, an additional qualification must be made. Causing a delay will not only fail to enhance the status of an unattractive server; it will also fail to elevate the server who cannot conceal from a

client the fact that he is deliberately making him wait. For, if the object of imposing a delay is to give the impression of important business when none really exists, then the initial sense of awe must turn into infuriation when the mystery of power is seen through.

Servers who do not serve.—In suggesting that a server may dramatize the scarcity and value of his skills by making others wait for him we imply that he eventually must appear and provide his services, otherwise he could not possibly profit from their increased value. This implication is certainly valid in connection with most server-client relationships; but it does not flow from the most pronounced form of the histrionics of scarcity: when, in the face of the most intense anticipation, the server never appears! The sense of awe thereby occasioned, moreover, is perhaps most poignant when the server himself is unknown to his attendants, for what then emerges is the unadulterated sense of anticipation itself, uncontaminated by any personal reference. Such is the case of *Godot* (Beckett 1954), whose efficacy lies in no concrete, substantive achievement but in the pure fact that he is waited for.

Delay and the maintenance of status boundaries.—This radical case points up the two contradictory tendencies that are common to the stand-points of all servers: (1) the desire to enter at once into relations with others for "instrumental" reasons and (2) the impulse to hold oneself apart from them for "expressive" ones. This dilemma has a structural as well as a psychological referent. We know that the maintenance and purposes of social organizations require social contact not only among constituents of a single stratum but also between members of different (higher and lower) strata. This prerequisite poses a problem because interpersonal contacts between strata tend in diverse ways to undermine the distance and erode the barriers that distinguish them. Dedifferentiating tendencies of this sort could only redound to the disadvantage of the superior, who profits both materially and morally in proportion to the decisiveness of the separation. The delaying ritual of waiting helps resolve this dilemma. Although the status gap must be bridged by social contact, the contact itself can be depersonalized and formalized; it can be made "by appointment only." This practice follows status lines in a very clear-cut way. While the factory worker, for instance, may approach his peers or even his foreman without appointment, he cannot do so if he is to meet with an executive. The subordinate must be delayed before he is allowed to make a cross-stratal contact. Such inconvenience nicely preserves the sense in which the superior is symbolically inaccessible to those beneath him. While an interactional breach of status boundaries may occur, it can be ritualized in a way which makes it appear that it does not.

The Imposition of Waiting as an Aggressive Act

If the temporal aspect of relationships between those occupying different social positions may be stated in terms of who waits for whom, then we would expect to find a reversal of the waiting-delaying pattern when persons "switch" positions. Furthermore, this reversal may be accentuated through retaliation by the one who suffered under the initial arrangement. A former president furnishes us with an example:

Ken Hechler, who was director of research at the White House from 1948 to 1952, recalled the day Mr. Truman kept Winthrop Aldrich, president of the Chase Manhattan Bank, waiting outside the White House office for 30 minutes. Hechler quoted Mr. Truman as saying:

"When I was a United States senator and headed the war investigation committee, I had to go to New York to see this fella Aldrich. Even though I had an appointment he had me cool my heels for an hour and a half. So just relax. He's got a little while to go yet." [*Chicago Daily News*, December 27, 1972, p. 4]

Punitive sanctioning through the imposition of waiting is met in its most extreme forms when a person is not only kept waiting but is also kept ignorant as to how long he must wait, or even of what he is waiting for. One manifestation of the latter form is depicted by Solzhenitsyn (1968a):

Having met the man (or telephoned him, or even specially summoned him), he might say: "Please step into my office tomorrow morning at ten." "Can't I drop in now?" the individual would be sure to ask, since he would be eager to know what he was being summoned for and get it over with. "No, not now," Rusanov would gently, but strictly admonish. He would not say that he was busy at the moment or had to go to a conference. He would on no account offer a clear, simple reason, something that could reassure the man being summoned (for that was the crux of this device). He would pronounce the words "not now" in a tone allowing many interpretations—not all of them favorable. "About what?" the employee might ask, out of boldness or inexperience. "You'll find out tomorrow," Pavel Nikolaevich would answer in a velvet voice, bypassing the tactless question. But what a long time it is until tomorrow. [P. 222]

The underlying technique for the aggressive use of delay involves the withdrawal or withholding of one's presence with a view to forcing another into an interactionally precarious state wherein he might confront, recognize, and flounder in his own vulnerability or unworthiness.¹⁰ By such means, the superordinate not only affirms his ascendancy but does so at

¹⁰ Of course, the impulse of stationary servers to make others wait for reasons that are independent of the scarcity of time is paralleled by the tactic, used by mobile servers, of keeping them waiting for these same reasons. Thus, a person may simultaneously exhibit contempt for a gathering and underscore his own presence (Parkinson 1962, pp. 73-74) by purposely arriving late. This measure is particularly effective when the proceedings require his presence.

the direct expense of his inferior's dignity. Russian bureaucrats are masters at invoking this routine in their dealings with waiting clients:

Casting a disapproving eye at the janitor's wet overshoes, and looking at him severely, Shikin let him stand there while he sat down in an arm-chair and silently looked over various papers. From time to time, as if he was astonished by what he was reading . . . , he looked up at him in amazement, as one might look at a man-eating beast that has finally been caged. All this was done according to the system and was meant to have an annihilating effect on the prisoner's psyche. A half-hour passed in the locked office in inviolate silence. The lunch bell rang out clearly. Spiridon hoped to receive his letter from home, but Shikin did not even hear the bell; he rifled silently through thick files, he took something out of a box and put it in another box, he leafed, frowning, through various papers and again glanced up briefly in surprise at the dispirited, guilty Spiridon.

All the water from Spiridon's overshoes had dripped on the rubber runner, and they had dried when Shikin finally spoke: "All right, move closer!" [Solzhenitsyn 1968*b*, pp. 482-83]

This kind of strategy can only be employed by superordinates who have power over a client in the first place. The effect on the client is to further subordinate him; regardless of a server's initial attractiveness or a client's realization that the delay has been deliberately imposed. Furthermore, this practice leaves the client in a psychologically as well as a ritually unsatisfactory state. The two presumably act back on each other in a mutually subversive way, for by causing his client to become tense or nervous the server undermines the self-confidence necessary for him to maintain proper composure. This tendency, incidentally, is routinely applied by skillful police interrogators who deliberately ignore a suspect waiting to be questioned, assuming that a long, uncertain wait will "rattle him" sufficiently to disorganize the kinds of defenses he could use to protect himself (Arthur and Caputo 1959, p. 31).

Ritual Waiting and Autonomy

We have tried to show that while servers may cause others to wait in order to devote their attention to other necessary matters, they may also make people wait for the pure joy of dramatizing their capacity to do so. Such elation, we saw, is understandable, for by effecting a wait the server demonstrates that his presence is not subject to the disposition or whim of another and that access to him is a privilege not to be taken lightly. And, if access is a privilege, then one may sanction another by deliberately holding oneself apart from him. But we must now make explicit a point that was only implied in our previous discussions: that the imposition of waiting expresses and sustains the autonomy as well as the superiority of the self.

While the imposition of delay allows a superordinate to give expression

to his authority, waiting may also be imposed in protest against that authority. The latter achievement is valued, naturally, among those of despised status and low rank. Because they lack the wherewithal to do so in most of their other relations, the powerless, in their capacity as servers, delight in keeping their superiors waiting. The deliberately sluggish movements of many store clerks, telephone operators, cashiers, toll collectors, and the like, testify to the ability of the lowly as well as the lofty to dramatize their autonomy. This accords with Meerloo's (1966) assertion that "the strategy of delay is an ambivalent attack on those who command us" (p. 249). This kind of aggression is perhaps most pronounced under sociologically ambivalent conditions: as the legitimacy of the existing distribution of status honor ceases to be taken for granted, prescribed deference patterns give way to institutionalized rudeness, which may be expressed by appearing late for appointments with a superordinate as well as by dillydallying while he waits for his needs to be serviced.

It goes without saying that members of the dominant class are not above such invidious intention. Often having the means to do so, they merely execute it in a diametrically opposite manner: by compulsively refusing to wait. These are the people who are targets of advertising campaigns through which establishments of various sorts announce that their customers do not wait as long as those who shop in lower-priced competitor stores. General store chains (specializing in groceries) have run such ads in the recent past. That time may be a marketable commodity is also confirmed by the frequency with which we observe "No Waiting" signs in the front windows of barber shops. Similarly, those who object to waiting as a matter of principle find satisfaction in "instant-on" television sets, Polaroid photographs, etc. For many persons, the higher cost of using such services is offset by the personal sense of self-worth and autonomy thereby affirmed. By paying a higher price, the individual may back up his claim that he is "not the kind of person who will be kept waiting." It needs to be stressed that the inflated price he pays is instrumental to this act of self-affirmation: the value of his time and, therefore, his self, is enhanced precisely because another value is sacrificed on its behalf; the individual thus convinces himself, and perhaps others, that he easily pays a cash price for the opportunity to dispose of it as he wishes. He presumably has "better things to do with his time" than to expend it behind a queue of others (whose time and selves—because they are willing to wait—may not be as valuable as his own).

Ceremonial Waiting

Because unwillingness to wait embodies a rejection of both the auspices under which it is demanded and the inferior self that awaits the incumbent

of the waiting role it may be said that readiness to wait symbolizes a measure of deference toward the authority who imposes it. Those who are kept waiting beyond the appointed time by very high political or professional figures, for instance, may not exhibit indignancy or sullenness at being delayed; on the contrary, the client must exhibit gratitude that an audience is granted at all. Thus, the waiting period that is taken in stride by the client of an internationally applauded brain specialist would give rise to seething if inflicted by the neighborhood dentist.

This variation in waiter irritation is governed by a general rule: the more pronounced the honor of the server, the longer we are expected to willingly wait for him. One of the clearest instances of this rule is found in those colleges which have "more or less unofficially standardized periods that students are to await a tardy teacher, and in some instances the period is graded according to the teacher's rank" (Moore 1963, p. 53).

If readiness to wait with good grace conveys an individual's deference to a person more elevated than himself, we should not be surprised to find an inferior waiting for the very purpose of expressing deference. This form is perhaps most conspicuous in its collective expression. On a very cold day in 1963, for example, almost 250,000 people waited up to 10 hours outside the Capitol Rotunda, where President Kennedy lay in state. Such a massive collective deference gesture can be made intelligible by reference to a simple principle. Given the charisma of its object, an event may presumably be so awe inspiring as to render banal and irrelevant—even profane—whatever one might oneself do. One consequently measures up to the occasion by doing nothing at all. Moreover, because suspension of activity in deference to another entails forfeiture of alternative activities and associated rewards, deferential waiting comes sharply into view as a functional equivalent to sacrifice. When in addition the renunciatory deferential tribute is rendered in proximity to a sacred center, its personal meaning is naturally intensified and focused. As one member of the long queue leading to the Capitol Rotunda put it: "We were going to watch it on television in our room at the 'Y.' But the more we watched the more we felt we just had to be here ourselves. It's so awful we felt we had to do something—something" (*New York Times*, November 25, 1963, p. 5). To wait deferentially at a sacred center is thus to be "where the action is" or, more precisely, where the in-action is.

The respect pattern.—The above is simply an extraordinary expression of the mundane tendency for persons to subject themselves to a wait as a sign of deference for those with whom they have an engagement. Hall (1959) refers to this as "the respect pattern" (p. 18) which inclines persons to arrive a little early for meetings and rendezvous so as not to subject another or others to such inconvenience and abasement as has been herein described. Self-imposed waiting is governed by the same rule that regulates

the impatience of those on whom waiting is imposed by another: the higher the rank of that other, the more imperative an unambiguous demonstration of the respect pattern becomes. For example, White House etiquette (as enunciated by Emily Post [1965]) dictates that: "When you are invited to The White House, you must arrive several minutes, at least, before the hour specified. No more unforgiveable breach of etiquette can be made than not to be standing in the drawing room when the President makes his entry" (p. 48). One of the most radical modes of this kind of ceremonial waiting is found in the Ethiopian practice of "Studying the Gate."¹¹ This involves a procedure followed by those who desire an audience with the emperor, for whose sake callers arrive several hours before their appointment and wait patiently outside the door leading to his chamber. Thus situated, visitors exhibit their respect, subjects their devotion, to him.

Individuals may express deference not only by arriving early and waiting for the appearance of a distinguished person, they may also wait for the departure of that person before leaving themselves. For some occasions this possibility becomes an imperative. Thus, according to Fenwick (1948), "The two cardinal points of White House Etiquette are that no guest is late and that no guest leaves before the President and his wife have gone upstairs" (p. 469). This rule shows that just as waiting may ritually precede access to another it may also precede his departure. A most radical example of the latter is the phenomenon of the death vigil, wherein a group awaits news of the passing of a prominent member and disperses when it is received. This form stands as a functional parallel to waiting for an honored person to depart before leaving oneself.

When juxtaposed with our initial remarks, such considerations as these (as well as others introduced in this paper) admit of a typological possibility that deserves more singular attention than it has up to now received.

Instrumental waiting and ceremonial waiting.—It is possible to distribute empirical instances across a continuum limited at one end by purely instrumental waiting, necessitated by the server's interactional inaccessibility due to real demands on his time and energy. Between the poles of this continuum we find cases which present the difficulty of ascertaining to what extent the wait may, on the one hand, be occasioned by the server's objective scarcity and, on the other, by the demand for temporal tribute implied in his refusal to open himself up for interaction at the first available instant.¹² Perhaps most cases would fall into this middle category, for, as Shils (1970) suggests, "Deference actions are not . . . always massive actions of much duration.

¹¹ This practice was explained to me by Donald N. Levine.

¹² What has been said in reference to delay may also apply when clients are seen immediately. In this case, too, it is often difficult to tell whether a server wishes to ritually acknowledge a client's worth or whether that client is seen at once because there are no other demands on the server's time.

They occur moreover mainly at the margin of other types of action. . . . Between beginning and end, deference actions are performed in fusion with non-deferential actions" (p. 433). However, we have observed deferential waiting in far less attenuated, far purer (indeed, ceremonial) forms. These constitute the limit of the other end of the continuum to which we are referring.

As an extreme case, ceremonial waiting sets in relief the devaluating aspects of waiting in more ordinary contexts. Precisely because it exaggerates their degradational implications, ceremonial waiting permits us to analyze these less radical forms in terms of their ritual "distancing" or "boundary maintenance" functions, through which superordinates may dramatize and so confirm their position in the social structure. However, we must not forget that the superordinate may be challenged by the very same means through which he confirms himself. We have ourselves observed compulsive refusals to wait. In view of this, we must concede that ritualized status-elevational possibilities—or, at least, reaffirmational ones—exist on both sides of the server-waiter relationship.

SUMMARY

Delay is a relevant sociological datum because it is general throughout society, is a measure of access to goods and services, and indexes the efficiency of the organizations which distribute them. Above all, delay entails two kinds of very conspicuous costs. Having nothing to do with waiting as such but rather with the losses occasioned by it, value foregone through idleness is an extrinsic disadvantage. On the other hand, the degradational implications of being kept idle are intrinsic to waiting and can arise in no way other than through involuntary delay. The purpose of this paper was to explore the way these costs are distributed throughout the social structure and to identify the principles to which this allocation gives expression.

We have introduced the category of power, as exercised in server-client relationships, as the ultimate determinant of delay, the main assertion being that the distribution of waiting time coincides with the distribution of power. This proposition turns on the assumption that power is related to the scarcity of the goods and skills that an individual server possesses. Accordingly, the relationship between servers and clients in respect to waiting is an instance of an "organized dependency relationship" (Stinchcombe 1970): servers' holding power is contingent upon clients not being able to frequent less accessible and/or more expensive servers, while client autonomy requires their availability. Delay is therefore longest when the client is more dependent on the relationship than the server; it is minimized, however, when the server is the overcommitted member of an asymmetrical

relationship.¹³ Personal and structural factors thus stand as intersecting contingencies: resourceful persons wait less within both competitive and monopolistic markets, while delay will be more pronounced in the latter regardless of personal power.

If waiting is related to a person's position in a power network, then a server may confirm or enhance his status by deliberately making another wait for him. In a more general sense, this is to say that the management of availability itself, regardless of the purpose for which an individual makes himself available, carries with it distinct psychological implications. Because a person's access to others indexes his scarcity as a social object, that person's social worth may only be realized by demonstrated inaccessibility. Openness to social relations may therefore be restricted not only to regulate interactional demands but also to enhance the self that one brings to an interaction. Because it is independent of the objective scarcity of servers and their resources, this type of delay was subsumed under the category of "ritual waiting." This form finds expression in positive as well as negative respects: just as a server may deliberately limit access to himself, so a client may wait when it is circumstantially unnecessary in order to exhibit deference to a server. The initial relationship between waiting and power thus gives rise to processes which strengthen it. That is to say, secondary dramaturgical modes have come to subserve a fact that was originally grounded in an objective supply-demand structure.

The broader implication of this essay is that it finds in time itself a generalized resource whose distribution affects life chances with regard to the attainment of other, more specific kinds of rewards. This is true in a number of respects. Time, like money, is valuable because it is necessary for the achievement of productive purposes; ends cannot be reached unless an appropriate amount of it is "spent" or "invested" on their behalf. On the other hand, the power that a time surplus makes possible may be protected and/or expanded by depriving others of their time. By creating queues to reduce idle periods, for example, a server exploits clients by converting their time to his own use. A server does the same by "overcharging" in the sense of deliberately causing a particular client to wait longer than necessary.

The monetary analogies we have used are not without some justification. Just as money possesses no substantive value independent of its use as a means of exchange, time can only be of value if put to substantive use in an exchange relationship. Both time and money may be regarded as generalized means because of the infinity of possibilities for their utilization: both are possessed in finite quantities; both may be counted, saved, spent, lost, wasted, or invested. And, just as the budget (which, for Weber [1964],

¹³ The subsumption of the server-client relationship under the concept of differential commitment was suggested to me by Philip Blumstein.

is the highest form of economic rationality) "states systematically in what way the means which are expected to be used within the unit for an accounting period . . . can be covered by the anticipated income" (p. 187), so the time schedule—which may be the highest form of interactional rationality—states in an identical way how the time required for the performance of numerous activities can be covered by its anticipated availability. Accordingly, while the powerful can allocate monetary means to their own desired ends by controlling the budget, they also regulate the distribution of time—rewarding themselves, depriving others—through their control of the schedule. What is at stake in the first instance is the amount of resources to which different parts of a system are entitled; in the second, it is the priority of their entitlements. Far from being a coincidental by-product of power, then, control of time comes into view as one of its essential properties.

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The Coup d'Etat in Theory and Practice: Independent Black Africa in the 1960s¹

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Attempts have been made to explain coups d'état by reference to macro social and economic variables, structural features of the military in general, and, more commonly, of military elites, or by reference to the individual psychological qualities of the coup participants. Such theories are often held to be mutually exclusive and competing. This paper examines these theories and then tests them against data for 35 independent countries in sub-Sahara Africa, a region for which it has been widely argued that only "micro" explanations of coup activity are possible. Each country is first assigned a score on a coup activity scale which provides an interval measure of the dependent variable. Colonial background, contagion, and temporal hypotheses do not explain differences in coup activity. The data are then subjected to multiple-regression analysis using sets of socioeconomic and military variables. Although no single independent variable is conspicuously related to coups, multivariate analysis does explain some variance in coup activity. An explanation of coup activity that integrates the three levels usually employed by social scientists is suggested. This takes into account societal conditions conducive to coups, the relative dominance of military institutions, and, for residual variance, the personal characteristics of military officers and external influences on them.

By coup d'état I mean the forceful seizure of the machinery of state government, a common phenomenon of history and of the contemporary world. The high frequency of such political action, however, has inspired surprisingly little theoretical work on the coup as a generic social act, or on the social structures in which it is likely to occur. Historicism, it seems, has been the dominant mode of analysis.

Early modern theoreticians usually concentrated on the mechanism of the coup rather than on social conditions. Thus, in his analysis of Europe, Curzio Malaparte (1932), echoing themes of Mosca and Pareto, argued that all governments are subject to overthrow. Their displacement is dependent on the personal characteristics of ruling elites and the tactical acumen of the would-be usurper. Thus, for him, Italy in 1920 was ripe for a coup, but there was no individual who possessed the necessary skill to bring

¹ An earlier version of this paper was presented at the Southwestern Sociological Association Annual Meeting, San Antonio, Texas, March 30–April 1, 1972. James Leung lent valuable assistance in the analysis of data reported here.

one about. In contrast, the Bolshevik revolution, which he interpreted as a coup, was successful because of Trotsky's superior tactics (p. 14). In a more recent examination of the European experience, Goodspeed (1962) also begins with a stress on tactics, but his investigation includes a study of the immediate preconditions of coup activity. Of importance here, he claims, are "the sympathies of the nation's armed forces, the state of public opinion, and the international situation" (p. 210). Turning to less immediate factors, he notes that the successful coup is usually preceded by prior failure of government.

This theme of political degeneration is taken up by others who have investigated the preconditions for a sudden seizure of power. Thus Janowitz (1968) claims that "militarism in the new nations of Africa and Asia is often reactive or unanticipated because of the weakness of civilian institutions and the breakdown of parliamentary forms of government" (p. 28). In a similar vein Bienen (1968*b*) claims that "when civilian governments are discredited, the military may be able to intervene without force because it does retain legitimacy untainted by civilian failures" (p. 38). Janowitz (1968), Bienen (1968*b*), LeVine (1967), Andreski (1968), and others have specified some of the social and economic factors that lead to these civilian failures. The result is a series of rather loose theories of the coup, employing variables of various levels of abstraction and immediacy to the coup itself. The purpose of this paper is to test the efficacy of such theories to the empirical realities of Africa in the past decade.

THE AFRICAN CASE

Just a few years ago, it was fashionable to view Africa with optimism and to discount the possibility of military intervention in politics. Thus, Morris Janowitz (1964) states: "Most of the new nations have had independence for such a short time that the non-partisan role of the military reflects its limited and primitive resources and the newness of the country" (p. 13). In a 1959 paper, Coleman and Brice (1962) could claim that "the question of civilian supremacy has not yet become a serious issue in the new states of sub-Sahara Africa" (p. 398). Gutteridge (1965) similarly held that "sporadic outbreaks of minor military intervention in African politics are not to be taken . . . as indicating a universal proneness to the historical malaise of Latin America" (p. 144). But by the late 1960s events clearly led to the opposite conclusion.

The commonly held view now is that all of black-ruled sub-Sahara Africa is prone to military coups (Bell 1968, p. 272) and that coups are natural and endemic to the continent (Andreski 1968, pp. 208-9). Thus, Zolberg (1968) states that "military coups are likely to occur *anywhere* in the

region because of fundamental and lasting characteristics of political life" (p. 72; my emphasis).

The reason for this switch in imagery stems from the African experience of the 1960s. The decade was marked by widespread violence, including the continuous unrest in the Congo and Sudan, the civil war in Nigeria, and the border wars fought by the Somalis (Matthews 1970). During the 1960s there were 27 successful coups in 13 countries in sub-Saharan Africa. Nine other countries experienced abortive coups d'état, while only seven countries reported no coup activity during the decade. Of the most unstable countries, Dahomey experienced five coups and Congo (Brazzaville) three. Six other countries—Burundi, Congo (Kinshassa, now the Zaire Republic), Nigeria, Sierra Leone, Sudan, and Togo experienced two successful coups during this period. The coup, then, despite the mid-decade protestations of LeVine (1967, p. 55) concerning an alleged "culture of violence" in Africa, has indeed become a fact of life in the region.

The belief in Africa's proneness to coups d'état has led many commentators to overlook social factors in their explanations. Zolberg (1968), for example, relies on specific circumstantial and current factors. Without actually attempting an analysis, he claims that it is "impossible to specify variables which distinguish as a class countries where coups have occurred from others which have so far been spared" (p. 71). Janowitz (1964) concurs with this skepticism (p. 73). Similarly, a cursory investigation reveals to LeVine (1967) "that explanations, if there are any, must be sought within the particular situation of each troubled African State" (p. 53). Perhaps, though, social explanations are rejected too readily here. Certainly uncausal explanations can be rejected by a light perusal of the African data, but even simple multivariate relationships would demand mental gymnastics going far beyond the running of mental checklists. The task here, then, is to systematically investigate how theories of the coup apply to the African continent; that is, to determine if social and economic variables can discriminate between those countries that have had coups or high coup activity and those that have not. Three common univariate explanations of coup activity are first investigated. The remaining analysis employs variables relating to socioeconomic conditions and military structures. Since no single variable can explain differences within the sample of the 32 independent African countries, the analysis uses multiple-regression techniques to determine combined effects of these variables.

THREE COMMON EXPLANATIONS: COLONIAL BACKGROUND, CONTAGION, AND TEMPORAL THEORIES

The colonial background of African countries has often been argued to be a crucial factor in coup activity. This argument is usually based on the

different political systems that have been inherited by the African countries from France, Britain, or Belgium (see, for example, Coleman and Brice 1962, pp. 364 ff.). Thus, according to Rivkin (1969), "The British legacy (as opposed to the French) provides a more promising base for nation-building and development" (p. 223), even if the relationship between colonial background and this political base is acknowledged to be relatively weak. Countries having unitary one-party systems, according to Rivkin, are "so constructed that they are peculiarly vulnerable to military *coup d'état*, no matter how small the military forces in being may be" (p. 51). The colonial powers also imported different military traditions which may effect forceful intervention. Lee (1969), for example, notes that the French continued to pay all officers full salaries after the countries had been granted independence (pp. 25, 72-73). The French had employed national service while the British relied on voluntary forces. Taken together, these military and political traditions (if they are not offsetting) suggest there should be a difference between the prevalence of coups in former French and British colonies.

Examination of the evidence, however, provides no support of this thesis. By the end of 1969 five out of 14 former British colonies had had successful coups, compared with six of 15 former French colonies. Two of the ex-Belgian states fell to military coups, but with a sample size of only three, this can hardly be taken to indicate that Belgian territories are peculiarly vulnerable. When scores on a coup activity index (used in the multiple-regression analyses which follows) are dichotomized, eight of the 14 former British territories rate high as against nine of 15 of the French. Thus the difference between the French and British traditions does not appear to be directly related to military intervention.

Contagion theories posit that the occurrence of a coup in one country stimulates those in other countries, especially neighboring ones. This idea is implicit in the work of Bell (1968), who talks of two waves of military intervention in the political affairs of Africa. The first, he notes, began in December 1962 and lasted for 14 months. The second began in November 1965 and continued to February 1966. Elite linkages in French Africa and the awareness by military officers that they were indeed participating in a coup series during the first wave has been noted by Zolberg (1968, pp. 85-86.) He found similar linkages for coups in former British territories in this period. For the 1965-66 wave, he notes that officers in the Central African Republic, Upper Volta, and Dahomey had served together in Indochina. This does not necessarily imply an international conspiracy on the part of military officers. Thus LeVine (1967), writing about the same series of coups, sees "no direct evidence" of contagion (p. 55). In a later work Zolberg (1969) extends his contagion theory. He claims that "the phenomenon of contagion contributed to the normalization of military rule

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as well: its establishment in one country enhanced both the opportunities available to other groups and the legitimacy of stepping in to save the situation" (p. 179). But his case studies of Congo, Dahomey, Central African Republic, Nigeria, Upper Volta, and Ghana indicate that military rule is itself unstable.

The evidence in support of the contagion thesis cannot be denied. In addition to the "wave" phenomenon, there also appear to be territorial linkages (see fig. 1). Within sub-Sahara Africa there are two contiguous

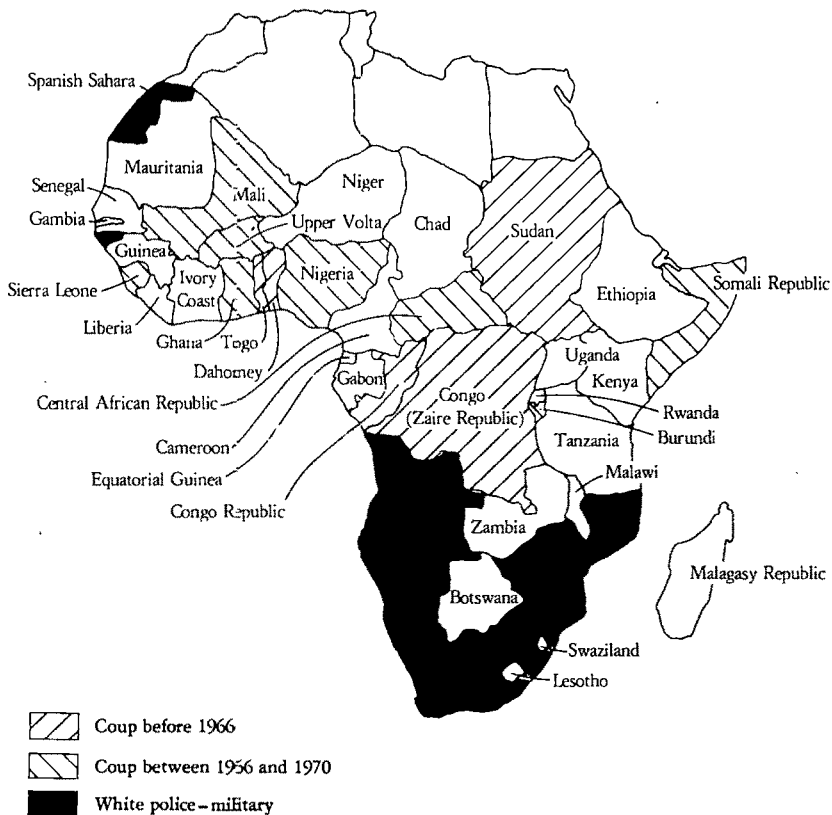


FIG. 1.—Black Africa: military coups and the domino theory

areas in which coups have occurred. The western block is composed of Togo, Dahomey, Ghana, Nigeria, Upper Volta, and Mali. A central block consisting of the Central African Republic, Sudan, Congo, Burundi, and Congo Brazzaville—recently joined by Uganda—is also discernable. Both blocs experienced coups during 1966 and in the earlier wave of military take-overs. Only the Somali Republic and Sierra Leone, which is culturally but

not territorially linked with former British West Africa, lie outside of these two regional blocs. The contagion thesis, however, is insufficient to explain all coup behavior. It cannot predict when and where an initial wave of coups will begin, nor which countries will follow the example of the first.

Temporal theories rest on the assumption that most newly independent countries are equally vulnerable to coups. Once independence is granted, it is only a matter of time before they have one. Janowitz (1964) has reported weak support for this thesis in a sample of new nations which excluded those dealt with here (p. 16). It is also suggested by LeVine (1967, p. 59), and by van Doorn, who claims that political structures in the underdeveloped new countries "erode" (a temporal concept) very rapidly. They readily collapse under military pressure because of their "failure to build up a political control structure" (van Doorn 1969, pp. 25-26). Similarly, Zolberg (1968) argues that there are overreaching similarities in African politics which make the countries liable to coups (pp. 74-79). Most civilian politicians, he notes, fail to fulfill their own goals or confront the problems posed by their typically heterogeneous populations, internal conflict, and fragile economies. He concludes that power is thereby devalued and force comes to the fore without institutional changes. Civilian rule, then, is only an interlude separating independence and military intervention. Its duration, according to Scott (1971), is about five years in Africa (p. 39).

The general temporal thesis seems to be supported by the experience of Africa, although the five-year claim is not very compelling. None of the five countries with less than six years of independence at the end of 1969 had experienced coups. Among those that were independent for between six and nine years, two of the eight had coups. The remaining 22 countries had all been independent for 10 or more years at the end of the decade, and 11 had experienced a military takeover. The latter group, however, indicates that perhaps not all African countries are equally subject to military coups. The thesis is therefore generally valid. But as a single explanatory variable, time is not very discriminating: additional variables are clearly needed if a more exacting explanation of coup activity is sought.

A MULTIVARIATE APPROACH TO COUP ACTIVITY

The literature on coups d'état yields two main classes of explanatory variables in addition to those dealt with above. The first set relates to general socioeconomic conditions, the second to military structures. Selected variables in both classes, together with one extrasocietal variable, are utilized in the analysis that follows. The rationales for each of the variables and their indicators are specified below.

Proneness to coups.—This is the dependent variable in the analysis. It is the propensity of a country to fall victim to coups d'état. This vulnerability,

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as Andreski has noted (1968, p. 209), seldom ends with the first military intervention: extensive—that is, revolutionary—changes seem necessary to forestall further coup activity. This “proneness” implies a continuous variable. Thus, countries that have experienced a coup are not necessarily equally prone to repeated activity. Similarly, countries with legally constituted governments are not equally immune to forceful intervention.

The indicator used for Africa was a coup rate index shown in table 1.

TABLE 1
COUP ACTIVITY IN INDEPENDENT BLACK AFRICA DURING THE 1960s

	OCCURRENCES OF COUP ACTIVITY			COUP ACTIVITY INDEX (10A+3B+C) (a)	YEARS INDEPENDENT (b)	RATE INDEX $\left(\frac{1+a}{b}\right)$
	A (Attempts)	B (Aborted)	C (Plots)			
Burundi	2	1	2	25	8	3.25
Cameroon	0	0	10	0.10
Central African Republic	1	...	2	12	10	1.30
Chad	0	10	0.10
Congo Brazzaville	3	3	2	41	10	4.20
Congo Kinshassa	2	4	5	37	10	3.80
Dahomey	5	50	10	5.10
Ethiopia	2	1	...	7	10	0.80
Gabon	1	3	10	0.40
Gambia	0	5	0.20
Ghana	1	1	1	14	10	1.50
Guinea	2	2	10	0.30
Ivory Coast	1	1	10	0.20
Kenya	1	...	3	7	0.57
Liberia	2	2	10	0.30
Malagasy Republic	0	10	0.10
Malawi	0	6	0.17
Mali	1	10	10	1.10
Mauritania	1	...	3	10	0.40
Niger	1	1	10	0.20
Nigeria	4*	...	2	42	10	4.30
Rwanda	0	8	0.13
Senegal	1	...	3	10	0.40
Sierra Leone	2	...	1	21	9	2.44
Somali Republic	1	1	...	13	10	1.40
Sudan	2	0	2	22	10	2.30
Tanzania	1	2	5	9	0.67
Togo	2	2	...	26	10	2.70
Uganda	1	3	6	8	0.88
Upper Volta	1	10	10	1.10
Zambia	2	2	6	0.50

NOTE.—Botswana, Equatorial Guinea, Lesotho, and Swaziland are excluded.

* Adjustment for war-induced changes in military government in the eastern region.

In addition to successful coups, this takes into account both abortive coups and publicly revealed plots to intervene. Information on such activity was systematically gathered from the *New York Times* and cross-checked with

other sources cited in this paper. A coup activity score was computed by assigning a value of 10 points to a successful coup, 3 to an overt attempt which failed, and 1 point to an uncovered plot. To yield the index, the total score for each country was divided by the number of years of independence. This adjusts the low scores of newly independent countries which could not exhibit coup activity under colonial occupation, and in so doing controls for the temporal effects already discussed.

Socioeconomic Variables

It is widely recognized that to forestall military coups some degree of economic and social progress is necessary. Thus Foltz (1966) has claimed that if this condition is not met, the state is likely to go through an extended period of coups, repression, stagnation, chaos, and revolt (p. 131). Similar sentiments have been expressed by Gutteridge (1965, p. 130). Socioeconomic factors are claimed to be especially conducive to coups if they limit political participation (Luttwak 1969, p. 24). In addition, various forms of economic crises (such as chronic unemployment and inflation) and social and political factionalism can constitute immediate preconditions for military intervention (Luttwak 1969, p. 17). In similar fashion Andreski (1968) argues that the political role of the military will be low if the society is fairly homogeneous, if it enjoys economic prosperity, and if there is no demographic pressure (pp. 126-27). LeVine (1967) adds a similar list of preventive conditions (p. 62). Nelkin (1967), perhaps, makes the strongest claim for the explanatory power of such variables when he states that "in every country the issues which best account for the ease of military access to power, relate to economic circumstances and their social consequences" (p. 131). The following are the specific variables of this type chosen for the multivariate analysis:

Population size.—This variable may relate to coups in several ways. Countries with small populations may be easier to govern successfully than large ones, but their scale may facilitate intervention. In Africa, ethnic diversity tends to be highest in heavily populated countries, and this may magnify the problems of rulership and increase the possibility of coups d'état. The indicator used for this variable was the estimated population for 1968 (U.N. 1968).

Population growth.—High growth rates put pressure on resources within the country and lead to disadvantageous dependency ratios. Under these conditions widespread economic well-being is extremely difficult to achieve, and political instability and coups may result. The indicator used for this variable was the percentage annual rate of increase of the population for the mid 1960s (U.N. 1967).

Urbanization.—It is often assumed that urbanizing societies are experi-

encing "modernization" and in the process building an immunity to coups. On the other hand, high urbanization in poor countries might indicate extremely unequal distribution of wealth and lead to instability. Luttwak (1969) has this in mind when he notes that "the urban poor living by experience, barely surviving in the day to day struggle for the necessities of life, are treated to the spectacle of cocktail parties, limousines, and grandiose villas of the ruling elite" (p. 20). Noting the same phenomenon, Nelkin (1967) argues that its end product is the erosion of support for the politicians in power (p. 131). Large inequalities, of course, are more obvious in highly urbanized societies. The indicator used was the percentage of the population in towns larger than 100,000 people (Hance 1970, table 16).

Centrality.—According to Luttwak (1969), states with a clear political center are the easiest to seize (p. 45). Measures of economic and transport concentration are included in the chosen indicator, a slight modification of an index developed by William A. Hance (1970, p. 234). Two points are scored if the capital city is the only major city in the country, and 1 if the capital is the most populous city, first port, or first river port; if it has the main airport, rail center, or road focus; and if it is the main manufacturing and administrative center.

Literacy.—It may be hypothesized that an unlettered population is a breeding ground for military intervention. A poorly informed public is unlikely to oppose a coup. Conversely, a highly literate public is more likely to express its discontent with civilian rule and give encouragement to military plotters. The percentage of the population claimed to be literate, reported in the Percy and Stoneman (1968) was the chosen indicator.

Mass-media availability.—One of the first actions of a successful coup d'état is the seizure of broadcasting facilities. The more receiver sets in the country the easier it is for the new regime to consolidate its victory by rapid dissemination of news. This only holds true if the broadcasting facilities are relatively concentrated and accessible to seizure. If they are not taken they may be a powerful source of opposition. The indicator used here is the number of radio sets per 100 population (U.N. 1967).

Economic level.—Economic well-being is usually claimed to be a factor in coup activity. Thus Luttwak (1969) has argued that "economic backwardness" is conducive to coups (pp. 18-25), while Miller (1970) adds that coups are "easier if the economy is small and fragile" (p. 16). By world standards the countries in this sample rate very low on most indices of wealth. In 1963 only Gabon, Ghana, the Ivory Coast, and Senegal had GNP per capita of more than \$200 U.S. In global perspective, then, all of the African countries are prime coup targets. The variable is included in the analysis to test its effect within a low wealth stratum. The measure used here was GNP per capita in 1963 (U.N. 1967, p. 582).

Economic growth.—A growing economy is an important sign of effective

government. If a high growth rate is maintained, public discontent and the possibility of violent intervention are likely to be low. The growth rate was calculated from GNP figures for 1963 and 1965 listed in the U.N. *Yearbook of National Accounts Statistics* (1969, vol. 2).

Military Variables

Since the military must take action to bring about a coup d'état, it is often argued that certain structural factors of the military may be conducive to coups. According to Miller (1970), the military may intervene if it feels threatened "as an institution" by civilian rulers (p. 16). Similarly Bienen (1968b) has argued that interest group demands may be a motivating factor, as they were in the East African mutinies in the sixties (p. 38). Gutteridge (1965) adds that "the small size of the forces in relation to population and area and the brevity of the officer professional tradition, will prove the ultimate determinants of the military role in African politics generally" (p. 108). The situation in the continent, however, is still a fluid one. Thus LeVine (1967) has remarked on the recent upgrading of African armies, their growing pride, and awareness of their own political potential (p. 54).

Several commentators on civil-military relations in less developed countries attributed coups to the modern characteristics of military organizations. Unlike civilian institutions in such countries, the military are usually national in scope. For these reasons, Gutteridge (1965) claims that they may be "the only group with the know-how to run an advanced society" (p. 10). This admiration for the technical competence of the military is widespread (e.g., Coleman and Brice 1962, p. 402; Scott 1971, p. 39), and such influential scholars as Edward Shils and Lucien Pye (see their articles in Johnson 1962, pp. 7-68, 69-89) are enamored of military modernity.

Andreski (1968) has argued along rather different lines (p. 35). He admits the advantages of the armed forces over civilians in technological sophistication and organization, but he also draws attention to their monopoly of modern weapons, which is crucial to intervention. Janowitz (1964) has likewise stressed the "significance of force" (p. 32). The variables chosen to represent these structural factors are outlined below.

Military participation ratio.—This has been defined by Andreski (1968) as "the proportion of militarily utilized individuals in the total population" (p. 33). He claims that it is directly related to the balance of power between civilians and the military, so it should be a factor in coup activity. The indicator used here was the percentage of men of military age on active service in the armed forces (Booth 1970, p. 27).

Military size.—Despite the protestations of commentators denying its importance (Scott 1971, p. 39), the absolute size of the military may be a

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crucial factor in its potential for effecting a military coup. There is evidence within the African sample which suggests that size is not a sufficient or necessary condition for coup activity. Thus, in the Togo coup of January 1963 the military consisted of only 250 men supported by about 300 veterans of the French colonial army who were seeking reinstatement (Grundy 1968, p. 31). Nonetheless, it seems plausible that there is a "critical mass" that may be conducive to the articulation of military interests. A large military relative to the country's territorial area (see Gutteridge 1965, p. 144) may contribute to favorable coup conditions. The total number of men serving in the regular armed forces (Booth 1970, p. 27) was employed as a measure of the variable.

Size of the police force.—Gutteridge (1965, p. 65) and Janowitz (1964, pp. 37–38) have suggested that the police may be a factor in coups d'état. A large police force under military command clearly enhances the strength of the military and its capacity for asserting political control. On the other hand, if the police force is under separate command, a large police force may in fact retard the possibility of military intervention (Scott 1971, p. 40; Andreski 1968, p. 209). The indicator was the number of men in the police and paramilitary (e.g., the Gendarmerie in former French Africa) services. The figures used, like those for the military, are estimates for 1970, the only recent year for which full African data were available (Booth 1970, p. 27). Although these figures may indicate manpower throughout the decade they may also reflect post-coup expansion in some states.

The defense budget.—Defense expenditure gives an estimate of the scale of military operations and influence in a country. The measure used was the estimated defense budget in thousands of U.S. dollars for 1968 (Grundy 1970, p. 26). These, together with the adjusted expenditures below, may reflect inflated budgets after a military coup has taken place. They may therefore indicate the gratification of military self-interests as much as causal factors in coup activity.

Defense expenditures and the economy.—This variable concerns the priority of military over civilian spending. Two indicators were employed: the percentage of the total government budget and the percentage of the estimated GNP in 1958 spent on defense (Booth 1970, p. 26).

External Factors

It is often argued that external effects can influence military coups. The general ideological position of the United States, for example, seems tolerant of military intervention. Thus according to Bienen (1968a), "Many American government officials, military leaders and academicians believe that the military in the developing areas is the best counterforce against both internal and external disruption . . . something of a guarantee against the

sort of chaos which would ultimately make development, hard as it is now, almost impossible" (p. xiii). More specifically, Grundy (1968) has pointed out that neo-imperialist views are common among radicals (p. 27). Thus it is often claimed that outside agitators encourage coup behavior. The intruders may be the CIA or Communists, depending on the proclivities of the commentator.

On the other hand, it has been argued by Lutwak (1969) that "the target state must be substantially independent and the influence of foreign powers in its internal political life must be relatively limited" (p. 32). Thus he points out that a military coup is less likely where there are powerful behind-the-throne interests; for example, foreign holdings in mines, plantations, manufacturing, and transportation. But this view, of course, assumes that these external interests would oppose a military coup. If they in fact support it, the situation is reversed and coups become more likely. Many African countries have such external ties, both with the former colonial powers and, increasingly, with the United States. Thus the Liberian economy is dominated by U.S. corporate investments, and Ethiopia houses U.S. bases and has received the bulk of U.S. military assistance aid to Africa (LeVine 1968, p. 13).

Such outside influences are of course extremely difficult to measure quantitatively across the sample of countries dealt with here. The chosen indicator relates only to United States aid (and hence perhaps influence) in the African countries. The data used were World Bank-IDA figures for the cumulative total of loans and credits to the African countries as of June 1968 in millions of dollars. The most favored recipients were the Union of South Africa (which is excluded from the sample used here), followed by Nigeria, Sudan, Kenya, and Ethiopia. Two of these countries have had coups, the other two have not.

CORRELATION ANALYSIS AND FINDINGS

The sample consisted of 31 black African countries. Four countries—Botswana, Equatorial Guinea, Lesotho, and Swaziland—were excluded because of incomplete data. All are anomalous for the region and might also have been excluded on the basis of their recent independence and continued domination by white-ruled countries. First, the analysis investigates zero-order correlations of the two sets of variables and coup activity; then it examines the multivariate relationship of the dependent and independent variables.

The covariation of socioeconomic variables and coup activity is presented in table 2. The socioeconomic variables, with the exception of mass-media availability and economic level, are not strongly interrelated. Literacy and

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TABLE 2

CORRELATION MATRIX OF SOCIOECONOMIC VARIABLES AND COUP ACTIVITY INDEX (9)

	2	3	4	5	6	7	8	9
1. Population	-.03	-.18	-.06	.11	-.14	-.24	-.12	-.06
2. Population growth18	.16	.07	-.29	-.26	-.07	-.06
3. Urbanization43	.02	.06	.38	.06	-.06
4. Centrality	-.20	.10	.17	.10	.03
5. Literacy	-.16	-.06	-.23	.22
6. Mass media64	.01	-.24
7. Economic level09	-.02
8. Economic growth01

mass media emerge as the strongest predictors of coup activity when there are no controls for other independent variables.

Table 3 shows the correlation of military variables and military interven-

TABLE 3

CORRELATION MATRIX OF MILITARY VARIABLES AND COUP ACTIVITY INDEX (8)

	2	3	4	5	6	7	8
1. Military participation ratio (MPR)22	.27	.36	.28	.39	.37	.25
2. Size of military63	.85	.43	.07	.76	.36
3. Size of police force76	.20	.08	.80	.24
4. Defense budget42	.20	.88	.37
5. Military/government expenses50	.27	.36
6. Defense expenditure, GNP12	.32
7. U.S. aid32

tion. United States aid was included as a variable here because, as the table indicates, it is strongly related to three of the military variables. It appears that large standing armies are being underwritten by U.S. aid; only the relation between defense budget and the size of the military (as would surely be expected) yields a higher correlation. Zero-order correlations between the dependent and independent variables are higher than those for the socioeconomic variables due to their closer interrelationship and, perhaps, immediacy to military intervention.

The multivariate relationships between coups and the above sets of variables were investigated in the stepwise regression analyses summarized in table 4. The first column shows the effects of socioeconomic variables alone, the second of military variables, and the third of all of the variables on coup activity. The β coefficients are interpreted as unstandardized partial correla-

TABLE 4

 β COEFFICIENTS AND MULTIPLE CORRELATION IN THREE ANALYSES OF COUP ACTIVITY

	Analysis I	Analysis II	Analysis III
Population	-0.01	...	0.07
Population growth	-0.15	...	0.21
Urbanization	-0.08	...	0.04
Centrality	0.06	...	0.21
Literacy	0.02	...	0.05
Mass media	-0.32	...	-0.52
Economic level	0.01	...	-0.52
Economic growth	*	...	-0.02
Military participation ratio (MPR)	0.16	2.06
Size of military	0.01	0.02
Size of police	-0.01	0.07
Defense budget	*	-0.05
Defense/government budget	0.03	0.08
Defense budget/GNP	0.26	0.23
U.S. aid	0.01	-0.02
Multiple correlation, <i>R</i>41	.48	.75

* Variable excluded for insufficient *F*-level.

tions and indicate the contribution of each variable to the total explained variance in the dependent variable.

The socioeconomic variables alone yielded a multiple *R* of .41 and accounted for a little more than 16% of coup variance. Mass media and population growth emerge as the strongest (negative) predictors of military intervention. The military variables in the second analysis explain a little more variation, about 23%, with MPR and defense budget/GNP the strongest predictors. Centrality and the predominant variables in the first two analyses emerge the strongest predictors of coups when all independent variables are included. Together they explain 56% of the variation in coup activity. Even for Africa, therefore, such variables should not be discounted out of hand.

CONCLUSIONS

On a simplistic level, a coup occurs because a group of officers decides to stage one. They may be reacting to a long- or short-term crisis, or perhaps none at all. They may have plotted extensively or, as Miller (1970) argues for Africa, they may fall into situations that are "totally unstructured, a series of happenstances when decisions by a man pushing his own interests at a certain point in time may be critical" (p. 17). Such explanations are appealing, but the analysis presented above indicates that more general variables also have a bearing on coup activity.

The following explanatory model of coup activity, based on the findings reported here and derived from the theoretical literature, is proposed.

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At the small-group level, characteristics of the officer corps may be important. Their morale and the belief in their own virtue, as Grundy (1968) notes, can "serve to sanction military takeovers" (p. 18). But the military elite does not exist in a vacuum. Their values and sentiments are shaped by military training. Overseas tours may breed discontent while domestic politicization has been claimed to deter intervention (Mazrui 1969). Since they have been found to relate to coup activity, the overall structure of the military and the socioeconomic setting in which it operates would also be expected to influence elite dispositions. Figure 2 suggests the relationship of these personal and societal variables.

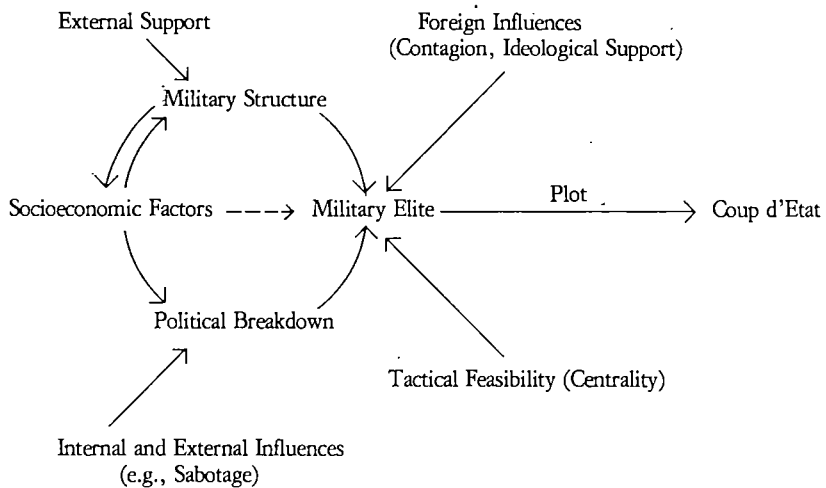


FIG. 2.—Explanatory model for coup activity

Whether or not a section of the military elite can isolate itself sufficiently to plot a coup depends in part on internal conditions (such as cohort rivalry) in the officer corps. But their motivation to intervene and the opposition that they can anticipate stems from less immediate factors. First, the plotters may be subject to foreign influences: contagion effects were noted for the African sample, and foreign moral and perhaps tactical support may encourage them. The elite are also influenced by the overall well-being of the defense establishment (which may, as in Africa, be closely associated with foreign aid), and the power it affords them. Their action may be predicated on the failure of civilian politicians, which is often the outcome of socioeconomic deterioration. Socioeconomic factors may also influence the military elite directly or through their impact on military structures (heavy demands of the latter might also contribute to socioeconomic malaise).

The inclusion of socioeconomic and military variables in the explanatory model is justified on theoretical and empirical grounds. Further use of the model, it is hoped, will show that it usefully portrays coups as complex, but not unexplainable, social phenomena.

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Racial Segregation in the Public Schools¹

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This paper presents data on racial segregation in public elementary schools in 60 cities for the 1967-68 school year. Wide variation was found among school districts in the fundamental demographic constraints confronting school systems seeking to desegregate. The percentage Negro among students varied from less than 5 to more than 90. Among instructional staffs the percentage Negro ranges from a low of 2 to a high of 84. Levels of racial segregation were typically high. The index ranged from a low of 39 in Sacramento to a high of 97 in Tulsa and Oklahoma City. The average level of school segregation among the 60 cities was 79. The task of desegregation for each city was estimated using an index that reflects both the degree of segregation and the racial composition of students. Cities in the South would have to permit an average of 32% of their students to shift schools compared with 26% in the North. Finally, the segregation of students of one race from teachers of another was determined. Teachers of one race are typically assigned to students of that race.

The Supreme Court, in *Brown v. Board of Education* (1954), ruled that racially segregated schools were "inherently unequal" and ordered desegregation "with all deliberate speed." The segregation of schoolchildren, it argued, "affects their hearts and minds in ways unlikely ever to be undone." Indisputably, racial segregation in the nation's schools persists 20 years later. The question of what to do about this continues to be one of the nation's most urgent and divisive domestic problems. The 1964 Civil Rights Act, authorizing cutoff of federal funds to school districts not in conformity with desegregation guidelines, placed a new administrative remedy in the hands of the Department of Health, Education, and Welfare. With this tool, desegregation plans were formulated and implemented in many small and some large school districts in the last years of the Johnson administration and the first years of the Nixon administration. Simultaneously, the cumula-

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tive process of case-by-case trial of many school desegregation suits led to a series of Supreme Court decisions affirming the original decision, striking down integration plans that did not effect desegregation, and gradually extending the scope of the decision.

Case studies have provided excellent information about school segregation in several cities (Crain 1968; Edwards and Wirt 1967; Stinchcombe, McDill, and Walker 1968; Rubin 1972; U.S. Commission on Civil Rights 1972; Moore 1973). The U.S. Commission on Civil Rights (1967, vol. 1, chap. 2) and the Southern Education Reporting Service (U.S. Bureau of the Census 1972, tables 180 and 181) have from time to time compiled data on larger numbers of school systems. Yet a paucity of data and a weakness of methodology have made it impossible to compare school segregation systematically in all cities. Shortage of information on a problem of such importance has fostered a variety of conflicting assertions and speculations. The year 1972 produced a veritable torrent of claims and counterclaims as candidates for national, state, and local office spoke out on school segregation and the associated emotion-laden topic of "busing." Better information could hardly have stemmed this flood of rhetoric, but some arguments were couched in terms that permit factual investigation. A syndicated newspaper column provides an example particularly relevant to our study: "There is no longer any objective difference between the school segregation in the North and South . . . the schools in the Northern cities, Boston, for example, are today as segregated as their Southern counterparts, Birmingham perhaps, and for the same reason" (Madison *Capital Times* 1971). Data appropriate for testing such assertions have recently become available as one result of the civil rights legislation of the mid-1960s.

Under Title VI of the 1964 Civil Rights Act, the Office for Civil Rights (Health, Education, and Welfare) was authorized to conduct periodic racial-ethnic surveys of public elementary and secondary schools. Results of the first such survey were published by the Office of Education, National Center for Educational Statistics (1969). The survey collected data on the racial composition of the students and staff of individual schools in all school districts with 3,000 or more pupils, in addition to selected smaller districts in some southern states. Similar data have subsequently been published for the 1968-69 and 1970-71 school years (U.S. Office for Civil Rights 1970, 1972).

We analyzed data for fall 1967 with the following aims: first, to describe the racial composition of the students and staff members in major school systems throughout the country; second, to measure the extent of racial segregation in the schools by applying techniques that have proven successful in the study of residential segregation; third, to assess the demographic magnitude of the task of effecting complete desegregation in a school district; and fourth, to determine the segregation of students of one race from

teachers of the other. As of fall 1967 few large school systems had been compelled by specific court order or federal administrative sanction to desegregate. These data, then, represent the best available baseline prior to the major desegregation efforts and controversies of recent years.

THE DATA

We consider 64 of the nation's largest cities, those with a population of 200,000 or more in 1970. Four cities—Albuquerque, Dallas, Honolulu, and Phoenix—are omitted because they did not submit data suitable for analysis. Of the 60 cities, 37 are in the North and 23 are in the South.

The data refer to public school districts rather than "cities." School district boundaries are not always coterminous with city boundaries. In some cases, the school district covers an entire county rather than conforming to municipal boundaries. This is true of all Florida districts; these and other county districts are noted in our tables.

Our analysis was conducted separately for elementary and secondary schools, but in this paper we emphasize findings concerning elementary schools. Any school in which the grade span exclusively or chiefly included grades 1–6 was considered an elementary school, while all other schools were classed as secondary. No attempt was made to separate junior from senior high schools or to isolate those schools which offered preschool classes. While the most common pattern of grade spans is K–6 in the elementary span, 7–9 in junior high, and 10–12 in senior high, many school systems have unique or varying patterns of grade spans. However, we believe this is not a significant factor in the present analysis.

For each public school in every school district, the survey tabulated the numbers of whites, Negroes, and others among students and instructional staff. The questionnaire stated that the "other" group "should include any racial or national-origin group for which separate schools have been maintained in the past, and any racial or national-origin groups which are recognized as a significant 'minority-group' in the community (such as Indian-American, Oriental, Eskimo, Mexican-American, Puerto Rican, Latin, Cuban, etc.)" (U.S. National Center for Educational Statistics 1969, p. 839). Responses were somewhat unpredictable: certain cities, such as Newark, reported large numbers of "others," while some cities, such as Baltimore and Houston, reported no "others." As a result, the data on "other" groups are of limited value, and the analysis is restricted to a comparison between Negroes and non-Negroes.

The instructional staff includes, in addition to classroom teachers and principals, supervisors of instruction, librarians, psychologists, and guidance personnel. It does not include noninstructional personnel such as nurses and food service, transportation, and custodial workers. In most

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cases, the data refer to full-time staff members plus the full-time equivalent of part-time staff members.

Certain listed schools were eliminated from the analysis: (1) schools with fewer than 100 students (they tend to be specialized schools); (2) schools for the mentally or physically handicapped; (3) schools for adults, such as night high school (but trade and technical high schools were retained in the analysis); and (4) schools for which the data were incomplete owing either to misreporting or typographical errors (mainly a problem in Houston and New York City). The survey omitted parochial and other private schools, although they enroll a large number of pupils in several cities.

The National Center for Educational Statistics acknowledged difficulties in collecting and processing the data. These data should be interpreted with some caution for they were published without extensive checks to eliminate inconsistencies or omissions. It is not certain that all schools were listed for each school district; nor that consistent procedures were used for determining the racial background of students and staff.

CHARACTERISTICS OF PUBLIC ELEMENTARY SCHOOL SYSTEMS

There is a considerable diversity in the size and racial composition of the 60 elementary school districts (table 1). Districts range from fewer than

TABLE 1
CHARACTERISTICS OF ELEMENTARY PUBLIC SCHOOL SYSTEMS IN SELECTED CITIES:
FALL 1967

SCHOOL DISTRICT	NUMBER OF					% NEGRO			AVERAGE SIZE OF SCHOOL
	Schools (1)	Students*		Staff*		City Pop.† (6)	Stu- dents (7)	Staff (8)	
		Total (2)	Negro (3)	Total (4)	Negro (5)				
South:									
Atlanta	123	75.0	44.8	3.1	1.8	51	60	56	610
Austin	48	27.3	4.6	1.3	0.2	12	17	18	570
Baltimore	149	123.6	80.0	4.5	2.6	46	65	58	830
Birmingham	85	48.8	25.3	1.6	0.8	42	52	50	570
Charlotte†	77	43.9	13.0	1.9	0.5	24	30	28	570
Corpus Christi	41	24.3	1.4	1.0	<0.1	5	6	4	590
El Paso	50	41.2	1.3	1.9	0.1	3	3	3	820
Fort Worth	88	47.5	12.6	1.9	0.4	20	27	23	540
Houston	159	144.2	48.2	5.3	1.8	26	33	33	910
Jacksonville†	99	68.0	19.1	2.7	0.8	22	28	29	690
Louisville	48	33.9	15.9	1.3	0.5	24	47	35	710
Memphis	91	72.9	37.0	2.8	1.2	39	51	43	800
Miami†	155	118.8	32.8	5.1	1.2	15	28	23	770
Nashville†	101	54.3	14.3	2.2	0.5	20	26	23	540
New Orleans	93	67.7	47.3	2.6	1.4	45	70	55	730
Norfolk	53	31.6	12.2	1.3	0.5	28	39	37	600
Oklahoma City ...	88	43.4	10.1	1.6	0.3	14	23	19	490
Richmond	39	29.3	20.2	1.3	0.8	42	69	63	750

TABLE 1 (Continued)

SCHOOL DISTRICT	NUMBER OF					% NEGRO			AVERAGE SIZE OF SCHOOL (9)
	Schools (1) *	Students*		Staff*		City Pop.† (6)	Stu- dents (7)	Staff (8)	
		Total (2)	Negro (3)	Total (4)	Negro (5)				
Saint Petersburg‡ ..	73	39.4	6.6	1.6	0.2	8	17	14	540
San Antonio	75	43.1	6.7	1.7	0.2	8	16	14	580
Tampa	85	51.9	11.6	2.0	0.3	14	22	16	610
Tulsa	74	45.1	5.8	1.7	0.2	11	13	12	610
Washington, D.C. ..	139	95.0	88.3	3.7	3.1	71	93	84	680
North:									
Akron	51	35.7	9.6	1.4	0.1	18	27	10	700
Boston	157	57.9	18.0	2.7	0.2	16	31	7	370
Buffalo	73	45.8	16.8	2.3	0.2	20	37	11	630
Chicago	416	407.1	214.5	14.7	5.0	33	53	34	980
Cincinnati	76	55.1	23.2	2.0	0.5	28	42	27	730
Cleveland	138	90.1	51.6	3.4	1.6	38	57	46	650
Columbus	121	67.4	17.9	2.5	0.4	19	27	15	560
Dayton	53	40.6	15.4	1.8	0.5	31	38	31	770
Denver	89	54.1	8.3	2.3	0.2	9	15	9	610
Des Moines	59	26.9	2.2	1.0	<0.1	6	8	3	460
Detroit	212	185.6	107.5	6.8	2.5	44	58	37	880
Indianapolis	105	78.8	25.6	2.7	0.7	18	33	27	750
Jersey City	31	27.9	12.7	1.1	0.2	21	46	16	900
Kansas City	80	46.7	21.8	1.8	0.6	22	47	32	580
Long Beach	55	39.6	3.6	1.5	0.1	5	9	4	720
Los Angeles	434	372.4	89.6	13.5	2.3	18	24	17	860
Milwaukee	123	78.5	21.8	2.4	0.4	15	28	16	640
Minneapolis	73	42.1	3.2	1.7	0.1	4	8	4	580
Newark	51	55.4	40.3	2.5	0.8	54	73	31	1,090
New York City ...	595	584.4	187.5	29.7	2.7	21	32	9	980
Oakland	64	37.8	20.7	1.6	0.3	35	55	20	590
Omaha	67	34.2	6.9	1.1	0.1	10	20	9	510
Philadelphia	209	173.3	102.9	7.3	2.5	34	59	35	830
Pittsburgh	87	48.7	19.6	1.9	0.2	20	40	12	560
Portland, Ore.	93	53.7	4.8	2.2	0.1	6	9	3	580
Rochester	42	28.0	8.6	1.4	0.1	17	31	9	670
Sacramento	56	29.5	4.2	1.1	0.1	11	14	6	530
Saint Louis	150	91.2	59.7	3.2	1.9	41	66	59	610
Saint Paul	60	26.4	1.6	1.0	<0.1	4	6	2	440
San Diego	116	74.6	9.5	2.8	0.1	8	13	4	640
San Francisco	93	51.7	15.1	2.0	0.2	13	29	8	560
San Jose	34	20.3	0.3	0.7	<0.1	3	2	2	600
Seattle	86	51.3	6.0	2.1	0.1	7	12	5	600
Toledo	56	40.1	10.7	1.5	0.3	14	27	20	720
Tucson	53	27.5	1.7	1.2	<0.1	4	6	2	520
Wichita	91	40.1	5.6	1.9	0.1	10	14	8	440
Yonkers	29	18.0	2.3	0.7	<0.1	6	13	6	620
Regional averages:									
South	88	59.6	24.3	2.4	0.8	26	36	32	660
North	118	87.5	31.7	3.6	0.7	18	30	16	660

SOURCE.—U.S. National Center for Educational Statistics 1969; U.S. Bureau of the Census, *Census of Population: 1970*, PC(1)-B, table 24.

* In thousands.

† Figures for the city population refer to 1970.

‡ Data for these places refer to the county rather than the central city.

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40 schools in Yonkers, Jersey City, San Jose, and Richmond, Virginia, to over 400 schools in New York, Chicago, and Los Angeles. The percentage black among students varies from less than 5 in El Paso and San Jose to more than 90 in Washington, D.C. Among teachers, the percentage black ranges from a low of 2 in Saint Paul, San Jose, and Tucson, to a high of 84 in Washington. Districts range from only a few hundred to over 100,000 Negro students and from fewer than 100 Negro staff members to several thousand.

The percentage Negro among public elementary school students substantially exceeds the percentage Negro in the total city population (cols. 6 and 7, table 1). This well-known phenomenon may be caused by higher Negro fertility and migration resulting in a younger age distribution among Negroes, as well as by greater white utilization of private and suburban schools.

Elementary schools are smallest in Boston, with fewer than 400 students each, and are largest in Newark, Chicago, and New York, with an average of about 1,000 students per school (col. 9, table 1). There is relatively little variation in the ratio of students to staff; the ratio is about 25 to 1 in most districts (data not shown).

Are there "regional" differences in these demographic characteristics of school systems, or are school systems throughout the country pretty much the same? Regional averages (unweighted) for the various characteristics are given at the bottom of table 1. Although regional differences in percentage Negro were formerly very great, by 1967 there was a surprisingly small difference between North and South. The average percentage Negro among public elementary school students was 30 and 36, respectively. If "the maximum problems of integration occur when the races are in the ratio of around 50:50" (Stinchcombe et al. 1968, p. 228), then a sizable number of school districts in each region face the school desegregation issue in its more unmanageable demographic dimensions.

The data on Negro staff reveal remnants of the major historical differences between the North and South. In the southern cities, the percentage black among instructional staffs is similar to the percentage black among students, 32 and 36, both exceeding the average 26% of population Negro. In the North the percentage black among staffs (16) is much lower than among students (30) and is also lower than the population percentage Negro (18). A later section will assess the implications of this difference for student-staff racial similarity.

SEGREGATION IN PUBLIC ELEMENTARY SCHOOLS

A number of techniques can be used to assess the extent of school segregation in the 60 cities. The U.S. Commission of Civil Rights in its landmark

1965 study, *Racial Isolation in the Public Schools* (1967, vol. 1), and in its more recent press releases dealing with changes in levels of school segregation, has relied almost exclusively on the "percentage of Negroes attending schools over 50 percent Negro" (or, occasionally, "over 90 percent Negro"). A serious shortcoming of this measure is that it is not independent of the citywide percentage Negro, so that a heavily Negro school system will, other things being equal, appear to be more segregated than a school system with relatively few Negroes. For example, a city with a small Negro population could place its Negro students as minorities in a few racially mixed schools and place most white students in all-white schools. Such a school system would appear to be unsegregated by such a measure. Yet if Washington, D.C., with a student population that was 93% Negro (1967), attained perfect racial balance in its schools, it would be completely segregated by this measure.

For this reason, we have chosen a measure, the index of dissimilarity, which takes as its standard of comparison the citywide student percentage Negro and proceeds to measure the extent to which the schools in a district deviate from this percentage.

In each school district we assembled the following matrix:

$$\begin{bmatrix} t_i & w_i & n_i \\ \dots & \dots & \dots \\ \dots & \dots & \dots \\ T & W & N \end{bmatrix},$$

where t_i , w_i , and n_i represent, respectively, the number of total, non-Negro, and Negro students in the i th school. By definition $t_i = w_i + n_i$. The entire school district contains T total students, W non-Negro students, and N Negro students. For the school district, the index of dissimilarity is computed

$$1/2 \sum_i |w_i/W - n_i/N|.$$

The value of this index indicates the minimum proportion of Negroes (or non-Negroes) who would have to change their schools in order to obtain equal proportions of Negroes and non-Negroes in each school. Complete integration prevails (0 on this index) if every school in the district has the same racial composition as the entire district. Complete segregation (100 on the index) occurs when each school is exclusively Negro or non-Negro (Taeuber and Taeuber 1965, appendix A; Duncan and Duncan 1955).

School segregation indexes for elementary students are presented in column 2 of table 2. Levels of racial segregation among elementary school students were universally high in large U.S. cities in fall 1967. The range is from a low of 39 in Sacramento to a high of 97 in Tulsa and Oklahoma

TABLE 2

SEGREGATION INDEXES AND REPLACEMENT INDEXES FOR ELEMENTARY
PUBLIC SCHOOL SYSTEMS IN SELECTED CITIES: FALL 1967

SCHOOL DISTRICT	SEGREGATION INDEXES			REPLACEMENT INDEXES		NUMBER OF STUDENTS TO BE SHIFTED*	
	Residential†	Students	Staff	Students	Staff	Both Races†	Minority Race‡
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
South:							
Atlanta	94	95	94	46	46	34.3	28.7
Austin	93	86	71	24	21	6.6	4.0
Baltimore	90	87	70	40	34	49.1	37.9
Birmingham	93	94	99	47	49	23.0	22.2
Charlotte#	94	77	79	32	32	14.0	10.0
Corpus Christi	89	77	66	8	4	2.0	1.1
El Paso	81	51	68	3	4	1.3	0.7
Fort Worth	94	93	92	36	33	17.2	11.7
Houston	94	92	88	41	39	59.3	44.5
Jacksonville#	97	92	96	37	39	25.3	17.6
Louisville	89	76	69	38	32	12.8	12.1
Memphis	92	95	92	48	45	34.7	34.2
Miami#	98	92	75	37	27	43.7	30.2
Nashville#	92	85	84	33	29	17.9	12.1
New Orleans	86	87	97	36	48	24.6	17.6
Norfolk	95	90	77	43	36	13.6	11.0
Oklahoma City	87	97	86	35	26	15.1	9.8
Richmond	95	95	89	41	41	12.0	8.7
Saint Petersburg# ..	97	91	92	25	23	10.0	6.0
San Antonio	90	88	77	23	18	10.0	5.9
Tampa#	95	88	91	31	25	15.9	10.2
Tulsa	86	97	83	22	17	9.8	5.6
Washington, D.C. ..	80	77	55	10	15	9.6	5.2
North:							
Akron	88	70	40	28	7	9.9	6.8
Boston	84	74	57	32	7	18.2	13.2
Buffalo	87	80	42	37	8	17.1	13.5
Chicago	93	92	72	46	32	187.3	177.6
Cincinnati	89	77	51	38	20	20.7	17.9
Cleveland	91	90	64	44	32	39.6	34.6
Columbus	85	81	58	32	15	21.4	14.6
Dayton	91	90	78	42	34	17.2	13.8
Denver	86	82	52	21	9	11.5	6.8
Des Moines	88	76	63	11	3	3.1	1.7
Detroit	85	79	43	39	20	71.6	61.9
Indianapolis	92	85	82	37	32	29.4	21.8
Jersey City	78	57	37	28	10	7.9	7.2
Kansas City	91	79	68	39	29	18.4	17.2
Long Beach	84	78	54	13	4	5.1	2.8
Los Angeles	82	89	66	33	19	121.8	80.1
Milwaukee	88	88	68	35	18	27.6	19.1
Minneapolis	79	74	53	10	4	4.4	2.4
Newark	72	68	32	27	14	15.0	10.3
New York City ...	79	52	48	23	8	133.2	98.1
Oakland	73	64	39	32	13	12.0	10.9
Omaha	92	88	79	28	13	9.6	6.0
Philadelphia	87	76	42	37	19	63.8	53.6
Pittsburgh	85	72	55	35	12	16.8	14.1
Portland, Ore.	77	74	68	12	4	6.4	3.5

TABLE 2 (Continued)

SCHOOL DISTRICT	SEGREGATION INDEXES			REPLACEMENT INDEXES		NUMBER OF STUDENTS TO BE SHIFTED*	
	Residential†	Students	Staff	Students	Staff	Both Races‡	Minority Races§
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rochester	82	61	44	26	7	7.2	5.2
Sacramento	64	39	41	10	4	2.8	1.6
Saint Louis	91	91	83	41	40	37.5	28.7
Saint Paul	87	62	68	7	3	1.9	1.0
San Diego	81	78	58	17	5	12.9	7.4
San Francisco	69	67	40	28	6	14.4	10.2
San Jose	60	49	75	1	2	0.3	0.1
Seattle	80	65	52	13	5	6.9	3.9
Toledo	92	80	71	31	22	12.6	8.6
Tucson	81	68	77	8	3	2.2	1.2
Wichita	92	86	70	21	10	8.3	4.8
Yonkers	78	60	38	13	4	2.4	1.4
Regional averages:							
South	91	87	82	32	30	20.1	15.1
North	83	74	58	26	13	27.0	21.2

SOURCES.—Taeuber and Taeuber 1965, table 1; U.S. National Center for Educational Statistics 1969.

* In thousands.

† Residential segregation indexes refer to 1960.

‡ Obtained by multiplying replacement index (col. 4) by total number of students (col. 2 of table 1).

§ Obtained by multiplying student segregation index (col. 2) by number of Negro students (col. 3 of table 1). In cities with a minority of white students, the number of white students was used (see ||).

|| In these cities Negroes are the majority group in elementary schools. The figures indicate the minimum number of white students who would have to be shifted.

School data for these places refer to entire county. Residential segregation indexes, in all cases, refer to central cities.

City. Two of the 60 cities have scores below 50; 42 have scores of 75 or above. The average level of school segregation among these 60 cities was 79. A simple interpretation of a score of 79 is that 79% of the Negro students would have to be moved to other schools to achieve complete integration.

School segregation in fall 1967 was greater in southern than in northern cities (87 vs. 74). Only four of 37 northern cities had indexes of 90 or above, while this was true of 12 of 23 southern cities. There is no justification for the assertion that school segregation in the North was as extensive as that in the South in 1967–68, although there may have been some changes since.

Levels of school segregation are quite similar in magnitude to levels of residential segregation. One recent study presented residential segregation indexes, utilizing census tracts, for 13 cities which took special censuses in the mid-1960s² (Farley and Taeuber 1968, p. 953). Census tracts, like

² Generally, the smaller the size of the areal unit used in computing the index of dissimilarity, the greater the value of the index. Thus, indexes computed using city block

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elementary school attendance districts, typically contain several thousand total population. Residential segregation scores for these cities averaged 75, while school segregation scores averaged 74.

Another study provides residential segregation indexes for all 60 cities analyzed in this paper, utilizing city block data for 1960 (col. 1 of table 2). The school indexes average somewhat lower than the 1960 residential segregation indexes (79 vs. 86), a difference which might well be due to the smaller unit of analysis (city blocks) used for the residential indexes. To what extent do school segregation patterns in these cities reflect residential segregation patterns? We can provide an answer only by overlooking the considerable slippage between the two sets of data—the possible lack of correspondence in school system and city boundaries, the different dates of the data, the reliance on blocks rather than school areas, etc.

The relationship between 1960 residential segregation scores and 1967 school segregation scores is portrayed in figure 1. The correlation is .80 for the 60 cities.

This relationship deserves attention in future work in this area. If school desegregation proceeds apart from any changes in residential patterns, as the law would seem to require, then we might anticipate a sharp weakening of this correlation.

Although residential patterns affect the racial composition of students who attend a school, this is not so likely to be the case with teachers. Any segregation of staff members is caused largely by a school district's teacher assignment policy. Are Negro teachers segregated from non-Negro teachers, or do they tend to be distributed randomly among schools? Teacher segregation scores are presented in column 3 of table 2. Segregation of Negro from non-Negro staff members is, on the average, less pronounced than segregation of Negro students from non-Negro students in these 60 school systems (67 vs. 79). A strong regional difference is apparent. Within the South, racial segregation indexes for staff members are nearly as large as those for students (82 vs. 87). Outside the South, school instructional staffs are more integrated than students (58 vs. 74). This still means that in most northern cities well over one-half the Negro (or white) staff members would have to change their teaching assignments to achieve complete staff integration, as compared with more than three-fourths in the South. The higher levels of teacher segregation observed in the South undoubtedly reflect the persistence of patterns established under the "dual" school system. Governmental and judicial desegregation efforts since 1967 may well have led to reductions in teacher segregation scores in many of these cities.

data exceed those computed using census tract figures. In the present case, census tracts seem to be the preferred unit, since elementary school enrollment areas and census tracts appear to encompass roughly equal-sized areas in many cities.

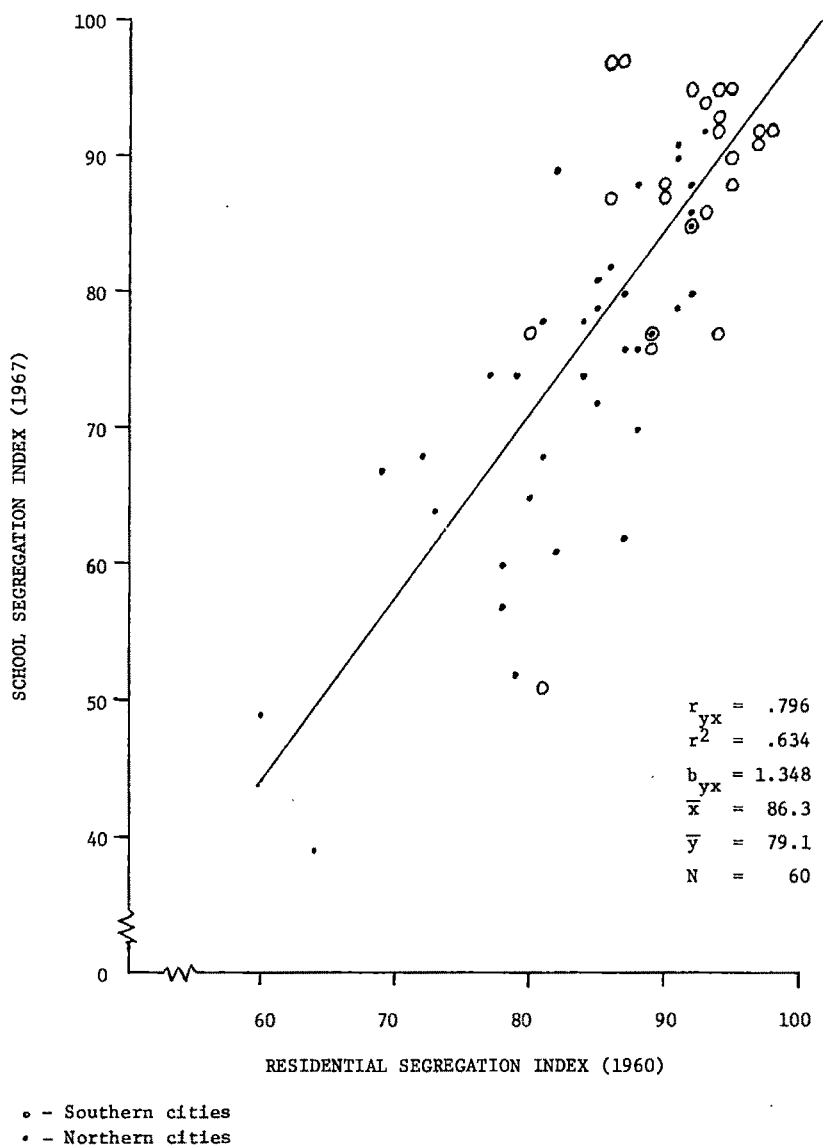


FIG. 1.—School and residential segregation indexes for 60 cities

THE TASK OF SCHOOL DESEGREGATION

Recent federal court decisions have required many school districts to adopt desegregation plans which call for the shifting of students from one school to another. The index of dissimilarity offers one measure of the task which a school system faces if it is to become racially integrated—it indicates the percentage of Negro or white students who would have to be shifted to

bring about integration. While moving students of any one race (usually Negroes) may seem inefficient and discriminatory, it appears that desegregation in some areas has been approached in this way—closing Negro schools, moving Negro students to previously white schools (but not vice versa), and releasing Negro teachers (*New York Times*, March 18, 1971, p. 1; April 3, 1971, p. 34).

A more efficient model for pupil desegregation is an exchange of Negro and non-Negro students while maintaining the number and size of existing schools. To measure the task confronting a school system embarking upon desegregation in this way, we use the replacement index. This index specifies the minimum percentage of the total student population that has to be shifted to achieve integration, while preserving each existing school at its current pupil size.

To derive the simple formula for the replacement index, consider a slightly different way of looking at the data presented above in the basic matrix of pupils by race by school. The t_i column represents the actual distribution of pupils among schools, ignoring race. In a desegregated situation, both w_i and n_i would have the same distribution among schools as t_i . An index of dissimilarity can be calculated to show the discrepancy between the distributions of w_i and t_i ; identify this index as D_{wt} . The formula is exactly analogous to that presented above for the index of dissimilarity; that original index can be specified as D_{wn} :

$$D_{wt} = \frac{1}{2} \sum |w_i/W - t_i/T|.$$

This measure tells us the minimum percentage of non-Negro pupils that must be redistributed in order to have non-Negro pupils distributed among schools in the same manner as all pupils. Substitution of $n_i + w_i/N + W$ for t_i/T in the above formula shows, after a few algebraic steps, that $D_{wt} = (N/T) D_{wn}$. To ascertain what proportion of all pupils is involved in this redistribution of non-Negro pupils, multiply by the proportion that white pupils constitute of the total. Hence redistribution of $(W/T) (N/T) D_{wn}$ of the school system's pupils is necessary to attain a proper proportional distribution of white pupils among schools.

A similar algebraic argument shows that $D_{nt} = (W/T) D_{wn}$ gives the proportion of Negro pupils that must be redistributed in order to have Negro pupils distributed among schools in the same manner as all pupils. To ascertain what proportion of all pupils are involved in this redistribution of Negro pupils, multiply by N/T . Again we get $(W/T) (N/T) D_{wn}$. The number of Negro pupils to be moved from schools in which Negro pupils are overrepresented is the same as the number of white pupils to be moved from schools in which white pupils are overrepresented. This exchange of white and Negro pupils accomplishes desegregation in the most efficient manner while still preserving the pupil size of each school. The proportion

of the system's pupils that are minimally involved in such a transfer shall be designated the replacement index, R : $R = 2 (W/T) (N/T) D$. The range of R , expressed as a percentage of all pupils, is 0–50. The value of the index is a function both of the degree of segregation in a school system and its racial composition. The index attains its maximum value, 50, in a school district which is 50% Negro and completely segregated ($D = 100$). In such a school district, 50% of all students would have to be exchanged among schools to achieve complete integration.

Replacement indexes for students in the 60 school systems in fall 1967 are presented in column 4 of table 2. The magnitude of the desegregation task approaches its maximum in southern cities such as Atlanta, Birmingham, and Memphis, and in northern cities such as Chicago and Cleveland. These cities combine high levels of school segregation with a student body nearly 50% Negro. At the other extreme are cities that combine a lower level of segregation with a small percentage of Negro students. To desegregate elementary schools in El Paso, Corpus Christi, San Jose, Tucson, Sacramento, and Saint Paul, fewer than 10% of their total students would have to be shifted among schools. On the average, the 23 southern cities face a more difficult situation in desegregating their schools than do the 37 northern cities—the average replacement index is 32 for the South compared with 26 for the North. The South has both higher average levels of segregation and a racial composition slightly less favorable from the viewpoint of integrating schools (i.e., closer to 50:50).

Highly segregated school systems can be integrated with relatively little shifting of pupils if the system has a favorable racial composition, that is, a predominantly Negro system (Washington, D.C.) or a predominantly non-Negro system (Des Moines and Saint Paul). Under complete integration, of course, the schools in Des Moines and Saint Paul would have overwhelmingly white student enrollments, while those in Washington would be predominantly Negro.

Under a policy of exchanging Negro and white students, the minimum number of students who would have to be shifted to achieve integration is obtained by multiplying the replacement index by the total number of students. Likewise, if we multiply the index of dissimilarity by the number of Negro students in a district, we obtain the number of Negro students who would have to be shifted to achieve the same goal if all white students remain in their current schools (cols. 6 and 7 of table 2). These numbers could serve for roughly estimating numbers of buses, costs of integration, and the like. In terms of numbers of pupils to be shifted it would be more "efficient" for school systems to achieve integration by moving students of only one race—the minority race whether Negro or white—rather than to exchange Negroes and whites among schools. Exchanging Negro and white students involves shifting more students than does moving only minority

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students, although the latter could not be done without closing some schools and enlarging others.

School systems have been under pressure to integrate their staffs as well as their students. Column 5 of table 2 presents replacement indexes for the instructional staffs of the 60 school systems. The task of integrating teachers is somewhat easier than that of integrating students—the replacement index averages 20 for teachers as compared with 29 for students in these 60 cities. Because of the substantial regional differences in teacher segregation and teacher racial composition, the magnitude of the teacher desegregation task is much greater in the South than in the North. The average replacement index for teachers in southern cities is 30 compared with an average of 13 among northern cities. In Atlanta, Birmingham, Memphis, and New Orleans in 1967, nearly 50% of all teachers would have had to be reassigned to integrate the instructional staffs. In these cities, teachers were highly segregated and their racial composition was approximately 50% Negro. In several northern cities, on the other hand, it would be necessary to move only a small fraction of the teachers to produce integration. In these systems, Negroes comprise a very small percentage of the teaching staffs.

SEGREGATION BETWEEN STUDENTS AND TEACHERS

The analysis to this point has demonstrated a high level of segregation between Negro and white students, and a moderately high level of segregation between Negro and white teachers. Additional measures are needed to assess the degree to which Negro teachers are teaching Negro students and white teachers are teaching white students.

Student-teacher segregation indexes are presented in table 3 for the four

TABLE 3
SEGREGATION INDEXES BETWEEN STUDENTS AND TEACHERS FOR ELEMENTARY
PUBLIC SCHOOL SYSTEMS IN SELECTED CITIES: FALL 1967

SCHOOL DISTRICT	SEGREGATION INDEXES			
	White Students vs. White Teachers	Negro Students vs. Negro Teachers	Negro Students vs. White Teachers	White Students vs. Negro Teachers
	(1)	(2)	(3)	(4)
South:				
Atlanta	11	8	91	96
Austin	8	14	82	74
Baltimore	22	15	69	84
Birmingham	4	6	94	99
Charlotte	8	17	74	81
Corpus Christi	5	27	75	68
El Paso	5	68	51	67
Fort Worth	8	10	89	94
Houston	10	16	87	89
Jacksonville	6	12	92	96
Louisville	16	16	63	79

TABLE 3 (Continued)

SCHOOL DISTRICT	SEGREGATION INDEXES			
	White Students	Negro Students	Negro Students	White Students
	vs.	vs.	vs.	vs.
	White Teachers	Negro Teachers	White Teachers	Negro Teachers
	(1)	(2)	(3)	(4)
Memphis	13	11	85	97
Miami	12	17	81	84
Nashville	8	19	78	88
New Orleans	19	17	83	99
Norfolk	11	13	80	85
Oklahoma City	10	20	89	93
Richmond	17	5	87	96
Saint Petersburg	7	21	86	93
San Antonio	7	28	84	78
Tampa	9	31	80	95
Tulsa	9	19	92	87
Washington, D.C.	42	7	51	78
North:				
Akron	19	27	53	56
Boston	28	35	47	78
Buffalo	29	23	54	68
Chicago	29	20	67	92
Cincinnati	25	20	55	70
Cleveland	25	12	68	85
Columbus	18	27	65	74
Dayton	13	15	78	87
Denver	15	37	70	62
Des Moines	12	66	67	62
Detroit	32	17	48	71
Indianapolis	10	19	78	86
Jersey City	24	27	35	50
Kansas City	18	21	64	81
Long Beach	10	33	71	60
Los Angeles	14	25	76	77
Milwaukee	17	16	72	84
Minneapolis	9	44	67	59
Newark	45	21	26	65
New York City	20	31	36	61
Oakland	32	20	33	67
Omaha	18	30	71	93
Philadelphia	38	18	42	73
Pittsburgh	27	28	46	75
Portland, Ore.	10	39	65	75
Rochester	22	25	41	62
Sacramento	9	33	32	45
Saint Louis	17	9	80	94
Saint Paul	8	73	59	69
San Diego	16	40	68	65
San Francisco	20	28	48	56
San Jose	4	67	47	73
Seattle	10	39	56	58
Toledo	11	18	71	78
Tucson	8	54	62	81
Wichita	10	29	78	78
Yonkers	10	50	54	40
Regional averages:				
South	12	18	80	87
North	18	31	58	71

SOURCE.—U.S. National Center for Educational Statistics 1969.

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combinations: white students versus white teachers, Negro students versus Negro teachers, Negro students versus white teachers, and white students versus Negro teachers. Each index is a measure of the dissimilarity in the distribution of two groups among schools. Our expectation is that segregation between Negro students and Negro teachers and between white students and white teachers will be relatively low, while the remaining two indexes will be relatively high. The results (table 3) confirm that teachers are concentrated in schools with students of their own race. Especially in the South, Negro children in 1967 were in schools with Negro teachers and white pupils were in schools with white teachers. In northern cities there were proportionally fewer Negro staff members, and many Negro students attended schools with white teachers. In both the North and South the largest student-teacher segregation scores were for white students versus black teachers. In general and for both regions, segregation indexes increase from one column to the next in table 3.

SEGREGATION IN PUBLIC SECONDARY SCHOOLS

We carried out a parallel analysis with data for secondary schools (including junior high schools) in these 60 cities. The degree of segregation was usually somewhat less in secondary schools than in elementary schools in the same city. Of course, secondary school attendance districts cover larger areas, and larger areas are more likely to include residents of both races. Because of the lower segregation, the task of integrating secondary schools would require proportionally less shifting of pupils or teachers. Still, the same patterns of segregation appear, and in most of these large cities many thousands of secondary students would have to be transferred to achieve integration.

SUMMARY AND IMPLICATIONS

Racial segregation of pupils and teachers was prevalent in public schools of the nation's large cities in fall 1967. In both the South and North, schools had not been desegregated "with all deliberate speed." Thirteen years after the Brown decision all the nation's large cities operated racially segregated school systems. While these general conclusions are already widely accepted, this is the first time it has been possible to provide systematic documentation with national data and an acceptable statistic for comparing cities.

We presented estimates for each city of the task of desegregation, utilizing an index that reflects both the degree of segregation and the racial composition of students. These replacement indexes and the associated estimates of the number of students to be shifted provide rough numerical guides to the size of the task of abolishing racial segregation in each city's

schools. In view of the complexity of instituting this type of social change, these measures cannot be taken as perfect guides to the desegregation programs of specific cities. Most desegregation plans seek to reduce variance among schools in racial composition, not to eliminate such variance. Actual desegregation plans must confront problems of practical utilization of school buildings, of reasonable transportation methods and times, of simultaneous educational innovations (middle schools, educational parks, etc.), and of other activities designed to secure public acceptance.

The standard against which we measured the degree of segregation is that students and staff in each school should have the racial composition that prevails in the city's public school system. This standard is the most obvious one and the one that is most often used. Yet, it is not the only appropriate one. Consider the indexes of staff segregation for northern cities. The average percentage Negro among teaching staff in these cities is 16. To obtain a zero segregation score the average northern city in 1967 needed only 16% Negro staff in each school. Yet 30% of the students in these schools were Negro. Some desegregation suits seek to equalize the percentage Negro among staff and students, for example, at 30% each. Other advocates of school desegregation seek to retain Negro teachers for as many Negro students as possible. Appropriate measures are easy to calculate once a standard is specified.

The list of other factors that might be taken into account in measuring school segregation or designing desegregation programs is endless. We call attention to a few that are particularly relevant from a demographic and policy perspective.

The nonpublic schools in most cities enroll sizable numbers of white pupils but few Negroes. An analysis embracing the total school system, public and private, would be preferred for certain purposes, but the survey does not currently include nonpublic schools. Another expansion of the system of reference would be to treat each metropolitan area as a single educational universe. From the perspectives of human ecology and urban sociology, the metropolis is functionally more relevant than the central city. Treating the metropolis as a set of disparate school systems yields a peculiar pattern of desegregation efforts. Desegregation of schools in a large city without considering the suburbs may accelerate white movement to school districts where the percentage Negro is extremely low. Both municipal boundaries and school system boundaries are subject to state regulation, and numerous metropolitan school segregation cases in both North and South are being decided by the Supreme Court.

The correlation we found between residential segregation and school segregation is not subject to a simple causal interpretation. Residential decisions are affected by the character of nearby schools, and the formulation of school attendance rules is an administrative process that is carried

out with knowledge of residential patterns. This reciprocal process is reinforced by a variety of related decisions on zoning, code enforcement, annexation, school busing, and other local services. Any attempt to desegregate schools must take account of continuing pervasive residential segregation. Simple redrawing of attendance districts (within the structure of a walk-to-school system) is an annual occurrence in most cities to accommodate demographic shifts. The utility of redrawing boundaries to effect desegregation is diminished by the ease with which subsequent and predictable residential changes obliterate desegregation efforts.

The Office of Education followed up the 1967 survey of racial composition of schools with similar surveys, in the fall of 1968 and 1970 (U.S. Office of Civil Rights 1970, 1972). These later data will document substantial changes in school segregation in many cities and little change in others. In the face of such rapid change it is important that similar surveys be undertaken annually and released promptly. To do so would be to capture a rare opportunity to monitor and analyze a major social change as it occurs.

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Ethnicity, Americanization, and Religious Attendance¹

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Two trends in the pattern of Americanization of immigrant groups are noted, one involving decreased second-generation religious interest due to alienation from the ethnic tradition and the other showing an increase in attendance at worship services from first to second generation due to the prominence of religion in American culture. It is suggested, upon a review of the literature, that the pattern of second-generation attendance depends on the extent of difference between the ethnic culture and the dominant American culture. In a secondary analysis of data on New York City Catholics, the respondents are grouped into western, eastern, and southern European categories based on country of origin. There are no meaningful differences in religious attendance among first-generation Catholics; among second-generation respondents there are substantial differences. Western Europeans show an increase in attendance from first to second generation, while southern Europeans show a decrease. It is concluded that the melting-pot concept of assimilation fails to take into account interethnic variations in patterns of Americanization.

The relatively high level of religious participation in the United States is well documented. It has been noted (for example, see Luckmann 1967, pp. 28-40) that while younger, urbanized, more educated (that is, more modernized) segments of both European and American populations are the most likely to evidence reduced levels of religious adherence, the American population as a whole is considerably more active religiously than the European population. Argyle (1958) has observed that American religious attendance is approximately 2.5 times as high as British attendance (p. 36). While the American example has meant a confounding of attempts at inferring a strong relationship between modernization and secularization, it has served to focus attention on perhaps a unique connection between American religion and secular American culture.

¹ This analysis draws upon data made available by the Research Archives of the Columbia University School of Public Health and Administrative Medicine. The Public Images of Mental Health Services Study, for which these data were collected, was conducted by Drs. Jack Elinson, Elena Padilla, and Marvin E. Perkins. The interpretations presented here are ours. We are indebted to the Research Archives for furnishing the data. We also acknowledge with gratitude a critical reading of the manuscript by Bernard Lazerwitz.

The apparently incongruous compatibility between high levels of religious adherence and rapid technological change in the United States has been resolved by noting two basic trends. On the one hand, Argyle (1958) has suggested that "common observation shows that the churches in America have far more purely secular activities than the churches in England" (pp. 36-37). Winter (1961) has made the same point in the case of the increasingly middle-class composition and life-style of the major Protestant denominations (pp. 87-93). On the other hand, American culture has tended to stress the importance of belonging to some religious tradition (see Herberg 1960, p. 256). Wilson (1969) has held that religion has become a part of the dominant system of American values (p. 121). Eckhardt (1954) has stated that religion does three things for Americans: it insures peace of mind, gives the support of "the Man Upstairs" to whatever people do, and supports the "national interest" (pp. 410-13). These two trends have meant the enmeshing of Americanism and religious life, an enhancement of the position of religion because of its place in the national culture.

Much of the literature on the relationship between religion and Americanism stems from Herberg's (1960) work on the role of changing religious loyalties in the assimilation of diverse immigrant populations. The "generation hypothesis" put forth in the Herberg volume has led to the tendency to gauge the extent of an immigrant group's adjustment to American life by its participation in religious activities. Religion has come to occupy a central position in American culture primarily through being instrumental in Americanization.

It is our purpose in this paper to review briefly the literature on church attendance vis-à-vis generational levels and to point out the conflicting conclusions that can arise from a consideration of the empirical findings that have been reported. We then consider the specific case of Catholics and report the results of a secondary analysis of data collected in 1963-64 from residents of New York City.

AMERICANIZATION AND RELIGIOUS INTEREST

Herberg (1960) comments that "the religious community has emerged under compelling circumstances to serve as a context of self-identification and social location in contemporary American life. . . . Increasingly the great mass of Americans understand themselves and their place in society in terms of the religious community with which they are identified" (p. 36). (For the most recent discussion and testing of this idea see Laumann 1969.) America has a "triple melting pot" consisting of the three major religious orientations (see also Kennedy 1944; Gordon 1964). Building on the "principle of third-generation interest," first introduced by Hansen (1952), Herberg developed (p. 19) his well-known thesis that many Americans are

now third generation and are more likely to attend religious services than their parents, who evidenced depressed attendance rates in undergoing Americanization. Herberg represents the second generation as wanting to avoid identification with an ethnic subculture and being unable, at the same time, to assimilate fully into the mainstream of American life. The first generation strongly identifies with its country of origin and pays less attention to the dictates of American culture. Thus while the first generation pays attention to religion because it is part of its national heritage (of the country of origin), the second generation rejects it for the same reason. The third generation finds renewed identification with religion after exposure to the rootlessness of their parents and the consequent desire for tradition and stability.

From his 1958 study of Detroit residents, Lenski (1963) reported that "basically our findings confirm Herberg's thesis that the Americanization process is linked with the recent strengthening of religious associations" (p. 44). He suggests (p. 45), however, that the Herberg thesis could be modified by noting a tendency for religious activity to increase over generations, with the second generation being as active or more active than the first in religious-service attendance. In fact, Lenski found that for Catholics the greatest increase in attendance was between the first and second generations. The cell frequencies in his study were too small, however, for extensive controls (for example, there were only 36 first-generation white Catholics in his sample).

Subsequent work on the Herberg thesis has been done by Lazerwitz and Rowitz (1964). They report (p. 529) that the Lenski finding of religious attendance increasing with generation holds true for Protestant men and women for the national sample survey data that they analyzed. Catholic men displayed the Herberg pattern of diminished second-generation attendance and increased third-generation attendance.

Taken as a whole, these findings illustrate the extent of ambiguity present in our knowledge of both religiosity and ethnic assimilation. Nevertheless, it is possible to clarify the points at which the various findings fail to cohere.

The Herberg hypothesis stresses the qualitative differences in social pressures impinging on the different generations. While the first generation responds primarily to the dictates of the ethnic tradition and the ethnic community, the second generation responds both to these pressures (negatively, by fleeing from them) and the pressure to assimilate. The third generation is more fully assimilated and becomes interested in religion from a more thoroughly Americanized perspective. While the first and third generations are similar in that they show interest in religion, they are different in that the first generation's understanding of religion is "foreign," while that of the third generation is "American."

Lenski's findings lend themselves to a different interpretation. If religious

interest increases from first to second and from second to third generations, then the pressure to become religious can be seen as part of the more general pressure to assimilate into American society. This suggests that the same sort of pressure is exerted on all three generations, but that it becomes stronger for the second and strongest for the third. Thus the differences among generations is quantitative rather than qualitative.

The conflict between the Herberg theory and our interpretation of the Lenski findings is more apparent than real. Herberg's work is essentially a historical study of the origins of the role religion has come to play in American culture. Indeed, Herberg's work has its germ in a paper delivered by Marcus Hansen (1952) to the Augustana Historical Society in 1938. Lenski's data were collected at a time when the culture pattern whose origins Herberg traced had become a *fait accompli*. Thus the Herberg study can be taken as an account of the development of a culture pattern that made the Lenski finding probable. In any case, relatively recent immigrants must assimilate into an American culture in which religion occupies a quite different position from that encountered by the great waves of immigration in the 19th century.

The findings of Lazewitz and Rowitz, however, indicate that the tendency for succeeding generations to become more religious may not be pervasive. The fact that Protestants in their study displayed the pattern observed by Lenski, while the Catholic men behaved more in the manner suggested by Herberg, indicates considerable irregularity. It is undoubtedly true, as Glazer and Moynihan (1963) point out, that "American society . . . could not, or did not, assimilate the immigrant groups fully or in equal degree." Metzger (1971) has cautioned against the continued use of an "assimilation model" to analyze the relationships between ethnic communities and the larger American society. Findings by Laumann (1969) support this contention. In any case, it is necessary to consider the possibility that the path to accommodation with American society is different for various immigrant groups and that these differences are reflected in transgenerational variation in religious involvement.

Glazer and Moynihan (1963) ascribe differences between the various ethnic communities to the interaction of the culture of the country of origin with American culture, rather than to the country of origin itself (pp. 13-14). This interaction produces characteristics distinct from both of these countries, though they involve a combination of the two. The relative smoothness with which the culture of the country of origin fits in with the dominant American culture determines in large measure the extent to which the second generation is free from the social psychological difficulties associated with culture shock. To the extent that the two cultures conflict, the second generation can be expected to show less tendency to be assimilated and also less interest in religion.

Ethnic differences may influence the pattern of religious interest in a different way. Differences in religious factors have been accounted for by alluding to cross-national differences in orientations to religion (see Greeley and Rossi 1966, pp. 94-99). Thus the historical tendency for Italians to take religion in a "casual" way is counterposed with the traditional piety of the Irish. In any case, patterns of change over generations may be confounded by "national character" differences.

HYPOTHESES

The findings of Lazerwitz and Rowitz might indicate, in the light of the above discussion, that the ethnic heritage of Protestant immigrants is less of a liability to assimilation than is that of Catholics. The second generation's increase over the first in religious attendance suggests that this generation is less caught up in casting off "alien" ties than is the second generation of Catholics. In analyzing only New York City Catholics, we would expect, however, that the diverse ethnic tradition of that city would minimize the alienation of the second generation and lead to an overall pattern of increased second-generation attendance. The third generation should evidence an even higher rate of attendance. Moreover, we would expect to find a typical pattern of higher levels of religious attendance among women than men and among older than younger segments of the population, not only because these individuals traditionally evidence higher rates of attendance (Lenski 1953; Lazerwitz 1961), but also because these would be less likely to feel the pressures for assimilation.

Concerning the differences in ethnic traditions, we would expect to find considerable differences among nationalities in religious attendance if rates of attendance were affected by a phenomenon such as "national character"; for this variable to be operational we would expect differences to be most pronounced in the first generation since it is this generation that is most strongly influenced by ethnic tradition. On the other hand, if ethnic differences in religious attendance are due to a lack of similarity in patterns of assimilation, we would then expect differences in attendance to be greater at the second- rather than the first-generational level. In our analysis we favored the latter position—that a particular group's religious behavior is more the result of that group's experience in America than its experience prior to immigration.² We therefore expected to find greater differences

² Femminella (1961) proposes an impact rather than assimilation model to describe Italian influence on American Catholicism whose tone and character were "immigrant Irish" and not Italian. He describes indifference to the church as one of several reactions to Catholicism (pp. 238-40). Mobility was seen by these individuals as being located outside the church. Our analysis and Femminella's model are most at odds in the latter's assignment of great importance to the difference between the religious spirit of first-generation Italian Americans and the cold formalism found in the

among nationalities in religious attendance in the second generation than in the first.

The Herberg pattern of second-generation decrease should hold true among nationalities whose traditions are less an asset to Americanization, and a pattern of second-generation increase in religious attendance should emerge among nationalities whose traditions are more of an asset. The groups that are most easily assimilated evidence increased church attendance because their acceptance into American life has validated those institutions (including the religious identification) that they brought with them. Conversely, those whose assimilation has been more difficult tend to lose faith in the institutions (including their basic religious affiliation) which they brought with them.

Carried to its logical conclusion, this would seem to indicate that tradition has relatively little extended power to influence a people unless those traditions are reinforced in the new setting, that is, by a willingness to rapidly assimilate a people brought up under those institutions. Specifically, among Catholics from countries farthest removed from the Anglo-Saxon ideal generally held by Americans, there should be a decrease from first to second generation; for example, immigrants from western Europe should evidence a pattern of increase in attendance in the second generation, while immigrants from southern Europe should evidence a decrease. (For a discussion of the history of anti-Catholic prejudice, see Billington 1938; Higham 1963.)

THE ANALYSIS

This secondary analysis draws upon data made available by the Research Archives of the Columbia University School of Public Health and Administrative Medicine. The sample is representative of the population residing in New York City.³ Included within the sample were 885 white Catholics,⁴

American church (see pp. 236-37). If we had given prominence to this disparity we would have expected greater differences in attendance by origin in first- rather than second-generation respondents.

³ According to Elinson, Haberman, and Gell (1967, p. 221), the data for the Public Images of Mental Health Services Study were collected in a "multi-stage, probability area sample of housing units, with all persons 20 years of age and over in each housing unit to be interviewed, designed to yield a self-weighted sample of adults." They note that "this sample is representative of the five million adults living in New York City, with each borough represented in proportion to its adult population." Working from census tracts, blocks, and housing-unit sets, 1,653 housing units were selected, or one per 1,724, out of the estimated 2,818,000 units in New York City as of the end of 1962. From these, 1,501 were randomly selected; and, after a loss of 66 due to being vacant, enumeration of adult household residents was made for 1,260 units, or 88%. A total of 2,118 respondents were interviewed, which was 87% of the eligible persons listed. The design for this multistage, systematic (cluster) sample is described more fully by Elinson, Padilla, and Perkins (1967, pp. 154-95), and comparisons are made between

of whom 41.0% were foreign born, 39.9% second generation, and 19.1% third (or later) generation.

Respondents were placed in the first-generation category if they reported their birthplace as being outside the United States. Those designated second generation were born in the United States, but their parents were born in another country. Of the second-generation respondents, one-fourth were of mixed parentage (one parent born in the United States). We included these as second generation in our tabulations, following Lazerwitz and Rowitz (1964), who generally lumped the children of foreign born with those of mixed parentage (about one-third of their second generation were of the mixed type). Table 1 shows the composition of first- and second-generation respondents by nation of origin.

The respondents were further grouped (see table 1) by the region of the country of origin. These groupings include Latin American, which was almost entirely Puerto Rican and almost entirely first generation; western (and northern) European, of which respondents of Irish descent predominate; eastern European, with respondents of no single national origin dominating the group; and southern European, nearly all of whom are of Italian extraction. In cases in which both parents were born abroad but in different countries, the respondent was classified according to the nationality of the father; among respondents who had both parents born abroad, only four had parents who came from different regions.

characteristics of interviewees and New York City 1960 census data. The N of the entire sample was 2,118, but for our use, lack of religious identifications at both adult and childhood periods reduced the N to 2,091. Within this N , the number of Catholics was slightly reduced. We have not reported F -tests of significance from the sums of squares given by the multiple classification analysis (MCA) program for computing such tests, since these statistical procedures would be based on the assumption of simple random sampling. Regina Loewenstein has indicated to us that cluster-sampling errors were never computed for this study. Thus we have placed reliance upon the size of coefficients (R , η , and β) as well as the direction of percentage change. Furthermore, additional support is given our study by the consistency of the findings, especially when compared with those reported by Lazerwitz (1970) and Lazerwitz and Rowitz (1964). Through personal communication, Bernard Lazerwitz has indicated to us that for his Chicago area sample there are no interacting factors among sex, generation, age, social status, and religious service attendance (the dependent variable). While we have not utilized the automatic interaction detector program (AID) (which complements MCA), we would not expect to find interaction among these same factors in the New York City data.

⁴ The analyst researching the influence of various variables upon religious attendance faces the decision whether to base the respondent's religious identification on the religious faith in which he grew up or on his present religious orientation. For example, there were two individuals who reported growing up in an agnostic or atheistic home environment, but there were 18 respondents presently with these orientations. For this paper we have utilized present religious faith. Of the total number of Catholics, 7.2% made basic changes of faith (for example, growing up Catholic but changing to Protestant or "None"). It should be noted that Orthodox Catholics have been included in the analysis; these constituted only 3% of the Catholic sample.

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TABLE 1
RESPONDENTS' COUNTRIES OF ORIGIN BY GENERATION

REGIONAL GROUPING AND NATION OF ORIGIN	N RESPONDENTS BY GENERATION	
	First	Second
Latin America:		
Puerto Rico	144	4
Cuba	5	1
Other Spanish or Portuguese	13	1
All other Western Hemisphere	2	0
Subtotal	164	6
Western and northern Europe:		
Austria	5	9
Germany	16	14
Ireland	26	60
United Kingdom	4	9
Scandinavia	2	4
Other western and northern Europe	5	7
Subtotal	58	103
Eastern Europe:		
Czechoslovakia	3	9
Hungary	3	8
Poland	10	15
USSR	6	3
Other eastern Europe	9	3
Subtotal	31	38
Southern Europe:		
Italy	89	184
Other southern Europe	12	9
Subtotal	101	193
Other:*		
Subtotal	9	13
Total	363	353

* Includes respondents from Canada, the Middle East, and the Far East. These respondents were excluded from the analysis, and all textual comments are based upon the revised *N*.

All respondents who reported both themselves and their parents as being born in the United States were designated as "third or higher" generation and will be referred to as "third generation." There was no way of ascertaining the national origins of these respondents, as the interview schedule asked only whether grandparents were foreign born (country of origin was not included). Therefore, the third-generation respondents were omitted from that part of the analysis that dealt with comparisons of nationality.

Religious attendance was the main dependent variable in the analysis. The data included the responses to two questions on religious attendance—regularity of attendance and whether the individual attended during the

past week. Since the latter item is more likely to produce more factual responses, it was utilized in this secondary analysis.

The data were subjected to multiple classification analysis (for information on MCA see Andrews, Morgan, and Sonquist [1967]), which is a multivariate technique for examining interrelationships between several predictor variables and a dependent variable. The former can be at less than interval (i.e., nominal or ordinal) measurement, while the latter can consist of a dichotomy. The program gives the relationships among each predictor and the dependent variable, adjusting for the effects of the other predictors. Besides computing the multiple correlation coefficient, the program gives the zero-order (η) and partial (β) coefficients. After examining proportions, we have assigned signs to η and β coefficients.

THE FINDINGS

Table 2 shows the relationships between religious attendance and generation, sex, and age. Older respondents attend in greater proportion than

TABLE 2
RELIGIOUS ATTENDANCE BY GENERATION, SEX, AND AGE

Predictor Variable	N in Each Category	Percentage in Each Category	Percentage Attending Services	Adjusted Percentage	η	β
Sex:						
Male	403	45.5	50.4	49.4
Female	482	54.5	64.1	64.1	0.14	0.13
Age:						
39 and under ..	384	43.4	50.5	50.0
40 and over ..	501	56.6	63.5	63.9	0.13	0.14
Generation:						
First	363	41.0	52.6	52.1
Second	353	39.9	59.5	59.2
Third	169	19.1	65.7	67.5	0.10	0.12

$R = .21$

NOTE.—Includes all white Catholics (respondents of "other" areas have not been excluded).

younger respondents, and women attend at a higher rate than men. It can be seen that attendance increases over generation, with 52.1% of first-generation white Catholics reporting attending church within the past week, compared with 59.2% of second-generation respondents and 67.5% of third-generation Catholics.

The conclusion of religious attendance increasing by generation is weakened when the fact of different ethnic compositions of the first and second generation is noted. Table 3 reports the percentages of respondents attend-

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TABLE 3

RELIGIOUS ATTENDANCE BY REGION OF ORIGIN, SEX, AGE, AND GENERATION,
FIRST- AND SECOND-GENERATION CATHOLICS

Predictor Variable	N in Each Category	Percentage in Each Category	Percentage Attending Services	Adjusted Percentage	η	β
Region:						
Southern Europe	294	56.1	54.1	54.3
Eastern Europe	69	13.2	71.0	71.5
Western Europe	161	30.7	68.3	67.6	0.15	0.15
Sex:						
Male	253	48.3	52.6	53.3
Female	271	51.7	68.3	67.6	0.16	0.14
Age:						
39 and under ..	167	31.9	52.7	53.7
40 and over ..	357	68.1	64.4	64.0	0.11	0.10
Generation:						
First	190	36.3	61.6	60.0
Second	334	63.7	60.2	61.1	-0.01	0.01
$R = .22$						

NOTE.—Includes white first- and second-generation Catholics of western, eastern, and southern European origins.

ing religious services in the past week by region of origin. The respondents who were born in Latin America have been deleted because of a lack of a sufficient number of second-generation New Yorkers of Latin American extraction (see table 1). When only Europeans are considered, there is no increase in attendance between first and second generations for New York City white Catholics.

Tables 4 and 5 give differences in religious attendance by region of origin within the first and second generations, respectively. Within the first generation there were few differences in religious attendance by region of origin. Indeed, the coefficient for the multiple correlation was only .03, and the η and β value for region was at a low level (.06). If we had included Latin American respondents in the first generation, the differences by region would have been substantial, due to differences in attendance between those respondents who were born in Latin America (who exhibited relatively low attendance) and those who were born in Europe (who exhibited relatively high attendance). This may be due to traditional Latin American anti-clericalism or to the overall poverty of Puerto Rican immigrants. In any case, there are no apparent differences of a "national character" nature among the respondents from different areas of Europe.

Differences among respondents of parents born in different areas of Europe are more pronounced among second-generation respondents, as can

TABLE 4
RELIGIOUS ATTENDANCE BY REGION OF ORIGIN, SEX, AND AGE,
FOR FIRST-GENERATION CATHOLICS

Predictor Variable	<i>N</i> in Each Category	Percentage in Each Category	Percentage Attending Services	Adjusted Percentage	η	β
Region:						
Southern Europe	101	53.2	59.4	59.5
Eastern Europe	31	16.3	67.7	68.4
Western Europe	58	30.5	62.1	61.7	0.06	0.06
Sex:						
Male	94	49.5	55.3	55.4
Female	96	50.5	67.7	67.6	0.13	0.13
Age:						
39 and under ..	38	20.0	57.9	56.7
40 and over ..	152	80.0	62.5	62.8	0.04	0.05
$R = .03$						

NOTE.—Includes white first-generation Catholics of western, eastern, and southern European origins.

be seen from table 5. About one-half of the respondents of southern European background attended religious services in the past week, compared with about three-fourths of the respondents of eastern and western (and northern) Europe. The value for the multiple correlation coefficient (.28)

TABLE 5
RELIGIOUS ATTENDANCE BY REGION OF ORIGIN, SEX, AND AGE,
FOR SECOND-GENERATION CATHOLICS

Predictor Variable	<i>N</i> in Each Category	Percentage in Each Category	Percentage Attending Services	Adjusted Percentage	η	β
Region:						
Southern Europe	193	57.8	51.3	51.7
Eastern Europe	38	11.4	73.7	73.8
Western Europe	103	30.8	71.8	71.1	0.21	0.20
Sex:						
Male	159	47.6	50.9	52.0
Female	175	52.4	68.6	67.6	0.18	0.16
Age:						
39 and under ..	129	38.6	51.2	53.7
40 and over ..	205	61.4	65.8	64.2	0.15	0.10
$R = .28$						

NOTE.—Includes white second-generation Catholics of western, eastern, and southern European origins.

is substantially larger here than in the previous table (.03). Also, the values for η (.21) and β (.20) for the regional variable are at a moderate level, whereas before (table 4) they were at a low level (.06). In summary, regional differences are interpreted as being of low magnitude at the first-generational level, while at the second-generational level regional differences are seen as prominent.

These strong second-generational differences, coupled with the absence of first-generational differences, indicate that ethnic differences in religious attendance (with the exception of Latin Americans) are more the result of the encounter between European and American cultures than the nature of the European cultures themselves. Furthermore, in examining the direction of change from first to second generation within regional groupings (comparing tables 4 and 5), it can be observed that southern Europeans decrease in attendance from 59.5% in the first generation to 51.7% in the second generation, a loss of 7.8%. Eastern Europeans slightly increase in attendance from 68.4% in the first generation to 73.8% in the second generation, a gain of 5.4%. Western (and northern) Europeans increase from 61.7% in the first generation to 71.1% in the second generation, a gain of 9.4%. (All percentages given are adjusted for the effects of other predictor variables.)

Earlier we noted that Lazerwitz and Rowitz (1964) found that Catholic men displayed the Herberg pattern of diminished second-generation attendance (p. 529). In comparing males and females on attendance, as shown in tables 4 (first generation) and 5 (second generation), it can be observed that attendance for females remains at the same level across the two generations while attendance for males slightly diminishes. The decline on the part of males resembles the trend found by Lazerwitz and Rowitz.

We have found diminished attendance for Catholics of southern European extraction. At this point we might ask whether this decrease exists for both men and women or for men only. Table 6 reports percentages attending, with regional grouping and age used as predictor variables and with males and females analyzed separately. Differences in attendance by regional grouping are especially found for second-generation Catholic males ($\beta = .24$), while differences for second-generation Catholic females are less in evidence ($\beta = .18$). Southern European males decrease in attendance from 50.8% in the first generation to 40.9% in the second generation, a decrease of 9.9%. Eastern European males evidence a slight decrease (5.5%) from first to second generation, with 67.0% of first-generation males attending compared with 61.5% of second-generation males. Western European males, on the other hand, show an increase of 8.6% in attendance from first to second generation, or from 57.8% to 66.4% attending. Table 6 also shows that southern European females evidence a decrease in attendance from first to second generation, with a decline of 6.6%, while eastern European

TABLE 6
CHANGE IN RELIGIOUS ATTENDANCE BY SEX AND REGION

SEX AND Region	N IN EACH CATEGORY		PERCENTAGE ATTENDING SERVICES		ADJUSTED PERCENTAGE*		CHANGE IN ATTENDANCE (ADJUSTED PERCENTAGE)
	First Generation	Second Generation	First Generation	Second Generation	First Generation	Second Generation	
Male:							
Southern Europe	53	93	50.9	40.9	50.8	40.9	- 9.9
Eastern Europe	15	18	66.7	61.1	67.0	61.5	- 5.5
Western Europe	26	48	57.7	66.7	57.8	66.4	+ 8.6
η , β †	0.12	0.24	0.12	0.24	...
Female:							
Southern Europe	48	100	68.8	61.0	68.3	61.7	- 6.6
Eastern Europe	16	20	68.8	85.0	69.8	83.2	+13.4
Western Europe	32	55	65.6	76.4	65.7	75.8	+10.1
η , β †	-0.03	0.20	-0.03	0.18	...

* With sex specified, using region and age as predictors.
† η refers to unadjusted data and β to adjusted data.

Americanization and Religious Attendance

and western European females increase in attendance, with gains of 13.4% and 10.1%, respectively. In sum, a decline in attendance from first to second generation is shown by both males and females of southern European extraction, although the decline is more pronounced for males than females.

If religious attendance taps Americanization, a very different pattern of assimilation is indicated among southern, eastern, and western Europeans. Americanization depresses attendance for second-generation Americans of southern European extraction. Eastern European males show a decline in attendance, while eastern European females evidence an increase in attendance; but the small *N* sizes here must be remembered. Finally, Americanization acts to increase attendance for second-generation Americans of western European background.

CONCLUSIONS

Through a secondary analysis of religious attendance for New York City white Catholics, we have found that higher attendance exists for females and for older respondents. While the analysis of the entire sample of Catholics has succeeded in confirming Lenski's finding of increasing attendance over generations, the difference between the first and second generations can be attributed to low attendance among Latin American respondents, nearly all of whom were first-generation Americans. A comparison of first- and second-generation attendance rates for respondents of European origin only showed very little difference. A comparison of respondents from southern, eastern, and western (and northern) Europe, however, showed that, rather than discarding any theory of generational differences in religious attendance, we can at least tentatively accept both the theory of attendance increasing over generations and the hypothesis of second-generation decline in religious interest as long as the conditions under which one or the other holds true are specified.

The Herberg thesis rests on the assumption that the second generation finds disengagement from a "foreign" culture to be problematic. Since second-generation Italians display the decline in religious interest suggested by Herberg, it is reasonable to ask whether the already observed close-knit nature of family and neighborhood (Glazer and Moynihan 1963, p. 188) and its tendency to produce a teen-centered culture among second generation youth (Whyte 1955) has produced a statistically visible alienation which continues into adulthood, or whether southern Italian anticlericalism is responsible (Gans 1962). The latter is unlikely to be solely responsible, since it should affect the first generation more strongly than the second. The tendency for peer-group ties from adolescence to continue relatively unchanged into adulthood, even after marriage (Glazer and Moynihan 1963, p. 189), is generally accepted as a realistic description of second-

generation Italian social patterns. This phenomenon might help explain the low rates of religious attendance among second-generation southern Europeans, but considerable further analysis would be required for such a link to be firmly established.

Whether the prominence of Irish in the Catholic hierarchy (O'Dea 1970; Glazer and Moynihan 1963, pp. 230-38), the pervasiveness of Irish culture in much of New York City (Glazer and Moynihan 1963, pp. 252, 254), or the lack of language problems between first and second generation is sufficient to account for the increase in religious attendance from first to second generation among western European respondents is a question beyond the scope of this analysis. Nevertheless, the large difference in religious attendance among southern, eastern, and western Europeans in the second generation (but not in the first generation) suggests that American society has been experienced in different ways by the different nationalities.

We have also suggested that one factor affecting assimilation is the degree to which the nationality approximates the Anglo-Saxon ideal. Western Europeans are perhaps less identified with the Roman Catholic church than are southern Europeans (to take the two extremes), and thus for the former, religious identification may be more of an asset in assimilation than it is for the latter. Whether this identification is only perceived by the second-generation Catholics or whether it is actual (in the form of prejudice and/or discrimination) is beside the point here. Lazerwitz (1970, p. 57) has suggested that as Jews and white Protestants "become highly incorporated into the advanced urban, industrial matrix that rules life in the United States," they place greater emphasis upon religious education and the value of being involved in the religioethnic organization. Our findings have stressed that Catholics of extraction most resembling this Anglo-Saxon ideal evidence an increase in church attendance from first to second generations because they interpret their religion as compatible with the dominant system of American values. Conversely, Catholics of southern European extraction show a decrease in church attendance. We have suggested that this fact is related to their degree of assimilation into American society.

Due to the location in which the data were collected, our discussion of the experiences of the various ethnic groups has stressed New York City. However, cities not reflecting the same, or similar, ethnic histories as New York City might be expected to show results more in line with their own ethnic compositions. Not only have many cities had different ethnic groups dominating their institutions, but also many (especially those outside the Eastern Seaboard area) have had a smaller proportion of their population coming directly from nations in Europe. Surely this could be expected to affect the various ethnic communities in diverse ways. In seeking to extend this analysis to other cities, therefore, it would be necessary to be sensitive

to differences in the composition of the various groups that make up a city's population.

In our discussion we have suggested several mechanisms that give greater understanding to the differences in religious attendance among second-generation Catholics of European background. At the less speculative level we can state that our findings confirm Glazer and Moynihan's (1963) thesis that the "melting-pot" theory is an oversimplification (p. 13). Any theory of the link between Americanization and religious interest must recognize the complexity of its subject matter if it is to be useful.

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Community Change: A Stochastic Analysis of Chicago's Local Communities, 1930-60¹

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Two different urban ecology perspectives, the static one emphasizing uniform spatial distributions, and the dynamic one emphasizing stages of community change, are compared. More recent syntheses of these two perspectives are discussed. A number of propositions and implicit assumptions from the two perspectives are explored by factor analysis of census data for Chicago from 1930 to 1960. When community changes in the two dimensions of economic and family status were interrelated in a two-attribute stochastic model, the following were found: (1) rates of community change are increasing, but not "uniformly"; (2) society-wide and historically specific contexts must be considered in explaining different types and rates of community change; (3) four empirically derived stages of community change are delimited that show an ordering with respect to family and economic changes; (4) the four stages of change are arranged in "concentric zones"; and (5) the four stages of change are not mere spatial indicators of processes occurring over time but are sequentially ordered.

INTRODUCTION

The growth of a city and the ecological distribution of a city's population were seen to be closely related in the early writings of Ernest Burgess (1925), his colleagues, and students. As a city's population grew, the rising demand for centrally located land and its intensive utilization were offered as explanations for the spatial expansion of a city through the now classic "concentric zones." This same general process of "decentralization" was seen to account for transformations of local communities and neighborhoods as the continuing invasion, competition, and succession of different functions and population types swept outward across the urban landscape. The generality of this descriptive model was established by numerous studies that "mapped" the spatial or ecological distribution of a variety of population characteristics (Frazier 1932), institutions (Mowrer 1927), and types of behavior (Reckless 1933).

The general concept of "decentralization" contained in the above model has both a static and a dynamic component. Students and critics of human ecology have repeatedly observed that concepts and hypotheses are often

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dynamic, while data used to test them are often static spatial distributions which are assumed to be indicators of processes occurring over time. However, a different point is being made here; namely, that researchers with a "static" orientation look at spatial distributions and tend to emphasize "uniform" distance gradients, while researchers having a more explicit concern with dynamic propositions (though often "testing" them with static data) tend to emphasize "stages" of community change.

The static component is seen in studies from a number of disciplines which map phenomena at a single point in time in some form of a "distance gradient" from the central business district outward. Studies of population densities, land values, and other variables considered by urban sociologists, geographers, and economists have resulted in a variety of distance gradient curves usually of an exponential or half-bell-shaped form. With respect to population density, for example, Colin Clark's (1951) early formulation was seminal. He posited a declining or negative exponential curve of population density from the central city outward.

In general, the decentralization concept results in a descriptive model that defines some "uniform" decrease (or, conversely, increase) in the value of variables outward from the city center. A summary and exemplary statement of findings from this perspective is that by Brian J. L. Berry (1971a, p. 101), dealing with one cluster of variables: "At the edge of the city are newer, owned, single family homes, in which reside larger families with younger children than nearer the city center, and where the wife stays at home. Conversely, the apartment complexes nearer the city center have smaller, older families, fewer children, and are more likely to be rentals; in addition, larger proportions of the women will be found to work. This 'family structure' pattern is consistent with the ideas of Burgess, and has been called by sociologists the 'urbanism-familism' scale."

The dynamic component of decentralization, on the other hand, although implicitly referred to in many of the above types of studies, has more often been studied from the perspective of "stages" of community change. A given community or neighborhood located at a fixed point in the urban landscape is seen to go through stages of change as the decentralization process is carried out over time. One of the more frequently cited examples of such stages is the temporal sequence listed by Hoover and Vernon (1959), focusing on housing and density variables: new, single-family subdivisions; apartment development; downgrading usually associated with conversion; thinning out; renewal.

Although there are differences in the two perspectives with respect to questions explored and answers found, they are essentially concerned with the same phenomena, urban growth and decentralization. An important empirical link between the two is that different stages of community change are often defined by some arbitrary division of continua stretching outward

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from the city center. However, the dilemma of empirically demarcating zones or stages of community change when dealing with continua was early noted by Milla Alihan (1938, pp. 224–25): “The five zones, as presented by Burgess, cease to be sharply demarcated from each other, as they appear to be when described in terms of qualitative factors, such as economic and educational standards or types of professions, and so forth. The standard zonal boundaries do not serve as demarcations in respect of the ecological or social phenomena they circumscribe, but are arbitrary divisions.” And put more succinctly: “If the gradients are as continuous as the name implies, the zonal lines can be drawn indifferently at any radius from the center” (p. 225).

One purpose of this paper is to define empirically such stages of change by exploring the relationships between two different dimensions. Alihan also suggested the use of this approach: “The zone can have significance only if it marks a distinction of gradients or between gradients” (p. 225).

REPLICATIONS, REVISIONS, AND SYNTHESSES

The concentric zone model of the city has had a large number of replications and revisions, some of the older studies themselves having acquired the status of “classics.” Homer Hoyt’s (1939) discovery of the “sectoral” pattern of high-rent areas was one such revision, but one which still supported the concentric zone model because, within sectors, decentralization was observed over time. The “multiple nuclei” model of Harris and Ullman (1945) was another revision that posited a more random distribution of functionally specific community nuclei around a central node.

Although the above alternative models necessitated revisions and have resulted in some significant syntheses (Berry and Rees 1969), additional questions were raised by other studies explicitly concerned with the dynamic component of decentralization and the internal change of cities. Kish’s (1954) study of “differentiation” and “organization” of urban areas showed that the spatial distribution patterns of three variables (percentage voting Democratic, percentage professional, and average monthly rent) were not uniform but differed significantly from one another. This suggests that the dynamics of decentralization may produce qualitatively distinct patterns depending upon the variables studied, and also that it is advisable to focus on more than a single variable’s descriptive distribution. Dealing more directly with stages of community change, Duncan, Sabagh, and Van Arsdol’s (1962) study of Los Angeles found that the patterns of population change within local urban communities differed depending upon the growth context for the city as a whole, and upon the historical period in which such growth occurred. They found, for example, that different “community

cohorts" did not exhibit the same characteristics and composition (for example, density) at the same stage of community "maturation."

Since the seminal article of the Duncans in 1955, the study of social-class residential distribution—and specifically its decentralization in metropolitan areas—has been of continuing interest to human ecologists. More recent studies by Schnore (1967), Schnore and Pinkerton (1966), Schnore and Jones (1969), Pinkerton (1969), and Smith (1970) point to variations in the decentralization of socioeconomic variables between central cities and suburbs depending upon a variety of independent variables, such as size and age of the central city. This line of research is essentially a synthesis of the two decentralization perspectives mentioned previously: "age of city" is a dynamic variable suggesting different "stages" in the analysis of static spatial distributions. Pinkerton, for example, found that newer and smaller metropolitan areas showed different patterns of change and less marked status differences between central city and suburbs compared with older and larger areas. Schnore similarly has integrated the dynamic and static components by suggesting that suburban and central city status differentials go through various stages or an "evolutionary" sequence. In the first stage, the suburbs of newer metropolitan areas have a lower socioeconomic status than the central city. However, as the metropolis matures and grows, a later stage is reached consistent with the Burgess model in which suburbs have a higher socioeconomic status than the central city.

More recent work on distance gradient curves similarly has come to integrate both the static and dynamic orientations. For example, drawing upon Clark's work, Newling (1969) formulates a series of bell-shaped curves for a city's population density depending upon the "maturity" or age of the city. As a city becomes older, the "peak" or maximum population density moves out in "stages" while the center experiences a decline in density.

Two recent articles by Haggerty (1971) and Guest (1971) shed further light upon the dynamics of decentralization for socioeconomic variables. Using a stochastic Markov chain analysis, and looking at changes in the distribution of one variable—education—across three concentric zones, Haggerty found that "the factors which were involved in producing concentric zonal patterns in the older northern and northeastern cities have not been radically supplanted by new forces operating to produce an opposite, or entirely different, pattern in the newer cities of the South and West."

Moving closer toward explanation than description, Guest attempts to account for the spatial distribution of white collar workers as the dependent variable by using partial regressions. He finds that there is a slight tendency for the proportion of white collar to increase with distance from the central business district (CBD), and furthermore, that this occupation distribution is explained better by a set of housing variables than it is by other variables

reflecting population composition. Like the previous researchers, Guest also looks at "age of the city" as a variable and concludes that "in general, the older the metropolitan area, the greater the decentralization of white collar workers . . ." (p. 1106).

Although the above studies point to a recent convergence of the dynamic and static components of decentralization, some problems in the literature still remain ill-defined and unexplored. First, treating "age of city" as a variable across several cities does not take into account the dynamics of change for any given city over time. Obviously, the appropriate way to solve this problem is to have measures of variables at two or more points in time for a given city, or sample of cities. The recent article by Haggerty exemplifies an important way of analyzing such data as a stochastic process. However, the Haggerty article also exemplifies a second continuing problem, that of focusing upon a single dependent variable, usually some measure of socioeconomic status. For adequate analysis and the development of theory, there is a need for more complex models that explore the relationships and interactions between two or more variables or sets of different variables. The Guest article is an example of an attempt to move beyond description toward multivariate analysis in that he looks not only at occupational distribution but also at its relationship to housing and population composition or family structure.

A third continuing problem of which most researchers are aware, one noted earlier by Alihan, is that of using empirically derived zones as opposed to relatively arbitrary divisions. The most common demarcation used to date has been the legal differentiation of a central city from one or two rings of surrounding suburbs. This is not to say that these demarcations are not without social and political significance, but there is a need to develop better empirical ways to derive such zonal boundaries.

THE PROBLEM

The purpose of this paper is to look at patterns of decentralization and stages of community change and to test a number of assumptions and implicit dynamic propositions with data from the city of Chicago for the years from 1930 to 1960. In general, the research uses a stochastic model to consider the relationships over time between two general dimensions of urban community structure—socioeconomic status and family status. More specifically, it will note the rate of community change by looking at changes in these two dimensions over the three decades; it will explore the impact of historical and growth contexts of each decade upon local community change along the two dimensions; it will develop distinct stages of community change by exploring joint change along the two dimensions; it will "map" the spatial distribution of these stages of change in a test of the decentral-

ization or "zonal" hypothesis; and finally, it will explore whether or not communities pass through these stages of change in a sequential order over time.

METHODS

The early work in "social area analysis" (Shevky and Bell 1955) and related recent work in "factorial ecology" (Berry and Rees 1969) have produced numerous studies which isolate general dimensions or factors of spatial segregation in urban areas. The three most commonly found factors for American cities are social rank or economic status, urbanization or family status, and racial-ethnic status. Although a number of valid criticisms have been raised against this method,² the continuing accumulation of studies in this field has resulted in some important empirical and theoretical syntheses. For example, the study by Anderson and Egeland (1961) demonstrated an empirical link between the older ecological theories of Burgess and Hoyt and the newer social area analysis. They found that the factor of social rank or economic status varied principally by sectors, while the factor of urbanization or family status varied principally by concentric zones. Important theoretical syntheses are to be found in the works of Greer (1962), Beshers (1962), and Berry and Rees (1969).

Recognizing the limitations of factor analysis, there are still two principal strengths which recommend its use in this research. It is a heuristic device for reducing a complex array of variables to a smaller number of underlying dimensions or factors, and it is rooted in an existing body of theory and research. This method was used as a starting point for isolating dimensions of urban community structure. These basic factors of urban community structure then are used for the primary purpose of this paper, the analysis of community change.

Four orthogonal varimax factor analyses were performed on nine census variables for the years 1930, 1940, 1950, and 1960 (Harman 1960). The variables were preselected on the basis of approximating as closely as possible the variables used initially by Shevky and Bell (1955). The three orthogonal factors that emerged for each decade and the variables loading highest on them are presented in table 1.

For each of the four factor solutions a community area was assigned a standardized score on each factor with a mean of zero and a unit variance

² The major critique of this method by Hawley and Duncan (1957) is still valid in part, as are more general critiques of factor analysis that emphasize the often post hoc interpretation of the factors that emerge. However, as in this research, the use of factor analysis as a heuristic tool rather than an empirical and theoretical end is a research avenue that should not be closed prematurely. More specific limitations of factor analysis for the present research are noted in a later section. One should also see a recent article by Brian J. L. Berry on "The Logic and Limitations of Comparative Factorial Ecology" (1971b).

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TABLE 1
THREE FACTOR SOLUTIONS, VARIABLE LOADINGS, AND VARIANCE EXPLAINED
(1930-60)

	1930	1940	1950	1960
I. Economic status—% variance explained: ...	33.0	34.4	33.0	30.4
% males professional909	.952	.961	.894
Median school years946	.944	.932	.928
Median value of owner-occupied dwellings860	.874	.896	.872
II. Family status—% variance explained:	35.3*	30.5	30.7	19.9
% adults married	— .913*	.831	.885	.872
% females employed877*	— .833	— .891	— .852
% single family dwellings	— .790*	.813	.772	.331
% under five years of age	— .842*	.704	.736	.378
III. Racial ethnic status—% variance explained:	18.1	18.9	20.1*	25.6
% Negro904	.922	— .932*	.911
% foreign born	— .811	— .854	.905*	— .932
Total variance explained	86.4	83.8	83.8	75.9
Number of rotations for varimax Convergence ...	8	8	7	7

* Variable loadings are reflected for family status in 1930, and racial ethnic status in 1950.

of plus one and minus one. These factor scores then were dichotomized for economic and family status, with those above zero being high family and high economic status areas, respectively, while those below zero were categorized as low family status and low economic status areas. Consequently, each community had a score on two factors at four different points in time.³ The racial-ethnic factor was handled differently due to the meaning attached to it from the factor analysis and will be handled somewhat separately in this analysis. The factor scores of the communities were dichotomized primarily because of the small number of areas (75) and the desire to keep expected frequencies large enough for the mode of analysis to be used. The analysis of community change with respect to both dimensions of economic and family status was carried out by using "turn-over tables" or a two-attribute stochastic model (Lazarsfeld 1954; Coleman

³ Given that the factor scores of communities are standardized separately for each decade, this means we are focusing upon relative change of communities rather than intrinsic change. That is, we are analyzing changes of a community's position relative to Chicago's other communities at each point in time. For example, Community A may have the same median value of homes in 1950 and 1960; but if the median value of homes for the other communities has risen during this decade, then Community A would experience a relative decline on this variable. Such a decline would suggest that the community has come to occupy a different relative or "functional" position within the city's overall ecological structure. The above analysis of relative change does not rule out the possibility that we are also tapping intrinsic change, but the former may be considered a closer approximation to the classical ecological concept of "functional niche."

1964). This method provides a systematic way to consider the relationships and interactions between two dimensions at two points in time and apparently has never been applied to the study of community change.

The units of analysis selected were the 75 community areas of Chicago initially delimited by Burgess in the twenties. Although this unit is larger than a census tract, its use is warranted on two grounds.

First, the community areas bear a closer relationship to more general sociological conceptions of local urban communities than do census tracts, which are often little more than statistical aggregates. Recent research suggests that although some of these areas have changed, many remain identifiable as persistent local communities according to the criteria by which they were initially delimited.⁴

Second, and more pragmatically, the community areas were selected as the unit of analysis because their boundaries, in contrast to those of Chicago's census tracts, have remained relatively constant over this 30-year period, permitting greater comparability in the data.

FINDINGS

The Rate of Community Change

There are a number of concepts in the literature of urban ecology that define not only certain patterns of change within cities, but implicitly suggest as well that the rate of change is increasing. For example, the concepts of (1) "differentiation," meaning an increasing complexity of statuses, roles, and interaction found in the urban context, and an increasing variability in their spatial distribution, and (2) of "increase in scale," referring to an increase in territorial size and extensiveness and complexity—both implicitly contain the idea that these processes are occurring at an increasing rate, or accelerating.⁵ If this is true one would expect the rates of community change with respect to economic and family status to

⁴ When the community areas were initially delimited as "natural areas," considerations included: "1, the settlement, growth, and history of the area; 2, local identification with the area; 3, the local trade area; 4, distribution of membership of local institutions; and 5, natural and artificial barriers such as the Chicago River and its branches, railroad lines, local transportation systems, and parks and boulevards" (Kitagawa and Taeuber 1963, p. xiii). In a 1968 survey of 802 Chicago residents sampled from all of the 75 community areas, 42.3% named their community by the same name found by Burgess in the twenties, while 34.9% gave a different name, and 22.8% were unable to give an area name (Hunter 1970).

⁵ An accelerating or increasing rate of change is not a central component of these concepts but is often implied in discussions which utilize them. For a concise discussion of the concept of "increase in scale" and also its relationship to "differentiation" or "fragmentation" see Scott Greer, *The Emerging City* (1962, pp. 41–54). See also Godfrey and Monica Wilson's *The Analysis of Social Change* (1954) for a general treatment of "societal scale."

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be greater in the fifties than they were in the thirties. At first glance, the data appear to support this proposition.

Table 2 shows a marked degree of stability of community areas on these two dimensions for the decade of the thirties as compared to later years.

TABLE 2
CHANGES OF COMMUNITIES, 1930-40 (ECONOMIC STATUS, FAMILY STATUS)

1930	1940				TOTAL
	High ES High FS	High ES Low FS	Low ES High FS	Low ES Low FS	
	Positional				
High ES, High FS	14	3	0	0	17
High ES, Low FS	0	11	0	2	13
Low ES, High FS	1	0	21	2	24
Low ES, Low FS	0	0	2	19	21
Total	15	14	23	23	75
	Proportional				
High ES, High FS82	.18	.00	.00	1.00
High ES, Low FS00	.85	.00	.15	1.00
Low ES, High FS04	.00	.88	.08	1.00
Low ES, Low FS00	.00	.10	.90	1.00

Looking at the major diagonal, more than 80% of the areas in each status category were stable from 1930 to 1940. Of the 75 areas, 65 did not change during this decade, while only 55 and 53 of the areas remained stable during the decades of the forties and fifties, respectively (tables 3 and 4).

It is inherently difficult and at best risky to infer rates of change from only three points in time. Although the fifties do show a greater rate of change compared to the thirties, which would appear to support the above propositions, the rate of change is not uniformly increasing throughout the three decades. The rate of change in the forties is relatively much closer to that of the fifties than to that of the thirties. An alternative explanation of these differing rates of change may be found in the historical and growth contexts of each decade.

The Historical and Growth Context of Community Change

In their article on "Patterns of City Growth," Beverly Duncan et al. note a common oversight in many studies of urban change. They say: "The present patterning of intra-urban population changes can be understood only in terms of the past development of the city, for the form of the city—the 'residue' of its past development—is a major element of the context within which growth occurs" (1962, p. 419). Similarly, a local community's position within the ecological structure of a city may be expected to affect differentially the probability and nature of its change. This proposition can be explored by looking at the effects which a community's prior economic status and prior family status have upon the likelihood of its experiencing change.

Two approaches can be utilized in exploring this proposition. First, one can control for a community's family status and see what independent effect prior high or low economic status has upon the probability of change, and conversely, control for economic status and see the independent effect of prior high or low family status upon change. A second method is to utilize a stochastic model and calculate the "random shocks" toward high or

TABLE 3
CHANGES OF COMMUNITIES, 1940-50 (ECONOMIC STATUS, FAMILY STATUS)

		1950				
1940	High ES High FS	High ES Low FS	Low ES High FS	Low ES Low FS	TOTAL	
Positional						
High ES, High FS	10	4	1	0	15	
High ES, Low FS	0	11	0	3	14	
Low ES, High FS	6	1	15	1	23	
Low ES, Low FS	0	1	3	19	23	
Total	16	17	19	23	75	
Proportional						
High ES, High FS67	.27	.07	.00	1.01	
High ES, Low FS00	.79	.00	.21	1.00	
Low ES, High FS26	.04	.65	.04	.99	
Low ES, Low FS00	.04	.13	.83	1.00	

low economic status and toward high or low family status for each decade and compare them. A random shock may be interpreted as a summary measure of all "exogenous factors" in each decade which tended to produce a shift toward one of the four status categories (Coleman 1964, p. 124).

Using the first method and looking at the decade of the thirties, one sees that prior economic status and prior family status had little effect upon the stability or change of an area along the two dimensions. When controlling for economic status there is almost no difference in the stability of high or low family status areas (.82 versus .85, and .88 versus .90). When controlling for family status there is only a slight tendency for low economic status areas to be more stable than high economic status areas (.88 versus .82, and .90 versus .85).

Looking at table 3 for the forties and controlling for economic status, one sees that the areas of low family status in 1940 are more stable than those of high family status (.79 versus .67, and .83 versus .65). Controlling for family status at the earlier date, there is little difference in the probability of a community remaining stable depending upon its economic status at the start of the decade.

During the fifties (table 4) one sees the highest probability for change

TABLE 4
CHANGES OF COMMUNITIES, 1950-60 (ECONOMIC STATUS, FAMILY STATUS)

1950	1960				TOTAL
	High ES High FS	High ES Low FS	Low ES High FS	Low ES Low FS	
Positional					
High ES, High FS	13	1	2	0	16
High ES, Low FS	1	12	0	4	17
Low ES, High FS	5	0	13	1	19
Low ES, Low FS	0	0	8	13	23
Total	19	15	23	18	75
Proportional					
High ES, High FS81	.06	.12	.00	.99
High ES, Low FS06	.71	.00	.24	1.01
Low ES, High FS26	.00	.68	.05	.99
Low ES, Low FS00	.09	.35	.57	1.01

for all three decades and also the greatest variation in the probability of a community changing depending upon its status at the start of the decade. The range of probabilities for remaining stable in the fifties is from .81 to .57, a range of .24; while in the forties this range was .18, and in the thirties, .08.

Making the same comparisons as in the previous tables and controlling for family status one sees that prior economic status has a marked independent effect in the fifties, with high economic status communities more likely to remain stable than areas of low economic status. Also, controlling for economic status, areas of high family status in 1950 are slightly more stable than those of low family status.

Using the second method of viewing these changes as a stochastic process and calculating "random shocks," essentially similar results emerge (table 5). Consistent with the previous analysis, the random shocks are minimal during the thirties compared with the later decades. Also, during the thirties and forties the random shock toward low family status is greater than that toward high family status, while in the fifties, the random shock toward high family status is much greater than that toward low family status.

The findings with respect to economic status are similar to the previous analysis for the thirties when there was relatively little change in this dimension, and for the forties when there was a slight tendency for greater movement toward low economic status. For the fifties, however, the second method suggests a slightly greater movement toward low economic status, while the previous analysis suggested a slightly greater movement toward high economic status.

These varying effects upon family status and economic status from decade to decade are understandable from the society-wide "exogenous events" of each decade. The Depression of the thirties was a period of stagnation compared with earlier and later years with a slight tendency toward lower family status (Bogue 1955).⁶ The war years of the forties did not see an immediate return to widespread economic prosperity, and the variables used to measure family status suggest a decline in this dimension.⁷ The increasing formation of families during the fifties is readily apparent with the movement toward higher family status. The increasing prosperity of the fifties

⁶ Chicago's growth rates were similar to the national pattern. In the twenties Chicago's population increased by 25.0%, in the thirties this dropped to a 0.6% increase, and again increased more rapidly in the forties, 6.6%. In the fifties, Chicago had a population loss of 1.9% (Kitagawa and Taeuber 1963, p. 8).

⁷ Bogue notes two significant trends produced by the war which help to explain this movement toward lower family status. "The number of white female [labor force] participants increased at a rate more than twice that for white males"; and also, "between 1940 and 1950 the older age groups in the urban population grew at extraordinarily rapid rates." Given that two of these variables load high on the family status factor, one can understand the decrease in family status during this decade.

TABLE 5
PERIOD OF CHANGE

Effect Parameters*	1930-40	1940-50	1950-60	1930-60	t_0-t_{10}
Random shocks:					
Toward high family status (ϵ_1)	.1000	.1434	.4565	.4196	.2216
Toward low family status (ϵ_2)	.1942	.3243	.0692	.4549	.1893
Toward high economic status (η_1)	.0000	.0478	.1141	.0600	.0512
Toward low economic status (η_2)	.0000	.0811	.1384	.0000	.0710
Interaction effects:					
Of high economic status on family status (α)	-.1000	-.1434	-.3868	-.4196	-.1958
Of low economic status on family status (β)	-.1052	-.2709	-.0059	-.4010	-.1192
Of high family status on economic status (θ)	.0445	.2728	.2021	.4251	.1391
Of low family status on economic status (ϕ)	.1670	.1601	.1402	.2913	.1611

* The effect parameters were calculated using the approximate method as developed by Coleman (1964, pp. 166-73).

was not clearly and unambiguously captured in this analysis, perhaps because it was occurring mostly in the expanding suburbs beyond Chicago's city limits.

The above findings support the general conclusion that the probability of change in a community's economic and family status is highly variable from decade to decade and that unique historical processes were operating in each period producing shifting probabilities of change for each dimension. Therefore, to more fully understand changes in a city's internal structure, it is necessary to place them within specific societal and historical contexts. These findings are similar to those of Beverly Duncan et al., who concluded from their earlier study of Los Angeles that "to evaluate the descriptive validity of the [Burgess] scheme . . . allowance must be made for changes in the growth context, whether these be the result of temporary disruptions such as war, of cyclical fluctuations as in the rate of new construction, or of long-run trends like those produced by shifts in modes of intra-urban transportation" (1962, p. 419).

Stages of Community Change

Although the previous analysis points to different influences on community change from one decade to the next, certain uniformities are observed across the different time periods when one turns to the question of whether or not there are distinct stages of community change. In order to develop stages of community change, one must look not only at the probability of change, but at the direction of change and the interaction between the two dimensions of economic and family status as well. To consider this question within the framework of a two-attribute stochastic model, one can first dichotomize each dimension and create four possible status categories (high economic/high family status, high economic/low family status, etc.), and then explore the probability of moving from any one category to the others over time.

To explore this question and develop such stages, two different simplifications of the data can be made which yield essentially similar results. First, one may explore the changes of the 75 community areas over the entire 30-year period from 1930 to 1960. These results are presented in tables 5 and 6. Second, one may condense the data from the three previous tables into a single "turnover table" by summing the three previous tables. The universe then consists of 225 possible moves of communities from one of the four states to another during a decade (tables 5 and 7, and fig. 1). The second method will be referred to in the following discussion, as it tends to minimize effects, while the former maximizes them. Summary results are also presented in table 5 for each decade considered separately, that is, without the simplifications.

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TABLE 6

CHANGES OF COMMUNITIES, 1930-60 (ECONOMIC STATUS, FAMILY STATUS)

1930	1960				TOTAL
	High ES High FS	High ES Low FS	Low ES High FS	Low ES Low FS	
	Positional				
High ES, High FS	10	6	0	1	17
High ES, Low FS	0	8	2	3	13
Low ES, High FS	9	0	14	1	24
Low ES, Low FS	0	1	7	13	21
Total	19	15	23	18	75
	Proportional				
High ES, High FS59	.35	.00	.06	1.00
High ES, Low FS00	.62	.15	.23	1.00
Low ES, High FS38	.00	.58	.04	1.00
Low ES, Low FS00	.05	.33	.62	1.00

The central finding which emerges is that each of the status categories or positions of communities in this two-factor space has a single predominant or most probable direction of change. For example, 17% of the areas having a high economic/high family status at time (1) change to a high economic/low family status 10 years later, and only 6% change to all other possible positions.

Second, one should also note that these probabilities of change are non-reciprocal. Although 17% move from a high economic/high family position to a high economic/low family position, only 2% move in the opposite direction. This nonreciprocal nature of the transitions is true of all four categories, and in general there is three times as great a probability to move in the one predominant direction as opposed to its opposite.

Summary measures of the above findings are seen in the "interaction effects" presented in table 5. The most important and consistent finding, true of all time periods, is the relative magnitude and signs of the effects. Over all the time periods, economic status, both high and low, has an inverse effect upon family status, while family status, both high and low, has a positive effect upon economic status. This means that a community having a

TABLE 7

SUMMED CHANGES OF COMMUNITIES OVER A DECADE 1930-40, 1940-50, 1950-60
(ECONOMIC STATUS, FAMILY STATUS)

TIME 1	TIME 2				TOTAL
	High ES High FS	High ES Low FS	Low ES High FS	Low ES Low FS	
	Positional				
High ES, High FS	37	8	3	0	48
High ES, Low FS	1	34	0	9	44
Low ES, High FS	12	1	49	4	66
Low ES, Low FS	0	3	13	51	67
Total	50	46	65	64	225
Proportional					
High ES, High FS77	.17	.06	.00	1.00
High ES, Low FS02	.77	.00	.20	.99
Low ES, High FS18	.02	.74	.06	1.00
Low ES, Low FS00	.04	.19	.76	.99

high (low) economic status at time (1) is most likely to change, if it changes at all, to a low (high) family status 10 years later. On the other hand, a community of high (low) family status at time (1) is most likely to change to a high (low) economic status. These relationships are characteristic of what Coleman refers to as a system of "mutual negative reinforcement" (1964, p. 175).⁸

Above all, these results show that by considering the interaction between changes in a community's family and economic status, empirically derived stages of change are found. This is in contrast to the relatively arbitrary divisions and distinctions that result when "gradients" or "continua" are developed for dependent variables considered separately and are not inter-

⁸ The example which Coleman gives to describe a four-state system of mutual negative reinforcement is that hunger leads to eating, eating leads to nonhunger, nonhunger leads to not eating, and not eating leads to hunger. This example is more intuitively clear in its logical causation than are the results of this research, but nonetheless it would suggest the following: high family status leads to high economic status, high economic status leads to low family status, low family status leads to low economic status, and low economic status leads to high family status.

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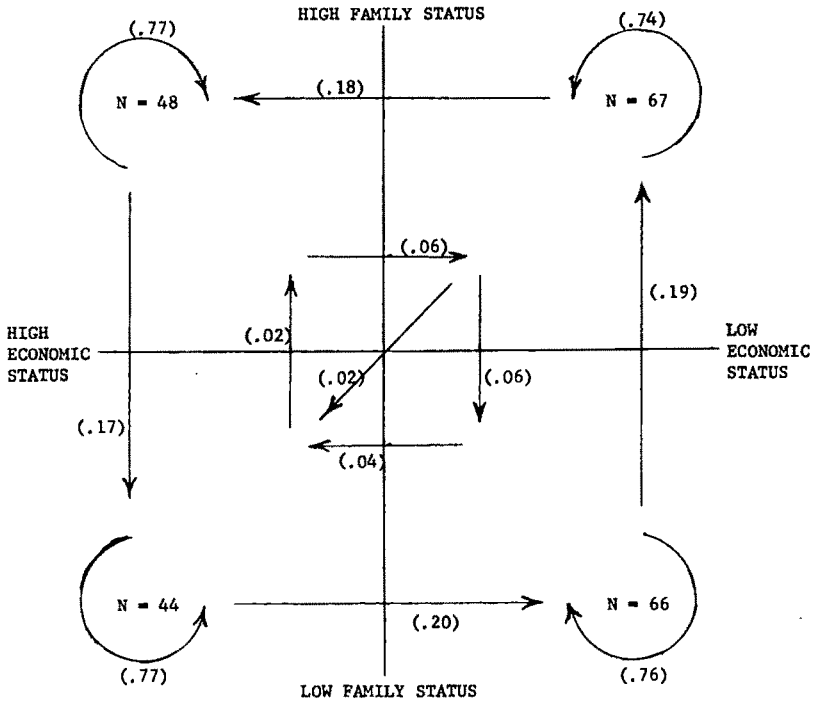


FIG. 1.—Summed positional changes of communities per decade, 1930-60

related. The four stages of community change may be summarized as: stage I—communities changing from low economic and high family status to high economic and high family status; stage II—communities changing from high economic and high family status to high economic and low family status; stage III—communities changing from high economic and low family status to low economic and low family status; and stage IV—communities changing from low economic and low family status to low economic and high family status.

The Spatial Distribution of Types of Change

To further explore these four empirically derived stages of community change and to relate them to the "decentralization" proposition of urban growth, the community areas which made these four most probable moves are located spatially on a map of Chicago's community areas. The findings of figure 2 and table 8 show clearly that these four stages are arranged in a concentric zonal pattern. Communities experiencing stage I changes are most peripherally located, stage II shifts are more centrally located, while stage III and IV changes occur in the most central communities of the city.

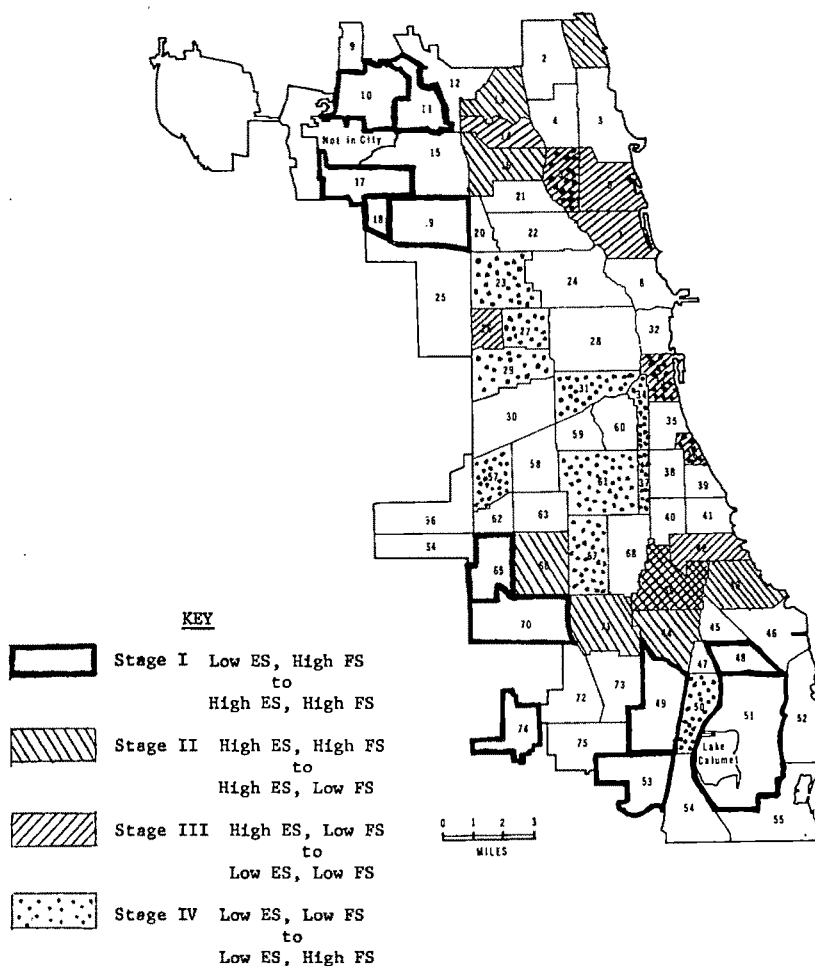


FIG. 2.—Communities experiencing most probable stages of change per decade, 1930-60.

Explication of the Stages and Racial Change

The central communities experiencing stage IV changes, from low economic and low family status to low economic and high family status, might be explained as areas of increasing black concentration with a disproportionate number of children. However, only six of the 13 areas experiencing this stage IV change also had a significant ($>10\%$) nonwhite population at either the beginning or end of the decade of transition. A more general explanation accounting both for the nonwhite areas and for the remaining seven white areas is the finding noted in other studies (Hoover and Vernon 1962)—that many low-income families seek housing in old and relatively

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TABLE 8

STAGE OF COMMUNITY CHANGE BY DISTANCE FROM LOOP AND
PERCENTAGE NONWHITE

Stage of Community Change	N Communities	Average Distance from the Loop (mi.)	N Communities with > 10% Nonwhite Population
Stage I: Low ES/high FS to high ES/high FS	12	11.1	0
Stage II: High ES/high FS to high ES/low FS	8	8.7	2
Stage III: High ES/low FS to low ES/low FS	9	5.3	4
Stage IV: Low ES/low FS to low ES/high FS	13	5.4	6

inexpensive homes and apartments located in poorer areas near the center of the city. It is suggested then that the transition of these centrally located areas into areas of high family status is in part a consequence of the tight housing market for large but poor families in the metropolitan area. It is also in part a reflection of new public housing being built in these older, centrally located areas. Families with higher incomes or fewer children would be more likely to find adequate housing in other areas of the city or the suburbs.

Stage III changes from high economic and low family status to low economic and low family status also occur in centrally located communities. These are areas in which a decline occurs in percentage of residents in professional occupations, median school years completed, and/or the median value of homes owned. Again, four of these nine communities have also experienced racial transition, but this is not a factor in the remaining five. In short, these are communities of low family status which experience a decline in economic status as poorer, less well-educated residents move into the declining housing market.

The more peripherally located communities experience the stage II change from high economic and high family status to high economic and low family status. These are areas experiencing conversion to increased density in multiple-family dwellings, a decline in the proportion of children, a decline in the percentage married, and/or an increased proportion of women in the labor force. Only two of the eight areas experiencing this transition also had experienced racial transition, and these are two black

middle-class communities on the periphery of Chicago's expanding black ghetto. In short, stage II communities are areas retaining a relatively high economic status but experiencing a conversion to a lower family status as they become built up and families move to the suburbs.

The outermost ring of transitions (stage I communities maintaining a high family status and raising their economic status) is the same phenomenon which explains the growth of suburbia over this 30-year period. Middle-class families searching for more space in which to raise their children leave the central city for more sparsely settled areas of single-family homes on the periphery (Rossi 1955). This finding is in part supported by the realization that in 1930 some of these more peripheral areas were still little more than prairie. For example, the community area of Mount Greenwood went from a population of 3,310 in 1930 to 21,941 in 1960, and during the same period Ashburn went from a population of 730 to 38,638. These areas clearly demonstrate that this most peripheral stage I transition is related to the influx of middle-class families into newly developed areas. None of these 12 areas experienced racial change during this period.

The location of these stage I transitions on the city's periphery is consistent with the findings of Schnore and others that in smaller and newer metropolitan areas the suburbs have a lower socioeconomic status than the central city, while older and larger metropolitan areas show a higher socioeconomic status in the suburbs. In short, this research has captured this "evolutionary stage" of peripheral urban communities raising their economic status as the central city grows and these areas come under development.

In addition, these findings show that changes occurring in the family and economic composition of Chicago's local communities cannot be explained solely or even primarily as a consequence of racial transition. In short, racial change is related to, though not identical with, family and economic stages of community change. The above findings on racial change essentially agree with but further specify the conclusions reached by Taeuber and Taeuber in their study *Negroes in Cities* (1965, p. 7):

Traditional accounts of the process of racial residential succession, which stress the low socioeconomic status of the Negro population entering a new neighborhood, the overcrowding and deterioration of housing, the declines in property values, and the flight of whites from the neighborhood are outdated oversimplifications. Expansion of Negro residential areas in recent years has been led by Negroes of high socioeconomic status—not only higher than the rest of the Negro population, but often higher than the white residents of the "invaded" neighborhood. *The invaded areas tend to be occupied by whites of moderately high socioeconomic status, and the housing is predominantly in good rather than substandard condition. . . .* Among both Negroes and whites, the search for better housing

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is led by those who can best afford it. Patterns of racial transition reflect such general processes of urban change as well as the racial attitudes prevalent in the national society. [*Italics added*]

Sequential Stages of Community Change

The above findings of stages of community change and their arrangement into a clear concentric pattern would suggest that there is a transition of communities in sequences from stage I on the periphery to stage II to stage III and finally stage IV, more centrally. However, to demonstrate this sequential pattern, rather than inferring it from a static spatial distribution, it is necessary that the data extend far enough in time to capture a community area experiencing at least two transitions. Fortunately, four of the community areas experienced two transitions during the 30 years under study. Of these four, three show changes supporting the above sequence. Both the Near South Side and Oakland made the stage III transition before going through the stage IV transition. Extending the sequence, Greater Grand Crossing made the stage II transition in the forties and the stage III transition in the fifties. None of the areas experiencing a stage I transition experienced another transition during this period. Therefore, the sequential order is supported for three of the stages.⁹

LIMITATIONS

There are three major limitations of this research which demand caution in the interpretation of its results and also suggest directions for further research. First, and perhaps most obvious, is the fact that it is a single case study. A more comparative analysis might show significant variations in the stages of change and their sequential ordering if more cities and different types of cities were included in the analysis. However, the finding with respect to the zonal location of these types of change suggests that variations in suburban/central city status differentials noted in previous studies is perhaps a function of a community's particular stage in the evolutionary sequence.

A second major limitation is the gross quality of the family and eco-

⁹ North Center, the one area not following the sequence, is one of the more aberrant and rapidly changing communities in the city in terms of the indices of this study. In the thirties it experienced a stage IV transition from low economic and low family status to low economic and high family status. Then, in the forties it made an aberrant shift from low economic and high family status to high economic and low family status; that is, it shifted both its economic and family status. Finally, in the fifties it made the stage III transition from high economic/low family status to low economic/low family status. In short, in 1960 North Center had the same economic and family status that it had had 30 years previously.

nomic factors. Although factor analysis is but an intermediate step in this research and the derived factors are based upon a broad literature, the inclusion of a larger number of more refined variables or dimensions of urban structure might further specify and clarify the processes of community change.

A more specific limitation of factor analysis is the fact that the variables used as indicators of a particular factor may have different loadings at different points in time, and therefore, a community's factor score derived from these factors may shift, not due to any intrinsic or relative change, but because the factors themselves are structured differently. For example, in the current analysis the variables for family status change their loadings from decade to decade, and therefore a community's family score may change over time simply because the variables used to measure this factor are accorded different loadings. This is not a problem with respect to economic and racial-ethnic status, as the variable loadings are fairly constant over the four time periods for each factor. Even for family status, the problem may not be that serious, as the two highest loading variables, percentage adults married and percentage females employed, have fairly constant loadings from 1930 to 1960.

A related limitation of factor analysis may be exemplified by the somewhat surprising finding that both the most central and the most peripheral communities of Chicago have a "high family status." This may be true because one set of variables may load heavily on this factor for peripheral areas (e.g., high percentage of single-family dwelling units, high percentage married) while a different set of variables of the family factor load high for central community areas (e.g., low percentage females employed and high percentage of children). In short, high family status in central areas may be different from high family status of peripheral areas. It is hoped that further research will clarify and specify this finding.

The third limitation is that the research is based solely upon data for the central city of Chicago and does not include its suburbs. This limitation was apparent at several points when interpretation forced speculation about what was happening in the suburbs beyond Chicago's city limits. However, given that the findings are consistent with the recent findings of city-suburban status differentials, and furthermore, that the peripheral areas of the city in the thirties are comparable in growth and development to today's suburbs, these findings would be expected to hold for current patterns of population distribution within the entire metropolitan area. But again, this is speculation.

In summary, the principal limitations of this research, (1) its noncomparative basis, (2) the gross quality of its "variables" derived from factor analysis, and (3) its exclusion of suburbs, will it is hoped be further generalized, specified, and extended, respectively, in subsequent research.

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SUMMARY AND CONCLUSIONS

This study explored a number of assumptions and implicit dynamic propositions contained in the literature about urban growth and stages of community change. By factor analyzing data from the 1930 through 1960 censuses for Chicago's 75 community areas and by using turnover tables and a two-attribute stochastic model, I have explored the relationships between change in a community's economic and family composition.

The findings show that the rate of change for local urban communities has increased during these three decades. However, the rate is not "uniformly" increasing, and it appears that the unique historical context of each decade is more significant in accounting for the differing rates of change than is some general increasing "complexity" or increasing "scale." Through the analysis of "random shocks" as summary measures of "exogenous factors," the findings also showed that change along the two dimensions of family and economic status was highly variable from decade to decade. These findings emphasize that the processes at work transforming the ecological structure of a city need to be more adequately understood in relationship to society-wide and historically unique contexts.

However, the findings also showed a uniformity across time periods in the relationship between changes in economic and family composition of communities through the emergence of four empirically derived stages of community change. This was most consistently seen in the interaction effects where a community's economic status had an inverse effect upon change in family status, while family status had a positive effect upon change in economic status. A community located in one of the four states of the dichotomous, two-dimensional stochastic model had a single most probable direction of change to one of the other three states. The results exemplify a system of "mutual negative reenforcement."

These four empirically derived stages of change were then shown to be arranged in a clear concentric or "zonal" pattern. In short, by considering the interaction between two dimensions, family and economic status, empirically derived zones rather than ad hoc distance zones were delimited.

The findings also showed that the four stages of change are not solely a consequence of racial transition, though the types of change are related.

Finally, the findings indicate that these stages of community change are not merely cross-sectional spatial "indicators" of processes occurring over time, but are in fact ordered sequences of community change. The stages of change, their zonal pattern, and their sequence is summarized as follows.

Sparsely settled areas on the fringe of the city with low economic but high family status are first most likely to raise their economic status as the central city grows and the area comes under development. This is consistent with and further clarifies the evolutionary sequence of central city suburban status differentials found in recent studies.

Areas which have made this transition to a high economic and high family status are most likely to experience a decline first in their family status while maintaining a relatively high economic status. In short, for areas in a state of "decline," family status declines prior to economic status. These are areas having an increased build-up of multiple-family dwelling units, with older but still relatively high economic status residents remaining after younger families have moved. These are also the first areas to experience racial change which agrees with recent findings (Molotch 1969).

Only after first experiencing this decline in family status are areas of high economic status likely to experience transition to a lower economic status as lower class residents move in and the older higher class residents either move or die. Finally, the most centrally located communities of low economic and low family status are most likely to change to areas of high family status, relative to the rest of the city, while maintaining their low economic status. This change is apparently due to the movement of poorer families into relatively cheaper housing both public and private.

These findings will hopefully be further generalized and specified in subsequent research. It is hoped, however, they have demonstrated the utility of using dynamic models which explore the relationships between variables and that this will aid in the movement from description to explanation in the study of community change.

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Partisan Stability and the Continuity of a Segmented Society: The Austrian Case

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Austria has been described as a prototypic segmented society. Religion, social class, and ideology produce sharp distinctions that are incorporated into all aspects of the political life of the country. With pressures from secularization and rising living standards, it can be questioned whether current voter loyalties should be solely related to historic ties with parties. A survey conducted in Austria in 1968 demonstrated that, beyond the organizational continuity of parties and the persistence of social cleavages, there was a separate line of continuity for partisan stability associated with the nature of group ties. While the group benefits that come from a particular party are not always evident, the social bonds linking individuals to parties in a sense bypass this political content. Some of the qualities of Austrian political life—for example, the lengthy experience of a coalition government that established a system for sharing administrative offices on the basis of party affiliation—are unique, but this should not obscure the more general relevance of the findings: that primary group ties continue to have a political importance even when the structural bases of the ties are weakened.

BACKGROUND

The Basis of Partisan Stability

Austrian social organization is a prototype of what Val Lorwin (1971) has called "segmented pluralism" or what the Dutch term *Verzuiling*, literally "pillarization."¹ "A political system is one of segmented pluralism when its cleavages have produced competing networks of schools, communications media, interest groups, leisure-time associations, and political parties along segmented lines, of both religious and anti-religious nature. . . . In the Dutch figure of speech, each of the nation's ideological groups is a 'pillar' standing vertical and separate on its own base of religious or secular ideology. Each has its own party, socio-economic associations, press, leisure-time groups, radio and television broadcasting chain, and—in the case of

¹ On this topic, see Lorwin (1971), Lijphart (1968), Daalder (1966), and Dunn (1972).

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Catholicism and two major forms of Calvinist Protestantism—its churches, parish leagues, and schools” (p. 142). In the case of Austria, these phenomena have their origin in the *Lager* (camps) which are as old as mass participation in Austrian politics.² The total society and its politics were polarized along lines of religion, social class, and ideology. In post-1919 Austria, the German Nationalist *Lager* declined, but the other two—Catholic-clerical and labor-socialist—preempted political life through their political organizations, the Christian Social and Social Democratic parties. Between the wars, the former was in office, the latter in opposition. Their relations were poor and worsened by sporadic paramilitary warfare. The eclipse of both by the Nazis led to a mutual resolve to collaborate in the rebuilding of the occupied country after 1945. A coalition was formed by their reconstituted parties, the People’s party (OeVP) and the Socialists (SPOe). Personnel was selected by a bipartisan patronage system known as *Proporz*.³ The *Proporz* system led, for the first time in Austrian history, to a systematic recruitment of Socialists to the public service. It has been argued, both for Austria and elsewhere, that these sociopolitical segmentations have lost much of their significance in post-World War II society. This is due to the combination of increased prosperity enjoyed by all classes; the general decline of the working class; and increasing secularization, making all militant faiths, religious or otherwise, less attractive (Kirchheimer 1957).

Yet even if the mental states and social institutions have altered, the political behaviors sustaining segmentalization have remained strong.⁴ This appears evident not only from the five instances of segmented pluralism that Lorwin examines—those of Austria, Belgium, Luxembourg, the Netherlands, and Switzerland—but also from a broader examination of persistence in Western party systems since 1945 (Rose and Urwin 1970). Does this mean that partisan loyalties are immune to social changes in the factors that originally gave them their meaning? Is it enough to explain persistence on historical grounds, virtually in terms of the strength of tradition? We think not, and without denying the continuing importance of originating conditions, we look to current circumstances to explain current political practices. To the extent that segmented pluralism is manifested in partisan stability, we argue that its causal mechanisms are embedded in three sets of conditions. The party system, the nature of the social structure, and the character of interpersonal networks affect each other and cumulatively provide the conditions for the continuity of partisan stability.

² For a basic description of *Lager*, see Wandruszka (1954) and Diamant (1958).

³ For differing interpretations of *Proporz*, see Secher (1958) and Engelmann (1966).

⁴ Conclusive evidence to this effect is shown in a recent thorough study of subcultural relations in a small Austrian community, the small city of Hallein (Powell 1970).

Party System

The political importance of a collectivity increases with its ability to act in a consistent and unified fashion. This will occur most readily where the avenues for mobilization remain constant. It is here that political parties play their special role. On the surface, our hypothesis is a simple one: *to the extent that voters are faced with stable party choices over time, partisan loyalty will be high*. The implications of this hypothesis are, however, more profound. For example, Lipset and Rokkan argue that the party systems of Western nations are related to historic cleavages, which have given form to particular parties. These parties in turn owe their support to the mobilization of the electorate along the lines of these deep-rooted cleavages. As a consequence, newly established parties will always have difficulty achieving large-scale support to the extent that the underlying cleavage structure of the society has not appreciably altered (Lipset and Rokkan 1967). Our hypothesis also finds support from Rose and Urwin's examination of party stability since World War II. Their examination of trend data for 19 countries indicates that older parties compared with newer ones show both an increased trend in voter support and less variability in this support (Rose and Urwin 1970).

Further support for our hypothesis is suggested by a negative case, that of France. There politics has long been characterized by political instability, the continual emergence and disappearance of political parties, and consequently an electorate without strong partisan attachments (Converse and Dupeux 1962).

Social Structure

The relation between social structure and party loyalties is more complex, if only because of the possibility that any number of social structural elements can become politically salient. At the minimum we would anticipate that collective interests in material welfare and in group values have greatest potential for political mobilization. In their most potent form such group interests lead to the formation of class and confessional political organizations. These parties have often attempted to encompass the total life experience of their members. It is only, however, some class or confessional interests that could be organized in this fashion. As Lorwin (1971) has observed: "The poverty of social relations experienced by most peasants, workers, and lower middle class elements made possible a concept of 'total' organization for them which one can hardly imagine proposed to a modern upper class constituency" (p. 156). Yet what is crucial in these kinds of parties, as well as in more heterogeneous ones—all of which can be assumed to have some body of core supporters—is not the nature of the party organization. It is rather that the voter's tie with his party is established

through the importance he gives to his group loyalties, experiences, and interests, either singly or in combination. We would hypothesize then that *party loyalties will be strong where group interests can be readily identified with political parties and where consequently parties have a definable body of core supporters.*

Our assumptions about the relation between social structure and party loyalties might suggest that alterations in the former should lead to a decline in loyalty. For example, it could be argued that increased secularization should lead to lessened support for religious parties. Alterations in the class structure through a relative decline in the industrial labor force, along with generally increased prosperity, should bring about decreasing support for working-class parties. Trends in electoral support in 19 countries, however, do not support either contention (Rose and Urwin 1970). We interpret these findings not as a refutation of our hypothesis, but as added support. We do so because we see them as consistent with our notion of core supporters. Core supporters remain attached to a political party not primarily because of what the party stands for, but because of their ties to a group. Conditions of the collectivity may alter without destroying its salience, and with it, the associated channels of partisanship. For example, the working class may become less militant and the religious community less devout, but the social relations that give a member his identity can still remain relatively constant.

Interpersonal Networks

These observations bring us to our third perspective on partisan loyalties, those stemming from the network of interpersonal relations in which the individual is embedded. Through the processes of socialization that take place through ties with others, the individual is placed in the social system. In this way he acquired his religious beliefs, his class position, and, generally, the social and intellectual equipment to cope, whether adequately or not, with his daily responsibilities. Socialization, particularly as it takes place within primary groups, can be viewed as the dynamic for the social structural attachments previously outlined.

The nature of interpersonal networks also adds a new dimension to partisan loyalty by making the attachments personal and direct. For example, a sociologist may relate the regularities he perceives in political alignments to the mesh between group interests and loyalties and the partisan channels that have developed for their expression. This may also be an adequate explanation for the individual voter, accounting for his personal political beliefs and actions. Depending on the extent of his information and the sophistication of his political understanding, the voter could as well account for his position in terms of some more or less coherent

ideology. It is not likely, however, that either group attachments or ideological justifications in themselves will fully account for the party choices of individuals. The self-conscious awareness of group interests may well be a precondition for political mobilization and directed activities. Yet, as Robert Lane has demonstrated in his recent work (1969), based on the political autobiographies of 24 unusually articulate and politically sensitive young men, even the politically sophisticated were unable to present much in the way of political ideas as underpinnings of political action. "What each of these men took to be a belief system marked by the richness of a political philosophy turned out on examination to be a set of undigested and unreflected-upon labels: conservatism, liberalism, internationalism, inadequately illustrated by examples from current events. Almost without exception, these highly intelligent and educated men had failed to give much thought to what *they* believed, though they might have done a creditable job reporting the beliefs of Thomas Hobbes, or John Stuart Mill, or Karl Marx" (p. 313).

If an elaborated ideology is not then the underlying mainstay for the political actions of such individuals, we can hardly expect that it will be more prominent in the lives of less educated and sophisticated voters. For the latter, as indeed is the case for voters generally, ties to the polity are provided by intimate relations with others. Lane (1969, pp. 261-87), for example, provides us with evidence of this in the case of his 24 subjects and their ties with their families. The general processes at work here have long been recognized by students of voting behavior, even when they have not examined the psychodynamics of these relationships in the way Lane does. Since there is an association between an individual's party loyalties and those of persons with whom he is in regular contact, it can be assumed that such loyalties are acquired and sustained in a homogeneous environment (Tingsten 1937; Lazarsfeld, Berelson, and Gaudet 1944). This suggests that a sense of concern with this environment should have a bearing on political loyalties. We hypothesize then that *the more a voter is concerned with the partisan nature of his interpersonal relations, the stronger will be the political and social attachments to his party.*

THE EXISTENCE OF *LAGER* IN PRESENT-DAY AUSTRIA

Stability of Preference

Our goal is not to explain the origins of partisan attachments, but to account for their presence even in circumstances where their decline has been predicted. We do this in the context of present-day Austria, a society which gives evidence of a high level of segmentalization. We rely on a survey of Austrian voters conducted by the Institut fuer empirische Sozialforschung (Ifes), under the direction of Karl Blecha. A random sample of 1,720

adults, 18 years of age and older, was interviewed in the spring of 1968.⁵

Since it is stability we wish to account for, the primary focus of our analysis is on consistent supporters of the two major parties, OeVP and SPOe. Consistent support was measured by an index constructed from the vote reported in the elections of 1966, 1962, and 1959 (parliamentary), and 1965 (presidential). The classification "consistent OeVP" was given to voters who stated they had voted OeVP in each of these four elections, or if only eligible in three, for three elections. The same holds for the classification "consistent SPOe." For purposes of comparison, the classification "inconsistent" was given to those who had been eligible in at least three elections but who had not consistently supported one of the major parties, having voted at least once for a minor party, or having switched at least once from one major party to the other, or having abstained at least once—a rare occurrence in Austria. The three categories, "consistent OeVP," "consistent SPOe," and "inconsistent," make up 92% of the entire sample. Of these 1,587 cases, 27% are "consistent OeVP," 36% "consistent SPOe," and 37% "inconsistent."⁶ The proportion of consistent voters is strikingly high, despite general evidence that some Austrians switch parties in presidential elections and the unexpectedly high vote for the OeVP in the parliamentary election of 1966. We are able indeed, because of the high degree of voting consistency, to use this index throughout the paper in lieu of the less valid indicator of partisanship obtained from a single question on party support at the time of the interview.⁷

Sixty-seven percent of those included in the analytic sample named a specific party in response to the question of how their fathers voted when they were children. The correlation between father's party and consistent voting by the respondent strongly supports our first hypothesis on the relation between a stable party system and party loyalty. It does so even in the special circumstances of Austrian history, where the party system suffered from the profound disjunctions of the *Anschluss* and the Nazi period. In its present form the OeVP was created in 1945, after a hiatus of seven years during which its predecessor, the Christian Socials (*Vaterländische Front* from 1934 to 1938), was disbanded and its leadership harassed or imprisoned. This period saw even more drastic measures in force against the predecessor of the SPOe, the Social Democrats, outlawed after its

⁵ This survey was made possible by a grant from the Graduate Research Board of the University of Illinois at Chicago Circle, which we acknowledge gratefully. We also thank the University of Alberta for providing computer time, and Grace Skogstad, Victor Hamm, and Jerry Ezekiel for programming assistance.

⁶ Those omitted include persons born after 1943 and regular supporters of minor parties.

⁷ Powell (1970, chaps. 4, 5) explains consistency of partisanship, *inter alia*, by two phenomena not directly investigated by us: (1) reinforcement of partisan sentiment by party elites, (2) partisan influence of individuals' organizational memberships.

unsuccessful rise against the Dollfuss dictatorship in 1934. Despite these events, the fathers of 61% of the consistent OeVP voters supported the OeVP or the Christian Socials, while 57% of the fathers of consistent SPOe voters supported the SPOe or the Social Democrats. When we eliminate "don't knows" and refusals concerning fathers' behavior, some of which was no doubt related to the Nazi experience, fully 77% of the fathers of consistent OeVP voters manifested similar voting patterns, as was the case of 75% of the fathers of consistent SPOe voters. The slightly lower Socialist percentage, if it has any meaning, can be attributed to a secular increase in SPOe support throughout the Second Republic.

Social Character of Supporters

Those aspects of the Austrian social structure that concern us here are the ones that allow us to distinguish a body of core supporters for each of the parties. To begin with, it is necessary to clarify the concept of "core" in order to separate attributes of the social system from those of political parties. To illustrate, we may use a criterion previously cited by Rose and Urwin (1969): "A communist party supported by only 10 percent of the working class can hardly claim to be the working class party in a country. On the other hand, if 90 percent of the vote it obtains comes from workers, it can be said to be a working class party" (p. 11). In other words, in order to emphasize the social homogeneity of a political party, we must determine whether its supporters come predominantly from one or more social groups. Normally, we cannot expect to find contrasts as extreme as those involving 90% of a group. "Core" is defined here as a characteristic shared by at least one-third of a party's supporters, which also differentiates between parties by at least around 10%, but in most cases much more.

Our second hypothesis leads us to assume that partisan stability in Austria will be a reflection of social homogeneity. Table 1 shows those characteristics in which there was a marked difference between consistent supporters of the Socialists and People's party. The SPOe is the party of workers, of union members, and of those who attend church either irregularly or not at all.⁸ It is generally the urban party, and particularly the

⁸ We chose the categories "several times weekly" and "almost every week" as making up regular church attenders. Thirty-three percent of our respondents were in these two categories (64% consistent OeVP supporters, 14% consistent SPOe supporters). Blankenburg (1967, pp. 22, 114-17, 154-58) has comparable data about Catholics in the West German land of North Rhine-Westphalia, in the Netherlands, and in France. Regular church attenders made up 67% of the Catholics in North Rhine-Westphalia (1962; 41% of SPD supporters, 79% of CDU supporters), 89% of the Catholics of the Netherlands (1956), and 37% of the Catholics in France (1952). By 1966, the regular church attenders among Catholics in North Rhine-Westphalia had declined to 58%.

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TABLE 1
CORE SOCIAL CHARACTERISTICS OF CONSISTENT PARTY SUPPORTERS
(%)

Core Characteristics	Consistent SPOe (570)	Difference, SPOe — OeVP
Irregular, non-churchgoers	87	55
Workers	56	39
Union members	48	33
In cities above 50,000	46	25
Region: Vienna	34	19
Trade-school graduates	53	15
	Consistent OeVP (431)	Difference, OeVP — SPOe
Regular churchgoers	69	56
Farmers	37	35
Non-union-members	85	33
In communities below 2,000	46	26
Region: western Austria	39	13
Common-school graduates	52	9
Business and professional	15	8
Secondary-school graduates	10	5

NOTE.—Because of incomplete reporting, *N*'s (in parentheses) may vary by as much as 18 from the figures given.

party of Vienna.⁹ While less uniformly attracting trade-school graduates (occupational training after common school), it does appeal to a majority of these. The People's party, in contrast, is the party of regular church attenders, of farmers, and of the countryside. It is also the party of non-union-members, of western Austrians, and of those with only a common-school education. The attraction of business and professional occupations and of secondary-school graduates to the People's party does not emerge clearly, since these are only small categories in the total population. Nevertheless, they are represented among consistent OeVP voters to an above-average degree.

At the same time, both parties attract similar proportions of some social categories. For example, both have prominent contingents of civil servants (20% of SPOe, 18% of OeVP consistent voters). Once the preserve of upper- and upper-middle-class groups, the civil service became bipartisan when both parties recruited bureaucrats under the *Proporz* system of the great coalition of OeVP and SPOe (1945–66). White-collar workers are found in similar proportions on both sides as well (13% of SPOe, 12% of

⁹ For most of the ways in which we categorize voters, we must rely on the procedures generally used by Ifes. In their categorization, Vienna is treated as a region, comparable with western and eastern Austria. Vienna is also part of the category of cities above 50,000.

OeVP consistent). Two regional cleavages were apparent, with Vienna "red" and western Austria "black";¹⁰ for the remainder, however, cores of party supporters tend to be distributed more along urban-rural lines, or along lines of provincial idiosyncracies, making for an eastern region with balanced party support.

From the perspective of the social character of supporters, it is evident that party alignments remain associated with polarization of the social structure. At the same time, there are distinct limits to this polarization. Reliable supporters of the People's party tend to live in the country and go to church, while reliable Socialist supporters live in cities and stay away from church services. A half-century in which commerce, agriculture, and labor have been compelled to organize into chambers¹¹ has helped ensure that the occupations associated with the former two will be "black" and those in the third chamber "red." In contrast, two decades of almost equally divided two-party patronage at the federal level and patronage associated with one or other party at the provincial level have cemented split party support of Austria's many civil servants. Another source of weakness for the existence of a completely polarized system stems from the relative status of workers, who tend to be wealthier and better educated than farmers. Both this and the equalization of civil service recruitment keep the OeVP from being a completely middle-class party and the SPOe from being a completely working-class party.¹²

The Partisan Nature of the Interpersonal Environment

Similarity in political affiliation was asked about family members, good friends, colleagues at work, superiors at work, those with whom one associated in sports and other entertainment activities, fellow churchgoers, and those whom one met at places of commerce. Those who answered that almost all their contacts were with persons of the same partisan loyalties ranged from 80% in speaking of family members to 5% in referring to those at places where they shopped. For this part of our analysis our concern is only with family members, friends, and work colleagues, with special attention to differences between the two parties.

In table 2, political homogeneity has been defined as at least a majority of the same political persuasion among family, friends, and colleagues. We also

¹⁰ Christian Socials and OeVP have the epithet "black," while Social Democrats and the SPOe are called "red."

¹¹ The chambers of commerce, agriculture, and labor have compulsory membership and partisan election to their governing bodies (Secher 1960).

¹² Powell (1970, chap. 3), in analyzing one urban community, was able to relate support for the People's party with high socioeconomic status. For the reasons just stated, such a relation does not exist throughout Austrian society.

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TABLE 2
POLITICAL ENVIRONMENT OF PRIMARY GROUPS, COMPARING CONSISTENT
AND INCONSISTENT VOTERS
(%)

Political Environment	Consistent OeVP (270)	Consistent SPOe (389)	Inconsistent (300)
Homogeneous	63	72	47
Not homogeneous	37	28	53

NOTE.—“Homogeneous” is defined as identical political persuasion of *majority* of family, friends, and work colleagues, or *all or almost all* in two primary groups. Nonapplicable cases are excluded. *N*'s in parentheses.

included in this category those who said that all or almost all their intimates in any two of the three settings were alike in partisan behavior. The relation between consistent party voting and a politically homogeneous environment is high, with little difference between the two parties. In contrast, those whom we have defined as “inconsistent” voters were much less likely to report exposure to a homogeneous interpersonal environment. At the same time we must recognize that the fit between consistent support and a homogeneous environment is far from perfect, since more than one-third of the consistent OeVP supporters and more than one-quarter of the SPOe were exposed to primary group relations with those of different political affiliations. This again, as was the case of core support, indicates some flexibility within the Austrian political system.

Our thesis emphasizes not only the importance of the interpersonal environment, but also active concern with maintaining its homogeneity.¹³ To the extent that such concern exists, it reflects a remaining *Lager-mentalitaet*. The term denotes a set of psychosocial attitudes, rather than of systematic ideas, connected with the *Lager*. Respondents were asked whether differing political affiliations among family, friends, and colleagues at work were matters of concern to them. A respondent showing any degree of concern when persons in his reference group were of a different political persuasion than his own—the question was put hypothetically—was classified as “annoyed.” We approach differences in the distribution of annoyance in two ways in tables 3 and 4. In table 3, the focus is on the relation between a homogeneous political environment, as we defined it earlier, and annoyance when members of family, some friends, or some colleagues are of a different persuasion. This is related to partisanship by using respondents’

¹³ Unlike Powell (1970), one of whose main research thrusts was the determination of political hostility, we emphasize active concern with the maintenance of a homogeneous political environment. Therefore, both he and we use concern with interpolitical marriage; but we consider the homogeneity of other primary contacts, while he asks for the likelihood that government by the opposing party might endanger Austria’s welfare.

TABLE 3

ANNOYANCE OF THOSE WITH HOMOGENEOUS POLITICAL ENVIRONMENT WHEN PERSONS
IN THEIR PRIMARY GROUPS ARE OF DIFFERENT POLITICAL PERSUASION, BY PARTY
SUPPORTED AT TIME OF INTERVIEW
(%)

ANNOYED AT PARTY DIFFERENCE WHEN AFFECTING PERSONS AMONG	HOMOGENEOUS POLITICAL ENVIRONMENT FOR THOSE SUPPORTING	
	OeVP	SPOe
Family	53 (210)	48 (349)
Close friends	44 (211)	38 (352)
Colleagues at work	27 (145)	38 (245)

NOTE.—N's in parentheses.

reports of the party they supported at the time of the interview. Consistent party voting, our more usual measure of partisanship, is our focus in table 4.

Regardless of which variable we use to indicate strong partisanship—homogeneity linked to party support or consistent party voting—the OeVP emerges as more annoyed with cross-party contacts when these involve a majority of their primary group. This annoyance is centered on family and friends and not on work relations. Tables 3 and 4 indicate one contrast,

TABLE 4

CONSISTENT PARTY VOTERS' ANNOYANCE WHEN PERSONS IN THEIR PRIMARY GROUPS
ARE OF DIFFERENT POLITICAL PERSUASION
(%)

Annoyed at Party Difference When Affecting Persons among	Consistent OeVP	Consistent SPOe
Family	49 (406)	40 (546)
Close friends	35 (406)	32 (552)
Colleagues at work	21 (223)	31 (286)

NOTE.—N's in parentheses.

however, with more of those in a homogeneous environment expressing annoyance, compared with consistent voters.

For the SPOe, there is also some difference in attitudes toward contact with others, depending on how we measure partisanship. More interesting is our finding, in tables 3 and 4, that almost as many voters showed annoyance with the possibility of contact with a majority of work colleagues of other parties as felt this way about close friends.

The data themselves give us no explanation for these findings, but they do suggest differences between the two parties in the politicization of their respective electorates. The stability of th OeVP is strongly linked to the im-

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portance of interpersonal networks, with the latter helping to ensure the continuity of party support. In the case of the SPOe, voters seem to obtain relatively more of their linkages through the party itself. Certainly, since Socialist voters are so often workers, the work place can be expected to have special importance to them. But it is also true that the work place is a primary political milieu, both for the origins of Socialist political organization and as the avenue for recruitment, reinforcement, and political action.¹⁴

Annoyance, as we have seen, is a function of party ties and the arena of contact across party lines. It is our contention that it is also a reflection of a group's place within a party, that is, whether or not it is part of the core supporters. We would anticipate that core members are most concerned with the preservation of the subcultural purity that their party represents. This issue is examined in table 5, in which both cores and noncores are

TABLE 5
ANNOYANCE WITH DIFFERENT POLITICAL PERSUASION WITHIN PRIMARY GROUP
CONTACTS, BY PARTY CORE GROUPS
(%)

GROUP	CONSISTENT VOTERS		DIFFERENCE, CORE — NONCORE
	SPOe	OeVP	
SPOe core:			
Skilled workers	36 (168)	19 (37)	17
Non- or irregular churchgoers	42 (483)	32 (133)	10
Union members	48 (272)	42 (65)	6
Viennese	42 (191)	43 (63)	—1
Cities above 50,000	40 (264)	43 (91)	—3
Trade-school graduates	45 (299)	49 (163)	—4
OeVP core:			
Regular churchgoers	32 (69)	55 (289)	23
Western Austrians	36 (149)	53 (168)	17
Business and professional	41 (39)	56 (64)	15
Non-union-members	35 (298)	49 (366)	14
Secondary-school graduates ..	38 (26)	52 (42)	14
Common-school graduates	37 (243)	46 (225)	9
Communities under 2,000	48 (113)	50 (195)	2
Farmers	56 (9)	56 (158)	0

NOTE.—N's in parentheses.

shown for both parties. Data on inconsistent voters are not included, but the question may remain whether concern with cross-party relations is related more to the strength of party ties or to the nature of the social group to which one belongs. The answer is clearly that it is party ties which are more

¹⁴ In addition, Socialist affiliations are reinforced through a network of ancillary party organizations, patronage-based municipal housing, and face-to-face contact with collectors of monthly dues (*Vertrauensleute*) (Shell 1961).

important, since generally about one-quarter of the inconsistent voters of each social group reported in table 5 indicated annoyance, a proportion almost always considerably less than that found for consistent voters, whether core or not. This does not, by the way, contradict our earlier suggestions about the OeVP, since there we implied that the strength of partisanship was preserved through interpersonal ties with those similar in political behavior, but not that the ties themselves determined attitudes toward outsiders.

Comparing the two parties in table 5, it is clear that the OeVP confirms our hypothesis on the relation between core supporters and annoyance better than does the SPOe.¹⁵ Taken together, however, there is little doubt that the general outlines of our argument are confirmed. At this time we have no way of accounting for the party differences except to again suggest that those in the People's party seem to feel greater need for the reinforcement that comes from primary group relations, reinforcement that Socialists receive both from their party and from the daily circumstances of their lives.

There is one instance where Socialists indicated every bit as much annoyance with cross-party contacts as did the OeVP. This came from civil servants in our sample, with 53% of the consistent SPOe voters expressing annoyance compared with 44% of the OeVP. This can be accounted for by the nature of the Austrian compromise with respect to filling offices in the federal bureaucracy. The *Proporz* system of the Austrian coalition has made the two parties, in their organizational role, into recruiting agencies for the civil service. A high premium is hence placed on party consistency and purity for civil servants. Socialists especially, being massively involved in the federal civil service for the first time in history beginning in 1945, want to keep their civil service involvement pure.

Perhaps the most intimate measures of group solidarity were tapped by our questions on daughter's marriage and children's playmates. In table 6 this is reported as positive feelings of pleasure if a daughter should marry into the same party as respondent's and if a child were to play with others with the same partisan background. Negative feelings are given in table 7, in response to the question on cross-party marriages and playmates involving one of the two major parties. We note that pleasure was more openly expressed than displeasure, that marriage choices appear more important than playmates, and that the OeVP, in keeping with their other more pronounced feelings about primary group contacts, were more concerned about the behavior of their children than were SPOe.

¹⁵ Here and in the remaining tables we speak only of skilled workers, while in table 1 we dealt with all workers. Workers generally are without question part of the SPOe core, but nonskilled workers turn out to be such a mixed lot, it is difficult to use them in more detailed analyses.

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TABLE 6

PLEASURE WITH FAMILIAL CONTACTS WITHIN PARTY, BY CONSISTENT VOTERS
(%)

Pleased with Same Party for	Consistent SPOe	Consistent OeVP
Daughter's marriage	35 (561)	47 (416)
Children's playmates	24 (562)	39 (417)

NOTE.—N's in parentheses.

The question on marriage is an adaptation of one used in Almond and Verba's *The Civic Culture* (1963). Their question did not differentiate between the sex of children, but pretests in Austria indicated the advisability of asking for feelings about daughter's marriage alone. Even so, there

TABLE 7

DISPLEASURE WITH FAMILIAL CONTACTS ACROSS PARTIES
(%)

Displeased with Opposing Party for	Consistent SPOe	Consistent OeVP
Daughter's marriage	15 (544)	19 (405)
Children's playmates	6 (558)	9 (406)

NOTE.—N's in parentheses.

is still some suspicion that our question did not¹⁶ generate as much reaction to exogamy as some observers feel exists. Given these limitations, the most comparable findings from Almond and Verba are from their German sample, interviewed a decade earlier, and related to voting in the most recent national election. Distribution of responses are quite similar to our own: 42% of the CDU were pleased with intramarriage compared with 25% of the SPD; while 19% of the CDU were displeased with intermarriage, compared with 8% of the SPD (Almond and Verba 1963, p. 136). In Austria, as in Germany, and even in the remaining countries in Almond and Verba's study, it is clear that we are dealing with phenomena that are more salient for parties of the Right. Despite what may be assumed about the more highly ideological content of traditional Socialist parties, it appears that this ideology is not fully protected from outside contacts. Two suggestions are offered by Almond and Verba (1963):

¹⁶ Blecha told us in 1969 that he suspected that a number of people who would be displeased if their daughter married across party lines had not admitted this. There are, however, no data contradicting our findings.

Generally speaking, conservative movements are more traditionally oriented than are left-wing movements. Traditionality implies religious or status exclusiveness. Hence we might expect supporters of conservative parties, more frequently than supporters of left parties, to have an in-group feeling about marriage across party lines. But this attitude on the part of conservative supporters may also be due to status differences. Since the supporters of the left parties tend more frequently to be lower class, in social and economic terms, they view marriage of a son or daughter with a son or daughter of the conservative party as upward social mobility. The contrary attitude on the part of the right may also be a status reaction; that is, they may view such an interparty marriage as a social step down. [P. 139]

In line with the argument we have been pursuing, we would anticipate that both pleasure and displeasure would be more prominent among core groups of consistent voters. This is not the case for the SPOe core. What we have defined as SPOe core who are consistent OeVP voters were equally or even more pleased than SPOe voters on the issue of partisan endogamy for their daughters. When we look at pleasure with children's playmates by groups associated with the SPOe core in table 8, then positive reactions among the OeVP are even more pronounced, with one minor exception. Our results do not seem due to a situation where minority groups within a party are more fearful of outgroup contacts, since in effect they are more

TABLE 8
PLEASURE WITH CHILDREN'S PLAYMATES OF SAME PARTY,
CORE GROUPS OF CONSISTENT VOTERS
(%)

GROUP	CONSISTENT VOTERS		DIFFERENCE, CORE — NONCORE
	SPOe	OeVP	
SPOe core:			
Viennese	31 (189)	51 (61)	—20
Cities above 50,000	30 (260)	46 (89)	—16
Skilled workers	24 (165)	38 (37)	—14
Union members	28 (269)	41 (64)	—13
Trade-school graduates	26 (296)	39 (158)	—13
Non- or irregular churchgoers	26 (477)	23 (129)	3
OeVP core:			
Regular churchgoers	15 (67)	45 (279)	30
Secondary-school graduates	27 (26)	50 (40)	23
Farmers	22 (9)	43 (153)	21
Communities below 2,000 ..	19 (109)	39 (191)	20
Non-union-members	21 (293)	39 (353)	18
Common-school graduates ..	21 (238)	37 (218)	16
Western Austrians	21 (147)	36 (160)	15
Business and professional ...	15 (39)	29 (62)	14

NOTE.—N's in parentheses.

likely to have them with individuals sharing socioeconomic characteristics but of the opposite political persuasion. At least we feel this to be a correct judgment on the basis of data, again in table 8, on OeVP core groups and also from data not shown here on attitudes toward daughter's choice of husband. In both instances, OeVP voters who are both core supporters and consistent in their voting patterns were invariably highly pleased with keeping their children's peer contacts within the confines of the party. For daughter's marriage, difference between SPOe and OeVP voters ranged from 31% to 17%, averaging about 21%, while they were only slightly lower on children's playmates.

Since active displeasure with cross-party contacts was not generally expressed, there is little to be gained by presenting additional tables, but a few results are worth noting. While 19% of all OeVP consistent voters would be displeased if a daughter married an SPOe supporter, 32% of OeVP secondary-school graduates felt this way. Displeasure with children playing with the offspring of SPOe supporters was 23% for secondary-school graduates, 20% for those living in Vienna, and 18% for those in cities above 50,000. All of these groups were made up of consistent OeVP voters, although on the average only 9% showed displeasure. Our data do not help account for these findings, but perhaps there are instances after all where minority position (as with OeVP Viennese and city residents in general) increases sensitivity to cross-party contacts.

Without comparative or longitudinal data, it is difficult to make judgments about the importance of interpersonal contacts, or the uniqueness of the Austrian situation. In general, it is probably true that Republicans and Democrats in the United States have a less homogeneous political environment, but we would fully expect core voters in both parties—automobile workers in Detroit, blacks almost anywhere, small-town businessmen, wealthy Protestants of northern European origins, and so on—to be as politically insulated in their primary group contacts as are Austrians of comparable background. But where Americans and Austrians would differ is in the extent that they care about the political purity of their reference groups. The only evidence we have is the question asked by Almond and Verba on intra- versus cross-party marriage. In that survey only 4% in each party expressed displeasure, and not too many indicated active pleasure with partisan endogamy (Almond and Verba 1963, p. 135). For the most part, Americans seemed indifferent to this issue. Of course we found that expressions of displeasure were a less effective measure of feelings than was pleasure, and even then its full impact did not emerge until we looked at core groups. Still we can anticipate that this is the crucial point of difference between Americans and Austrians. Even without showing how intensely Austrians feel about the partisan persuasion of those with whom they are

most intimately involved, data which do confirm the patterns already discussed, we have persuasive evidence of the continuity of *Lagermentalitaet*.

CONCLUSIONS

The cleavage structure of Austrian society takes political form through the creation of political parties. The passage of time does not obliterate these connections: in particular, history illuminates continuities within the Socialist and People's parties. History is a much less satisfactory source of explanation for why voters, who have varying degrees of commitment to the continuity of a party organization in itself, remain steadfast in their loyalties to that party. Certainly, once political parties come into being, they provide a significant avenue for reinforcing existing cleavages. But these cleavages, forming the basis of social values and statuses, are subject to pressures of change. Partisan behavior is not immune to these pressures as well. At the same time, it has a degree of independence stemming in part from the nature of the party system and the persistence of party organizations. And despite changes, the cleavages continue to give fuel to partisan stability through their association with specific groups. Historic loyalties, special group interests served by the parties, past or current group experiences—all of these contribute to the linkage between social groups and parties. We have argued, however, that it is the group ties themselves, without any necessary or obvious connection with parties, that are currently operative. That is, loyalty to a group and identification with its members propel individuals in a partisan direction, even when it is not completely evident, at a particular point in time, what the party does for the group. Here, interpersonal ties play a special role in perpetuating partisanship. In the primary group setting, partisan ties are first acquired, membership in status groups established, and salient reference groups continually reinforced. In this sense, primary group relations most directly touch the voter and link him to the political system.

It is our contention that three sets of conditions—those related to the party system, the social structure, and interpersonal networks—operate in all political systems in which there is more than one political party. They can be expected to be most important and prominent in those countries that have a history of deeply rooted cleavages, cleavages that were early translated into the party system. In this sense Austria is but a single case in what should be a more numerous universe. But there are also some very special qualities in Austria. The stability of partisanship there has been accentuated by the relatively even distribution of party support. The coalition imposed by the occupying powers after World War II resolidified party lines, since each had so much to gain from sharing power. But sharing power was not to be allowed to bring about a fusion of parties, and the

institution of the *Proporz* was one of the most effective mechanisms for perpetuating the parties that express the principal cleavages. *Lagermentalitaet*, meanwhile, has also continued. At the time of our survey, attitudes associated with primary group ties were of sufficient strength to be manifested in continued sympathy for sharply differentiated political worlds.

An analysis which stressed Austrian conformity to a traditional model of Marxist class cleavages would only be partially correct. For one thing, it would not anticipate confirmation of Rose and Urwin's thesis on the primacy of religious divisions, as indicated by church attendance. Also unexpected is the more flexible outlook of the SPOe, where *Lagermentalitaet* is confined to fewer voters than in the case of the OeVP. Our survey stems from the period of Josef Klaus's OeVP government, which succeeded the major-party coalition in 1966. In 1970 a Socialist government under Bruno Kreisky was elected. Do our questions on *Lagermentalitaet* anticipate the pragmatic cast of Kreisky's government, a pragmatism that would appear to widen his support while acceptable to his traditional supporters? Kreisky's improved fortunes in the election of 1971 are a tribute to this flexibility. Will this flexibility permit the SPOe to govern until the OeVP matches it?

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The Village Influence on Punjabi Farm Modernization¹

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This study, done in Punjab, India, had three objectives: (1) to evaluate a farmer's personal characteristics (education, land size) in relation to his adoption of modern agricultural technology; (2) to investigate certain village characteristics which are significantly associated with modernized farming; and (3) to determine whether modernization is more influenced by the village as a unit than by individual characteristics. By using the interview-questionnaire method, 495 Punjab farmers were interviewed in 93 villages designated high or low in modernized farming. While education and landholding size (individual characteristics) were positively related to modernization of farming, organizational help and communication (village characteristics) showed a higher correlation with adoption of farming innovations. The primacy of village characteristics over the individual factors was evident. The villages high on adoption of agricultural innovations were also high on other indices of development, such as educational, commercial, and overall development. These villages were also high on the use of bank loans, farming subsidies, and the use of agency help.

INTRODUCTION

The diffusion of innovations and acceptance of new technology, particularly by rural people, has attracted both theoretical and applied interest. Rural sociologists have by now gained a good deal of experience in diffusion studies, and there is a further need for such studies in underdeveloped countries, where the farming is undergoing rapid change and there is pressure to adopt new farming and health technology. Indian farmers, under constant pressure to produce more food and to reduce the number of new mouths to consume that food, are now trying new high-yield varieties of seeds and are using contraceptive devices in their conjugal life. Many studies of modernization among peasants in underdeveloped countries, in villages of Colombia, Brazil, Turkey, Kenya, and India (Rogers 1969), and of innovative agricultural programs in Nigeria (Hursh et al. 1968; Ascroft 1969) have used either the village as a unit of analysis at the macro level, or the individual peasant at the micro level (Saxena 1971).

The present study has three objectives: (1) evaluate the personal characteristics (education and landholdings) of the farmer that are signifi-

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cantly related to his adoption of modern farming technology; (2) investigate those village characteristics which are significantly associated with modernized farming; and (3) determine whether modernization is a function of the village as a unit, and if so, how the village influences individual characteristics.

We hypothesize that modernization is a function of the village, and that it is the global effect of the village unit which reinforces appropriate individual behavior. As Durkheim (1938) has suggested, the group exerts strong pressure on individuals. Those attributes common to the individual members of a group result from this pressure, which may not be apparent to the individuals themselves. A village in India can have a strong influence on the farmer because of the concentrated living arrangements and close-knit relationships. Unlike the American farmer, they do not live on individual farms. Most farmers live in the village, and their small farms surround the village. Living together, they are involved in intense interaction which includes sharing of implements, continuing discussion of agricultural and personal problems, and convivial social relations. An elected assembly of villagers called *Panchyat* performs such functions as settling petty disputes and appropriating and distributing funds for projects of village development. They also serve as a link between the villagers and the Block Development office. (This office is manned by several agricultural extension officers, social education officers, and statisticians, and is headed by a Block Development officer.) The Punjabi farmers do intensive cultivation, and with the recent use of new seeds and fertilizers, are able to grow three crops in a year. Though wheat is the main crop (Punjab being the wheat granary of India), farmers do grow cash crops like cotton, sugar cane, potatoes, peanuts, and fodder. Irrigation is done by canals, but tube wells have fast replaced the old Persian water wheels.

THE METHOD

Twenty inspectors of the Punjab Agriculture Department, who were briefed and trained for the purpose, administered a questionnaire and interviewed eight or more subjects each. All investigators had a fortnightly conference with the senior author to review research criteria and objectives.

Selection of Villages and Respondents

The study was conducted in the central and western part of Punjab, India, where the three districts of Jullundur, Amritsar, and Kapurthala afforded 93 agricultural villages meeting research specifications. Research plans were discussed with the District Agricultural Officers, and they were asked to name a number of villages which were high and a number which were low

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in the adoption of agricultural innovations. As we shall see, the designations by officials as high and low were correct estimates of the modernizing trends of these villages.

Local inspection disclosed that some of the villages designated low in farming were backward due to geographical and physical handicaps. These handicaps include poor irrigational facilities, vulnerability to flood, poor soil, inaccessibility, inadequate agency contact, and location close to the India-Pakistan border where some farmers ignored farming to make quick money by smuggling contraband. While the impact of these physical handicaps was obvious, because the intent of the study was to find sociological explanations for high or low modernization of farming, some villages were included which are about equal in such physical considerations as location, type of soil, irrigation facilities, and accessibility.

There are 38 such villages, with 138 respondents in high adopting areas and 138 respondents from low adopting areas. From 55 villages, 440 respondents were interviewed at an average of eight farmers from each village; and 55 other farmers were interviewed from 38 villages by an itinerant official. Of the 93 villages, 39 were designated as high on adoption of modern agricultural technology and 54 were designated as low. The number of respondents from the high and the low adoption categories was about the same, 240 from the first and 255 from the latter category. In every village they were randomly selected from land records. Most of these farmers were Jat Sikhs, and as such, the sample was homogeneous for religion and caste.

THE INSTRUMENT

The interview schedule contained 130 items on (1) antecedent, (2) intervening, and (3) consequent variables. The antecedent (or independent) variables, which might be called personal characteristics of individual variables, were age, education, landholding size, average yields, and profits. The intervening variables were peer emulation, organizational help, and communication (agency contact, media influence). These interactional factors constitute much of the village influence. With the realization of the influence of peers, respondents were asked: "How many of your friends are extensively using (1) new seeds, (2) fertilizer, (3) pesticides, (4) improved implements, (5) tractors, and (6) birth control measures?" They were also asked if they had tried any agricultural innovations in collaboration with their friends, and what kind of cooperation was being given to them by their farmer friends. Organizational help was the second intervening variable, based on questions about sources of financial help (land mortgage bank, other banks, the village cooperative society, and private sources), and sources of advice (Department of Agriculture). The farmers were also asked

if they attended agricultural demonstrations, lectures, and training camps. Communication was the third intervening variable, consisting of 18 items including visits to the agency, agency help, agency advice, listening to a radio program on farming, seeing a drama or a film, attending a lecture, reading a newspaper, trustworthiness of information, and practicality of information. The coded items were weighted and summed up into a composite score.

The consequent (dependent) variable was use of new technology in agriculture. Information was sought on the use of improved seeds, fertilizers, pesticides, irrigation means (tube wells, waterproof channels), improved agricultural implements, tractors, fodder-cutting machines (driven manually or by power), implements' shed, tractor shed, farm transport, farm cart (with or without rubber tires), and storage facilities. They were also asked if they had developed their own poultry, piggery, dairy, vegetable farm, and fruit garden, and if so, on what scale. Similarly, favorable replies to the following questions added to their farming modernization index: was the farmer a member of a village cooperative society; how many loans had he taken from the cooperative society; how many farming subsidies had he used; how many times did he consult agricultural experts and attend demonstrations and training camps; and how many agricultural magazines did he subscribe to? These items were also weighted and summed up for every farmer individually to give him an index of modernization. The terms "modernization," "adoption of technological innovations," and "development" all carry the same meaning for this study.

With the same instrument, these villages were assessed for their educational, commercial, and cooperative development. Educational development was assessed by the number and length of establishment of elementary schools, junior high schools, and higher secondary schools. If there was a library, it added to the score, depending on how old it was. Civic development included the existence of a recreation center, *panchayat ghar* (assembly center), and civic organizations. Commercial development was measured by the number of shops, small-scale industries, training centers, marketing facilities, and an agricultural supplies outlet. Cooperative development was judged on the basis of the number of cooperative organizations working in the village.

The sample was confined to established farmers, married, with at least two children. This condition was considered necessary for the study in family planning (to be reported elsewhere). Of these respondents, 200 were 36 years of age or younger, and 295 were older than 36 years. Ranked by education, there were 215 illiterates, 35 primary, 90 junior high, 78 high school, and 27 with some college or technical school. For 431 respondents, farming was the sole occupation; 64 either held previously or had currently an additional nonfarming occupation. Residentially, 416 of them had always

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lived in the rural villages, 55 had lived in one of the large cities of India, and 24 had lived abroad. About 13% of them had sons or brothers living abroad.

Economically, marginal farmers (3.0%) had landholdings below 2.4 acres; small farmers (22.3%) had landholdings between 2.5 and 7.4 acres; and subsistence farmers (74.7%) had landholdings above 7.5 acres (Small Farmers' Development Agency 1971). The average landholding was 15.8 acres. This holding included both irrigated and unirrigated land, owned as well as rented. When their average yield was compared with the district average, 60.0% had an above-average yield, 19.6% had an average yield, and 6.7% were below average (no data on 5.7%).²

In agricultural practice, half of them used many varieties of improved seeds; the other half used only two or three. Agriculture is an expensive occupation, and not all farmers can afford heavy use of fertilizer in their fields. Only about 29% of these farmers fertilized their fields three to four times per crop, while the rest applied fertilizer only once or twice. Sprays are also very costly, and as a result 57.5% never sprayed pesticide, although the rest did, if and when needed. Many complained of "bad economics" if their outputs did not equal their inputs into their fields. What seems to be the greatest improvement was in the area of irrigation: 62.2% had at least one tube well; 22.6% had two or three; and only 15.2% had none. Tractors are also coming into use: 23.8% had a tractor and a set of tractor implements, but only 15.8% had a tractor shed.

Only one-third of them had improved their storage. For farm transport, 20% had no transport, 60% had a cart, and 20% had a tractor-trailer or a small truck. For fodder cutting, about 37% used a hand "toka" or a manually operated fodder-cutting machine, 53% operated that machine with bullock, and 9% had a power-operated machine. To reckon by the number of tractors, 23% of the farmers are reasonably well mechanized in their farming. About 15% of the sample of Punjabi farmers have specialized in fruit, vegetable, dairy, poultry, or swine production.

Though 85.5% of them belonged to village cooperative societies, 30% took no loans from these societies, 51% did take some, and 19% got many loans. The village cooperative societies lent money mainly for fertilizers. Village co-ops were the chief source of loans, but there were other sources, such as the central cooperative bank, and the land mortgage bank. More than 37% of the farmers had borrowed money from the land mortgage bank despite the intensive investigation it required. For advice about farming, about half went to an official source, while the other half went to a semiofficial or private source (peer and others). Twenty percent consulted agricultural experts often, 50% consulted sometimes, 30% never. Eleven

² Landholding size was the only indicator of the respondent's socioeconomic status.

percent attended agricultural demonstrations often, 54% sometimes. Eight percent attended training camps often, 37% sometimes. About 9% subscribed to two agricultural magazines, and 13% to at least one journal. This tends to suggest that about 20% of the farmers are quite active in gaining new information.

THE FINDINGS

Education and Modernization

The higher the education, the higher the modernization of agricultural practices (table 1). The better-educated farmers use more varieties of im-

TABLE 1
FARMERS' EDUCATION AND MODERNIZATION IN FARMING

MODERNIZATION INDEX	YEARS OF EDUCATION (IN %)				
	0	1-4	5-8	9-10	11-14
Very low	26	14	5	8	0
Low	26	22	20	14	4
Medium	25	29	22	18	18
High	18	19	29	27	22
Very high	5	16	24	33	56
Total	100	100	100	100	100
N	214	85	91	78	27

proved seeds, use a wider variety of fertilizers and apply them more frequently, spray pesticides when needed, pump up water from their wells mechanically or install new tube wells operated by either diesel or electric motors. More of the educated farmers buy tractors (not easily available in India), and other agricultural implements ($p < .001$). Proper storage both for the produce and the expensive implements is essential, though not all the farmers could afford to build or modify storage facilities. The degree of improvement in storage was correlated to the level of education $r = .46$.

The question is, what is the minimum education necessary to assist in the modernization process? It is suggested that about four years of education marks the modernization "take-off" point.³ Forty percent of the Punjabi farmers in this study were above this literacy watershed. If a farmer could afford to go beyond this take-off point of four to five years of education, then the modernization and education might reciprocate each other, increase in one relaying the increment in the other, thus entering a spiral of progression (Rogers 1969).

³ The UNESCO Standard of four years of education approximates four years of education in India.

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Some innovations were increasingly adopted in exact correspondence to an increase in education. With increasing school years, there was an increase in small-scale poultry ($p < .05$) and dairy farms ($p < .01$) or fruit gardens ($p < .01$). Again, better-educated farmers had more frequent consultation with agricultural experts, attended agricultural camps, witnessed agricultural demonstrations more frequently ($p < .001$), and subscribed to more agricultural journals ($p < .0001$). Similarly, a large majority of educated persons read newspapers or listened to the news and radio programs on family planning ($p < .0001$). Most of the farmers who approached the department of agricultural or family planning workers for advice or help were educated persons. Illiterate farmers either do not ask for help or do not know where to go for help. Educated persons made more use of financial agencies like the land mortgage bank. For advice on farming practices, more of the educated persons tended to go to official agencies; the illiterate or less literate farmers sought advice from their peers.

To a question asking for reasons about their farming profit (high or low), the educated farmers tended to attribute their gains or losses to institutional facilities (land, supplies, loans, market, etc.), while the illiterate praised or blamed their personal efforts ($p < .02$). With increasing number of school years, more and more farmers pursued an additional occupation other than farming ($p < .0001$). Of those who had some college education, half of them were pursuing another nonfarming occupation. More and more farmers are sending their children to school for a longer period of time. Collective education of the family showed a high correlation with the education of the farmer ($p < .0001$).

Land Size and Modernization

Land size, average crop yield, and perceived farm profits are significantly related to the modernization of farming ($p < .0001$) (table 2). Although average landholding comes to about 15 acres per respondent, one-fourth of our respondents had less than $7\frac{1}{2}$ acres of land. Because of increasing pressure on land, the holdings are becoming extremely small.

The marginal and small landholders are too greatly handicapped to make use of the new agricultural technology or to buy new machines, but the Indian government has shown increasing concern for them. Perception of farm profits has a positive correlation with the modernization of farming. Those who perceived better profits had relatively higher farming modernization ($p < .0001$). The actual crop yield was also high for the modern farmer ($p < .0001$). It is a familiar finding in sociology that the poor either do not seek help or are not offered help. The marginal and small farmers get very few loans from the land mortgage banks or subsidies

TABLE 2
RELATIONSHIP OF LANDHOLDERS TO MODERNIZED AGRICULTURE
(%)

Modernization Index	Marginal (Up to 2.49 Acres)	Small (2.50-7.49)	Subsistence (above 7.50 Acres)	Total
Very low	33	22	14	16
Low	20	22	22	21
Medium	33	29	20	23
High	7	25	21	22
Very high	7	2	23	18
Total	100	100	100	100
<i>N</i>	15	111	365	491

NOTE.— $\chi^2 = 34.61$; $df = 8$; $p < .0001$.

from the government, compared to subsistence farmers ($p < .01$). Similarly, the small farmers sought fewer loans from the village cooperative society. This has since been recognized and small farmers' development agencies have been set up to fulfill the special needs of these farmers. Cosmopolitanism of the farmers, significantly related to modernization in previous studies, fell slightly short of the significance level of .05 in this study. Yet it was prominent. About 11% of these farmers had lived in large cities and 5% had lived abroad. Relatively, a larger majority of these cosmopolites had pursued nonfarming professions earlier, but when they farmed, they did better than others. They had higher crop yield ($p < .01$). This fulfills the first objective of the study in confirming that education and landholding positively correlated with the larger usage of modern technology in farming.

Village Characteristics and Modernization

Village characteristics are peer emulation, organizational help, and communication. These village dynamics interact on the farmers' personal characteristics to produce modernization effects. The association of both personal and village characteristics are in evidence in a correlation matrix (table 3).

It is evident that the two village characteristics, organization help and communication, show a higher correlation with modern farming than the individual characteristics. Peer emulation emerges as a relatively weaker factor. If these variables are arranged in order of their correlational strength, and the percentage of the variance explained, the village characteristics have more influence. The determinants of modernization in farming stand in the following order: education $r^2 = .14$, farm size $r^2 = .20$, organizational help $r^2 = .31$, communication $r^2 = .31$).

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TABLE 3
FACTORS AFFECTING FARMING MODERNIZATION
(PEARSON r)

	Education	Farm Size	Peer Emulation	Organized Help	Communication
Farm size28
Peer emulation22	.13
Organized help19	.11	.04
Communication27	.16	.19	.62	...
Modernization38	.45	.28	.56	.56

NOTE.— $N = 495$; $r_{.05} = .09$.

Village Influence on Modern Farming

Our third objective was to determine if adoption of modern farming practices is more strongly influenced by the village as a unit than by any individual characteristics. Table 3 is an indication that the village has more influence. However, this evidence is reinforced when the individual factors are partialled out and the influence of the village characteristics is assessed in relation to modernization (table 4). Clearly, when landholding and

TABLE 4
VARIABLE EFFECTS ON MODERNIZED FARMING

CONTROLLED VARIABLE	FIRST-ORDER PARTIAL CORRELATES WITH MODERNIZED FARMING				
	Education	Farm Size	Peer Emulation	Agency Help	Communication
Education38*	.39	.21	.54	.52
Farm size30	.45*	.25	.57	.55
Peer emulation35	.43	.28*	.57	.54
Agency help34	.47	.31	.56*	.33
Communication29	.44	.20	.32	.56*

* Zero-order correlations are on the diagonal.

education are controlled, village characteristics (agency help and communication) seem to correlate strongly with modern farming. On the other hand, if agency help and communication are controlled, the influence of education and landholding on modernization is greatly reduced. This testifies to the greater influence of the village than that of the farmer's individual characteristics on modernization.

Directional effects between the more influential variables can be estimated by path analysis (Duncan 1967). The path coefficient where i notes the endogenous variable and j the exogenous variable is estimated by the

formula $P_{ij} = s_j/s_i C_{ij}$. The term C_{ij} represents the partial regression coefficient holding a third variable K constant, and is expressed by the formula:

$$C_{ij} = \frac{\sum x_k^2 \sum x_i x_j - \sum x_i x_k \sum x_j x_k}{\sum x_j^2 \sum x_k^2 - (\sum x_j x_k)^2},$$

which applies when the antecedent set includes two variables (Land 1969). In the path diagram (fig. 1), antecedent variables 1 and 2 are farm size and education, respectively.

Variables

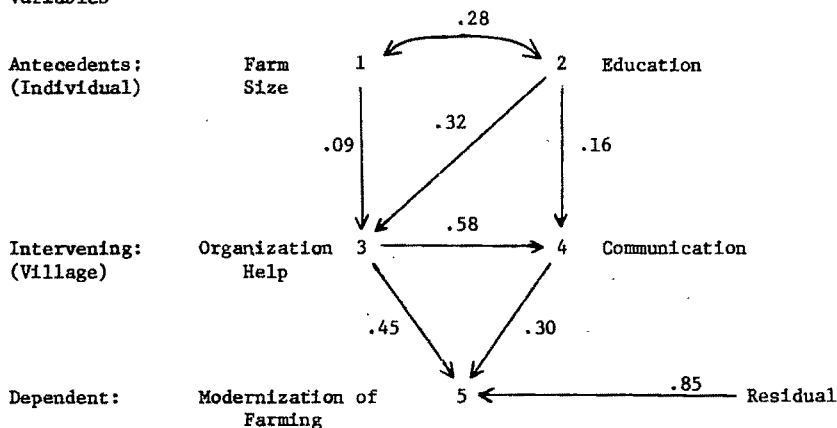


FIG. 1.—Farming modernization path model

Intervening variables 3 and 4 are organizational help and communication (also village characteristics). The dependent variable, 5, is modernization of farming. These variables have previously been shown to be correlated, and the path model hypothesizes the sequence of direct effects. The logical defense of the model is based on the temporal priority of farm size and attained education of the adult farmer over the actual conduct of farming. The farmer depends on loan agencies and specialized sources of information as a precondition to successive steps of modernization regarding mechanization, seed selection, fertilizer programs, and pest control. The results suggest that organizational help is a central factor in bringing modernization about, both in its direct effects on farming practices and in its indirect effects on communication relating to agricultural practice. The model also suggests that the antecedent variables (individual characteristics) are less influential than the intervening variables (village characteristics), and that the participation level is not much affected by variation in farm size if the extremely small holdings are excluded. The magnitude of the residual is large and indicates that the present assessment represents a relatively limited step in identifying the factors which account for variation in modernization of

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agricultural techniques among Punjabi farmers. The collection of time-series data and the application of the Poisson model would permit a more exact analysis of the modernization effects of a series of loans and the effects of publicizing a series of improvements through periodicals and through a sequence of group discussions in the villages (Coleman 1964, p. 288).

What characterizes a village that is modernizing its farms? The Department of Agriculture designated villages high and low on modernization farming. For this study, this designation was preferred over any other method of sampling to bring modernization indicators into clear relief. Our data showed that villages designated high and low on the use of modern farming technology came out as designated by the Department of Agriculture. This not only testifies to the ability of the officials to rate the villages correctly on the basis of their experience, but it gives us the benefit of selecting typical villages, without wasteful efforts, for characterization. It may be noted that villages high on adoption of farming technology are also high on other indices of development, that is, educational, commercial, and other such indices (table 5). Similarly, these villages are high on

TABLE 5
CHARACTERIZATION OF VILLAGES DESIGNATED HIGH AND LOW ON ADOPTION
OF MODERN FARMING

	HIGH ADOPTION (N = 240)		LOW ADOPTION (N = 255)			
	Mean	SD	Mean	SD	t	p
Individual characteristics:						
Farm size	17.2	14.2	15.2	12.4	2.46	.0076
Education	3.2	1.7	2.4	1.7	5.84	.0004
Family's education* ...	23.89	19.05	17.33	11.92	4.62	.0001
Farm profits	High†		Low			.0001
Interactional characteristics:‡						
Agency help	13.0	5.4	9.8	5.0	9.20	.0001
Communication	21.2	4.9	17.2	5.0	11.58	.0001
Peer emulation	21.0	4.5	16.6	4.4	11.39	.0001
Developmental characteristics:						
Modern farming	29.0	12.7	20.1	9.2	14.39	.0001
Educational development (N = 36 and 45)	12.42	7.01	6.20	3.69	4.98	.0001
Commercial development (N = 37 and 49)	8.38	4.51	4.39	3.10	4.64	.0001
Overall development (N = 36 and 48)§ ..	2.16	.54	1.34	.49	2.00	.05
Family planning	25.77	7.16	21.71	8.14	5.88	.0001

* Collective index of the education years of all family members.

† Farm profits as perceived by the farmers. $\chi^2 = 55.52$; $df = 8$, $p < .0001$.

‡ Interactional characteristics and the village characteristics are the same as intervening variables.

§ Composite score of different indicators of development.

educated people, high on the use of the organizational help, bank loans, subsidies, and communication. There is also a higher peer emulation.

How much more influential are the interactional factors than the physical factors? It was earlier pointed out that certain villages suffered from geographical, irrigational, physical, and soil handicaps which could be responsible for retardation in modernization. Since we were more interested in social psychological factors, we eliminated these villages, but even then, a comparison of the remaining villages, after keeping the individual factors constant, still bears out that modernized farming is a function of the village (table 6).

TABLE 6
MODERNIZATION SCORES OF FARMERS IN VILLAGES DESIGNATED HIGH AND LOW
IN FARMING BUT EQUATED IN PHYSICAL CHARACTERISTICS*

Factors Equated	HIGH VILLAGES ADOPTION SCORES			LOW VILLAGES ADOPTION SCORES			<i>t</i>	<i>p</i>
	<i>N</i>	\bar{X}	SD	<i>N</i>	\bar{X}	SD		
A. Education (all illiterate or some literacy) and landholding (all below 15 acres)	62	22.48	10.38	49	19.14	6.18	2.00	.05
B. Education (all above four grades) and landholding (all above 15 acres)	34	36.80	9.96	26	30.26	11.52	1.54	.0001

* The physical factors equated were those of village location, soil type, and irrigational facilities. Villages equated = 38; farmers high villages = 138, low villages = 138.

There is general support for our findings in the literature. In his study of the cultural pattern of the typical progressive farmer, Benvenuti (1962, p. 383) concluded that "this culture pattern (which is oriented to the outside world) has a totalitarian character, since it influences the total behavior of the group concerned, and therefore, its farmer management also." He also found the primacy of the social psychological elements in modernized farming, and particularly "the degree of awareness of his personal position within the structure of which he is a part, and his reference group" (Benvenuti 1962, p. 203). In demonstrating the compositional effect on adoption behavior in the Philippines, Qadir (1966, p. 82) observed that adoption is not just a function of the individual's personal attributes, but also that of the group to which he belongs.

Saxena, in his cross-national study of agricultural innovativeness in India

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and Nigeria, found that system measures were better predictors than individual measures in India (Saxena 1971, p. 27) and to a less degree in Nigeria.

SUMMARY

This study was designed to determine the relationship of the farmer's personal variables and the village characteristics to the adoption of modern farming technology. The second objective was to determine whether adoption of modern technology (modernization) was more strongly influenced by the village as a unit than by any individual attributes of the farmers. A comprehensive questionnaire was administered to 495 farmers in 93 villages of Punjab, India. The villages were designated high or low in modern farming by the agricultural officials.

Two personal variables, education and land size, were positively related with modernization, but such interactional factors in the village as organizational help and communication were more highly correlated with modernization than the individual variables. These village characteristics, being interactional and interventional in nature, bring about an orientation to modernization which replaces traditionalism. When the influence of the individual variables was held constant, the influence of the village characteristics remained unaffected and prominent. The path analysis also displayed the centrality of the organizational help and communication. When the physical and geographical factors, along with individual variables of 38 villages, were equated, village interactional patterns remained most influential.

Villages high on the adoption of agricultural innovations are also high on other developmental characteristics: educational, commercial, and overall development. The highly developed villages are also high on family planning. It appears that as these villages gained an increment in one developmental characteristic, they gained a similar increment in other characteristics.

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Physician Distribution across Metropolitan Areas¹

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This paper examines the effects of demographic and ecological variables on the distribution of physicians in greater American metropolises. The physicians considered are those in full-time, nonfederal private practice in 1966. By disaggregating the total body of physicians into general practitioners and six medical specialty groups it was possible to better specify the factors that should influence their distribution. The communities considered are those 22 SMSAs with a population of over 1 million. Number of physicians is regressed on population size, and the resulting residuals are examined in terms of indicators of four factors thought to influence physician supply. Indicators of three of these factors—medical need, access to medical care, and the presence of alternative sources of medical care—are shown to be associated with the distribution of physician categories considered. The policy implications of these findings and their relevance to ecological theory are discussed.

The fundamental concern of human ecology is twofold: the study of the morphological aspects of communal relations and the factors which give rise to variation therein (Hawley 1944, 1950, pp. 66–74; Schnore 1958). The positional hypothesis which ecology advances to explain community form is that organization arises from the interaction of population and environment (Hawley 1968, pp. 328–37). This general hypothesis has been elaborated by Schnore: "The structure of a given community is viewed as a collective adaptation on the part of the population to its total environment (including other organized population, as well as physical features), an adaptation that is strongly modified by the technological equipment in use and by certain 'purely' demographic attributes of the population itself, notably its size, rate of growth and biological (age-sex) composition" (Schnore 1958).

The analysis of institutional distribution is a specific issue in the study of

¹ This paper was developed from a study of physician distribution supported by a Public Health Service grant from the Regional Science Research Institute, Seattle, and an NIMH demography-ecology traineeship from the University of Washington. (See Barbara Garner [Reskin], "An Ecological Analysis of Physician Distribution in Greater American Metropolises" [master's thesis, University of Washington, 1970]). The authors would like to acknowledge Herbert L. Costner for his comments on an earlier version of this paper.

community form. Institutions can be thought of as agencies of an enduring nature which function to provide solutions to persistent needs of an aggregate. One persistent problem that any population must face is the maintenance of an adequate health level. Consequently, in any community there tends to develop a variety of solutions to the health care problem. Hospitals, public health agencies, and dispensaries for pharmaceuticals can all be seen as part of the institutional response to a population's need for health care. The principal role in the health-care institution, however, is the physician. This paper is concerned with the effect of demographic and ecological variables on the distribution of several categories of medical specialists across greater American metropolises.

The physicians considered here are those in full-time, nonfederal, private practice on January 1, 1966 (American Medical Association 1966, p. 10). In addition to the total number of physicians, data are examined on the number of physicians in general practice and in five of the 34 specialty groups recognized by the American Medical Association. These represent the largest of the medical specialties and include general surgery, internal medicine, obstetrics-gynecology, pediatrics, and psychiatry.² A final category of "other specialties" is considered which includes all those in the remaining 29 AMA-recognized specialties.

The unit of analysis in this study is the metropolitan community. Previous research has demonstrated that the Standard Metropolitan Statistical Area represents a unit large enough to encompass most of the doctor-patient medical exchanges within a particular area (Ciocco and Altman 1954).

This study is limited to those 22 communities with a population over 1 million,³ which have been designated "greater metropolitan areas" (U.S., National Health Survey 1965, p. 3; Pennell and Baker 1964, p. 25). There are three reasons for this limitation. The first concerns the demonstrated relationship between size and specialization. Previous research has shown that as the size of the metropolitan community increases, so also does the proportion of physicians in specialized practice (Pennell and Baker 1964,

² Throughout this analysis the phrase "specialty category" is used to refer to all categories of the dependent variable, including the category of general practitioners. Although recently the American Medical Association recognized a specialty board in general practice, most general practitioners now in practice have not undergone specialty certification. However, we can view general practice as a specialty in the ecological sense that GPs are specialized in function. They deal with a specific set of problems, ordinarily limit themselves to certain medical procedures, and make referrals to medical specialists as necessary.

³ Three of our SMSAs, Dallas, Kansas City, and San Diego, are shown to have populations slightly below 1 million because we are looking at civilian, noninstitutional population only. Because certain health data are reported only for the New York Northeastern New Jersey Standard Consolidated Area and are not available for the three eligible SMSAs contained therein, it was used instead of the SMSA.

p. 25). Since the object of our inquiry is medical specialists, it is useful to take advantage of their relative preponderance in greater metropolitan areas.

Unanswered questions in previous research also direct us to these 1 million plus communities. In Marden's study of physician distribution, community size was controlled using six size categories, the largest of which was SMSA's with a population greater than 1 million (Marden 1960). Using four demographic variables, he accounted for an appreciable proportion of the variance in three dependent variables: total number of physicians, number of general practitioners, and number of specialists. However, after taking into consideration the effects of population size, the largest amount of unexplained variance occurred in cities of over 1 million. With the set of explanatory variables employed here it should be possible to account more adequately for physician distribution across these greater metropolitan areas.

A third reason for focusing on greater metropolitan areas is that they contain over one-third of the U.S. population and almost half the physicians in private practice. Thus, by examining relatively few cases, information can be acquired on the medical institution serving a substantial proportion of the nation's population.

FACTORS AFFECTING PHYSICIAN DISTRIBUTION

There are four categories of factors which should influence the distribution of physicians in private practice: (1) the characteristics of the aggregate which affect the extent of its need for health care, (2) the presence in the community of alternative forms of medical care, (3) the characteristics of the aggregate that enhance or diminish its access to physicians, and (4) the conditions within the community which foster physician functioning.

The Need for Medical Care: Severity of the Problem

Population size.—The size of the aggregate sets limits on the level of the need for medical care within the community. Thus, total size of the population is expected to be related to the distribution of physicians serving that population. Marden has shown that a strong relationship exists between population size and the total number of physicians, the number of general practitioners, and the total number of specialists (Marden 1960, p. 294).

The effects of population size on various categories of physicians as well as the form of the relationships are shown in table 1. The correlation coefficient, r , equals .984 for the relation between population size and total number of physicians.⁴ Thus, size alone accounts for almost 97% of the

⁴ Physician distribution, the variable to be explained in this research, is an aggregate

TABLE 1
RESULTS OF REGRESSION OF PHYSICIAN SPECIALTIES ON POPULATION SIZE

Specialty	<i>r</i>	<i>r</i> ²	Regression Equation
Total physicians984	.968	MDs = -529.8 + 1.36/1,000 pop.
General practice992	.984	GPs = -178.8 + 0.412/1,000 pop.
General surgery983	.966	GSs = -17.53 + 0.115/1,000 pop.
Internal medicine971	.943	IMs = -97.97 + 0.207/1,000 pop.
Obstetrics-gynecology984	.969	For total pop.:
	.990	.981	Ob-Gyns = -12.17 + .094/1,000 pop.
			For women 15-44:
			Ob-Gyns = -20.66 + .500/1,000
Pediatrics967	.935	For total pop.:
	.949	.901	Peds = -25.99 + .077/1,000 pop.
			For children under 5:
			Peds = -56.05 + .845/1,000
Psychiatry949	.901	Psychs = -122.12 + .121/1,000 pop.
Other specialties976	.953	Other specs = -76.57 + .333/1,000 pop.

variance in the distribution of physicians across the 22 greater metropolitan areas. The form of this relationship is best described as a linear function. This linear relation holds for the communities under investigation; however, the negative *y*-intercept indicates that the relationship for all communities is curvilinear. Further, the slope of the regression line implies a substantial physician/population ratio relative to the rest of the country: about 136/100,000 population. In 1962 the average national physician/population ratio for medical doctors in private practice was only 90/100,000.

Population size is also strongly related to the distribution of the specialty groups. The values of *r*² range from .90 to .98, indicating that the distribution of each specialty category is primarily accounted for by the total size of the population. In fact, population size accounts for 90% of the variance in the distribution of practitioners of psychiatry, a specialty which provides medical services to only a fraction of 1% of the population.

Two of the medical specialties examined in this study should respond to specific subpopulations in the community: the distribution of pediatricians should vary with the proportion of children under age five in the population (Fein 1967, p. 31), and obstetrics-gynecology should be especially responsive to the number of women in the childbearing years (U.S., Bureau of Census 1963, pp. 304-27). In table 1 the correlations between the distribution of these specialists and both total population and the subpopulations they serve are presented. The amount of variance explained is slightly increased for obstetrician-gynecologists when the population of women in their childbearing years is substituted for total population. However, the magnitude of the correlation is weaker for the relation of pediatri-

characteristic. Consequently, all correlations reported in this paper are ecological correlations.

cians to their patient population, children under five years, than that to the total population. The inability of patient populations to account for a greater proportion of the variance than total population is not surprising. With r 's this high, the amount of improvement possible is minimal. Moreover, the correlations of population size with both number of women in their reproductive years and number of children under five are so high as to make them almost identical indicators (table 2).

TABLE 2
INTERCORRELATIONS FOR SIZE VARIABLES

	Population	Women 15-44	Children under 5
Population	1.000	.997	.996
Women 15-44	1.000	.991
Children under 5	1.000

Analysis of residual variation.—Our interest in determining the covariance of population size with physician distribution was primarily to determine the unexplained or residual variation, which ought also to be subject to explanation by structural variables. Thus the major goal of this investigation is to employ other demographic and ecological variables in accounting for the variance in physician distribution which is left unexplained by population size. This is accomplished by using the residuals from the regression of the number of physicians in each specialty category on population size as new dependent variables.

Age-sex composition.—One of the factors which can contribute to the extent of a community's health care problem is the proportion of certain demographic groups in the population which are subject to high medical risk. Two age-sex groups have been shown to have relatively high medical care needs. The first of these is the proportion of the population over age 65. The group is characterized by higher than average physician visitation rates (Fein 1967, p. 31) and thus should also be associated with the distribution of physicians. The second compositional variable considered is the proportion of the population made up of women between the ages of 15 and 44. Due to childbearing, these women have special medical needs and their presence in the community should be associated with the distribution of physicians. The correlation between these two independent variables is .27 (table 3).

Both the correlation coefficients and regression coefficients suggest that the proportion of women in their childbearing years influences every category of medical specialty. This indicator alone accounts for between 5% and 16% of the residual variation in physician distribution. The effect of

TABLE 3

ASSOCIATION BETWEEN TWO INDICATORS OF MEDICAL NEED AND PHYSICIAN RESIDUALS
AS INDICATED BY PEARSONIAN r AND THE UNSTANDARDIZED REGRESSION COEFFICIENT

SPECIALTY	PROPORTION WOMEN 15-44		PROPORTION OVER 65	
	r	b	r	b
Total physicians306	126.06	.274	132.79
General practice278	24.44	.452	46.74
General surgery369	13.08	.264	11.01
Internal medicine263	22.32	.216	21.56
Obstetrics-gynecology408	11.32	.091	2.97
Pediatrics251	8.39	.113	4.45
Psychiatry285	18.94	.205	16.03
Other specialties220	26.86	.206	29.59

the proportion of the population over age 65 is less consistent across specialty categories, although the positive relation is in the expected direction. It is interesting to note that the value of r is particularly high for the relation of old people and general practitioners. This might be due to the relatively lower fees of GPs as well as to the fact that the trend toward specialization and away from general practice was established after these older patients had established their medical preferences.

Community health level.—Community health level should be related to physician distribution. Unfortunately we are unable to predict the form that this relationship should assume. Employing the concept of demand we would expect that an unhealthy population would attract medical personnel, yielding a positive relationship between physician distribution and level of morbidity within the community. On the other hand, a strong medical presence should serve to reduce the extent of sickness within the community. This would result in a negative relationship between physician distribution and community health level. Indeed, we feel that both pressures are likely to operate simultaneously: theoretically, unhealthy populations create a demand for health personnel whose activities in turn should decrease the level of morbidity. If, in fact, both pressures are operating, the relationship actually observed will be either positive or negative depending on which one predominates at a given point in time. In either case, the observed relationship would stand as an underestimation of the predominant process. Thus, as we turn to an analysis of the data, we are more interested in the direction than the magnitude of the relationship between community health level and physician distribution.

The selection of a measure of community health presents special problems since no single health index currently exists despite attempts in this direction (U.S., National Center for Health Statistics 1966). Three separate measures of community health developed by the National Health Survey are

used in this study (U.S., National Health Survey 1965). The three indicators are: (1) days of bed disability per person per year for all ages; (2) days lost from work per currently employed person per year for those 17 years old and older; and (3) number of acute conditions per 100 persons per year for all ages. Their intercorrelation are shown in table 4.

TABLE 4
INTERCORRELATIONS FOR HEALTH VARIABLES

	Bed Disability	Work Days Lost	Acute Conditions
Bed disability	1.000	.205	.512
Work days lost	1.000	.024
Acute conditions	1.000

The correlation and regression coefficients between physician distribution and our measures of morbidity shown in table 5 reveal a moderate, positive relationship between all categories of physicians and the three measures of community health. General practitioners are most highly correlated with community health level followed by the total number of physicians and then

TABLE 5
ASSOCIATION BETWEEN THREE INDICATORS OF LEVEL OF HEALTH AND PHYSICIAN
RESIDUALS AS INDICATED BY PEARSONIAN r AND THE UNSTANDARDIZED
REGRESSION COEFFICIENT

SPECIALTY	DAYS OF BED DISABILITY/100		DAYS LOST FROM WORK/100		ACUTE CONDITIONS/100	
	r	b	r	b	r	b
Total physicians291	2.58	.244	2.79	.283	7.06
General practice354	0.67	.240	0.59	.426	2.27
General surgery234	0.18	.255	0.25	.105	0.23
Internal medicine184	0.34	.229	0.54	.187	0.96
Obstetrics-gynecology139	0.08	.288	0.22	.137	0.23
Pediatrics240	0.17	.270	0.25	.129	0.26
Psychiatry153	0.22	.247	0.46	.182	0.73
Other specialties348	0.91	.141	0.48	.300	2.22

pediatricians. Since the relationships are positive we conclude that the "attraction" of medical doctors to morbidity is stronger than their "effect" on health level. This tendency for supply to respond to demand might suggest a stabilizing system and the eventual movement toward an equilibrium state. If this is true, we would expect that, in the long run, assuming no major distortions from other factors, large American metropolises will tend toward homogeneity in health level and physician supply. A longi-

tudinal analysis would be essential to understand the development of such a process.

Thus far we have been assuming that the effect that an adequate physician supply would have on our measures of morbidity would be suppressive. However, in some cases this may not be true. Note the positive relationship between surgeons and days of bed disability and days lost from work. While these events may reflect a need for surgery, they also occur as an aftermath of surgery. Hence, it may be plausibly argued that the positive associations between the distribution of surgeons and these two measures of community health are a function of medical performance as well as medical demand. Notwithstanding our inability to unravel this last finding, community health stands in an important relationship to physician distribution.

The Need for Medical Care: Functional Alternatives

A second category of factors related to physician need within a population is the presence of functional alternatives to the physician in private practice. It is our expectation that the distribution of physicians in private practice will be negatively associated with the number of physician surrogates in the community. Two important alternative sources of medical care are considered here. The first is care provided in hospital emergency rooms and outpatient clinics, operationalized as the number of interns and residents per 100,000 population (Stewart and Pennell 1960, p. 189). The number of residents and interns is selected as an indicator of physician surrogates because they are known to provide a substantial proportion of the medical services which the public receives in outpatient care (Peterson and Pennell 1962, p. 3). A second functional alternative to physicians in private practice is the number of doctors of osteopathy per 100,000 population (Pennell and Baker 1965, p. 28). While the number of doctors of osteopathy exhibits a wide range of variability across the metropolitan areas in our sample, in some locales the osteopath/population ratio is substantial. Indeed, in one of the SMSAs examined in this analysis, it represents almost one-quarter of the physician/population ratio. The correlation between doctors of osteopathy and number of residents and interns is $-.04$.

Table 6 reveals that doctors of osteopathy are negatively related to physician distribution, as expected. This negative association is strongest for pediatrics, with psychiatry and internal medicine second and third. While somewhat lower in magnitude for the remaining specialty categories, it persists across all groups except for general practice and "other specialties" where it is virtually nonexistent.

The failure of a relation to emerge between doctors of osteopathy and "other specialties" is understandable. Specialists in esoteric areas are rarely

TABLE 6

ASSOCIATION BETWEEN TWO INDICATORS OF ALTERNATIVE MEDICAL CARE AND PHYSICIAN
RESIDUALS AS INDICATED BY PEARSONIAN r AND THE UNSTANDARDIZED
REGRESSION COEFFICIENT

SPECIALTY	DOCTORS OF OSTEOPATHY/100,000		RESIDENTS AND INTERNS/100,000	
	r	b	r	b
Total physicians	-.144	-1.30	.241	22.24
General practice	-.044	-0.08	.047	0.93
General surgery	-.148	-0.11	.425	3.37
Internal medicine	-.252	-0.47	.265	5.04
Obstetrics-gynecology	-.133	-0.08	.309	1.92
Pediatrics	-.333	-0.24	.274	2.05
Psychiatry	-.260	-0.38	.323	4.81
Other specialties016	0.04	.147	4.02

in competition with osteopaths who provide more routine medical services. Moreover, it is expected that patients are usually referred to these specialists. The lack of a relation between osteopaths and GPs is more difficult to explain. Perhaps osteopaths are viewed by much of the medical consuming public as a middle-range specialist who is to be consulted when the skills of a general practitioner are not deemed likely to suffice. Research in the characteristics of patients of doctors of osteopathy could shed light on this phenomenon.

The positive relationship between residents and interns and the distribution of physicians in private practice is contrary to our expectation. Research on the decision-making process in which physicians engage when selecting a location to set up practice provides a clue to understanding the direction of this association. A study of North Carolina physicians showed that a majority completed residency or internship in that state (Powell 1967). This tendency for a physician to locate where he was trained may be based on the importance of professional contacts made during medical training for establishing private practice. If physicians tend to locate their practice in the area in which they completed their medical education, the positive association between interns and residents and the distribution of physicians is not difficult to understand.⁵ This interpretation is supported by the low correlation between number of residents and interns and the number of general practitioners, as general practice is the only category of the dependent variable in which residency is not required. General practitioners are thus freer to leave the area in which they served their internship since they are less dependent on professional referrals (Last 1967).

⁵ This explanation was suggested by Dr. Frank Newton, director, Group Health Hospital, Seattle.

The Need for Medical Care: Access to Physicians' Services

Education and income.—Just as there are certain factors which contribute to physician need, so are there other factors related to the ability of a population to actively utilize and thus create a demand for the services of physicians in private practice. Although they do not determine actual medical need, education and income have been shown to be associated with physician use rates (U.S., National Center for Health Statistics 1964, p. 13). This is because these two variables represent knowledge of medical facilities and ability to purchase medical care, both of which are necessary for access to the services of doctors in private practice. We therefore argue that the distribution of physicians should vary directly with aggregate education and income levels.

Median number of school years completed for people over age 25 is used as a measure of educational level (U.S., Bureau of Census 1967, pp. 433 ff.). The National Center for Health Statistics has shown that people with at least one year of college have on the average one and one-half more physician visits a year than people with only elementary school education (U.S., National Center for Health Statistics 1964). Moreover, educational attainment is associated with the purchase of health insurance (U.S., National Center for Health Statistics 1964).

Total personal income after taxes is used as a measure of ability to purchase medical services (Pennell and Baker 1965, pp. 28 ff.). Inclusion of this measure recognizes that members of low- and high-income families differ in their average annual use of doctors by almost one office visit (U.S., National Center for Health Statistics 1964, p. 13).

The effects of education and income on physician distribution can be seen in table 7. Educational level is positively related to every medical specialty with the exception of obstetrics-gynecology. The values of r range

TABLE 7

ASSOCIATION BETWEEN THREE INDICATORS OF ACCESS AND PHYSICIAN RESIDUALS AS INDICATED BY PEARSONIAN r AND THE UNSTANDARDIZED REGRESSION COEFFICIENT

SPECIALTY	EDUCATION		PER CAPITA INCOME		PROPORTION NONWHITE	
	r	b	r	b	r	b
Total physicians321	311.14	.208	0.79	— .126	—16.25
General practice371	76.73	.225	0.18	—3.85	—10.59
General surgery210	17.52	.090	0.03	— .098	—1.09
Internal medicine197	39.33	.159	0.12	— .034	—0.90
Obstetrics-gynecology001	0.07	.006	0.00	.270	2.35
Pediatrics199	15.66	.173	0.05	.092	0.96
Psychiatry204	31.90	.101	0.06	— .030	—0.62
Other specialties452	129.86	.299	0.34	— .164	—6.27

from about .20 for general surgery, internal medicine, and psychiatry to .45 for "other specialties." The strong association between aggregate educational level and "other specialties" probably reflects the increased knowledge required to utilize the less well-known services offered by the diverse specialists in this category. The fairly high correlations for total number of physicians and number of general practitioners are in keeping with the hypothesis that greater median education leads to increased utilization of private doctors.

Table 7 shows that education is more highly correlated with physician distribution than is income. Income level is most closely related to the distribution of "other specialties," total physicians, and general practitioners. Of most interest, perhaps, are the two specialties least related to income level: general surgery and obstetrics-gynecology. These represent two areas of medicine where demand is least elastic: women who are pregnant need some minimal amount of obstetrical care, regardless of their level of income. (Note that this explains the zero relationship between education and obstetrics-gynecology as well.) This is also true of surgery. Although people may put off some types of surgery because they cannot afford it (thus, we have the notion of "elective" surgery), frequently this is not possible. Therefore, for the most part, the demand for surgery is more inelastic than that for general medical care or medical "luxuries." Thus, it is reasonable that the "access" variables should show little to no relationship to the distribution of inelastic specialties.

Race.—Race is also related to physician-use differentials: whites have a physician visitation rate of 4.7 visits per year compared to 3.3 for nonwhites (U.S., National Center for Health Statistics 1964). The nonwhite segment of the population is thought to be at a disadvantage with regard to both knowledge of and ability to afford medical services. Moreover, nonwhites are believed to experience an additional disadvantage in their access to physicians in private practice beyond that accounted for by education and income (Marsden 1966). Elesh and Schollaert, in a recent study of race and urban medicine in Chicago, found a substantial effect of race on physician location, after education and income were taken into account (Elesh and Schollaert 1971). The intercorrelations between education, income, and race are presented in table 8.

TABLE 8
INTERCORRELATIONS FOR ACCESS VARIABLES

	Education	Income	Proportion Nonwhite
Education	1.000	.570	— .271
Income	1.000	— .041
Proportion nonwhite	1.000

The expected negative relations between proportion of the population nonwhite and specialty category distributions are shown in table 8. The strongest association occurs for the distribution of general practitioners, confirming an earlier finding by Marden (1960, p. 299). While the relation between proportion nonwhite and total number of doctors is not high, this is obviously lowered substantially by the strong positive relation for obstetrician-gynecologists—the most interesting finding in this set of correlations. This result is also interpretable in terms of the relative inelasticity of the demand for obstetrical care.

These findings relating physician distribution to indicators of access to medical care have important meaning for social policy which is discussed in the conclusions.

The Presence of Medical Care: Factors Promoting Medical Functioning

Community features which attract and hold doctors should be related to physician distribution. In his work on physician distribution, Marden (1960, p. 298) suggested that the unexplained variation he found in the distribution of specialists could rest with environmental factors other than hospital beds, which for him was a particularly important variable in accounting for the distribution of all specialists. Therefore we are retaining hospital beds, the importance of which should vary depending upon the characteristics of the medical specialty considered, and including number of medical students as well. The latter indicates the scope of medical training facilities within a metropolitan area. The variety of opportunities which such facilities offer physicians in private practice is thought to constitute an important professional resource. The correlation between number of hospital beds and number of medical students is $-.05$.

TABLE 9

ASSOCIATION BETWEEN TWO INDICATORS OF FEATURES PROMOTING AN ACTIVE MEDICAL COMMUNITY AND PHYSICIAN DISTRIBUTION AS INDICATED BY PEARSONIAN r AND THE UNSTANDARDIZED REGRESSION COEFFICIENT

SPECIALTY	HOSPITAL BEDS/ 100,000 POPULATION		NUMBER OF MEDICAL STUDENTS	
	r	b	r	b
Total physicians161	16.98	-.260	-.033
General practice182	4.10	-.120	-.003
General surgery173	1.57	-.342	-.004
Internal medicine201	4.37	-.292	-.008
Obstetrics-gynecology057	0.41	-.132	-.001
Pediatrics159	1.36	-.211	-.002
Psychiatry132	2.25	-.197	-.004
Other specialties086	2.69	-.292	-.011

Table 9 presents the correlation between the number of hospital beds and the distribution of physicians. While in the expected direction, most of the correlations are low. Marden's findings are relevant here: while the number of hospital beds was important for the distribution of physicians in his smallest metropolitan areas, it became progressively less important as the size of the metropolitan area increased (Marden 1960, pp. 297-98).

The number of medical students is shown to be negatively associated with physician distribution. In selecting this variable as an indicator of the scope of medical training facilities we reasoned that their presence would provide various professional opportunities, such as part-time teaching, research facilities, and continuing medical education through workshops and special programs. We first examined the relation between number of medical schools and physician distribution, but there was so little variation in number of medical schools that the results were useless. We then decided on a more indirect measure, number of medical students, in an attempt to increase the variation. The findings suggest that this substitution fails to operationalize those professional opportunities which we believe medical schools provide to local doctors. A more direct measure, size of medical school faculty, might reveal the exact relation. A second indicator, magnitude of in-service training programs, while not available in published data, would permit the validity of this reasoning to be tested.

While our first interest is in whether a good measure of professional amenities can be developed, we are curious as to the cause of the negative relation observed between number of medical students and number of physicians. One explanation may be that the observed relation is artifactual, since a disproportionate number of medical students are trained in the east for historical reasons, and it is the eastern SMSAs which are most likely to have shortages of physicians (Garner 1970, p. 53).

EXPLAINED VARIATION IN PHYSICIAN DISTRIBUTION

When the effects of the 12 variables are combined, they are found to account for an appreciable proportion of the residual variation in physician distribution after the effect of population size is removed. As table 10 reveals, together these variables account for over half of the residual variation in the distribution of all physicians. The amount of residual variation explained is even higher for the specialty categories of surgery, pediatrics, general practice, and internal medicine. The distribution of obstetrics-gynecology and "other specialists" is least amenable to prediction, but even for these almost half of the residual variation is accounted for.

Returning now to the original question concerning the total variation in physician distribution, table 11 presents the results of multiple correlation

TABLE 10
MULTIPLE R INDICATING ASSOCIATION OF 12
INDEPENDENT VARIABLES WITH PHYSICIAN
RESIDUALS

Specialty	R	R^2
Total physicians735	.540
General practice777	.604
General surgery849	.721
Internal medicine752	.565
Obstetrics-gynecology692	.479
Pediatrics778	.605
Psychiatry715	.512
Other specialties695	.483

analysis when all the variables including population size are considered together. In every case they account for essentially all of the variation in the distribution of physicians and specialists across greater American metropolises.

TABLE 11
MULTIPLE R INDICATING ASSOCIATION OF ALL 13
INDEPENDENT VARIABLES INCLUDING POPULATION
SIZE WITH PHYSICIAN DISTRIBUTION

Specialty	R	R^2
Total physicians997	.994
General practice998	.996
General surgery998	.997
Internal medicine995	.991
Obstetrics-gynecology996	.992
Pediatrics996	.991
Psychiatry987	.974
Other specialties992	.983

SUMMARY AND CONCLUSIONS

This study has examined the effect of demographic and ecological variables on the distribution of physicians in private medical practice across greater American metropolises. The variables employed were selected to indicate four factors thought to influence physician supply. Indicators of three of these four factors—medical need, access to medical care, and the presence of alternative sources of medical care—were shown to be associated with the distribution of both the total number of physicians as well as physicians in each of the seven medical specialty categories, in the predicted direction. The indicators of the fourth factor—features promoting physician functioning—were either uncorrelated with physician distribution or correlated in a direction opposite to that predicted. By disaggregating the body

of medical specialists into six medical groups it was possible to extend our knowledge of physician distribution beyond that gained in earlier studies.

Policy implications can be drawn from this analysis, based on findings which direct medical economists to aggregate characteristics shown to be associated with high physician need or use. Of particular relevance are those findings regarding the importance of the socioeconomic variables (race, education, and income) on the distribution of physicians in private practice.

Cities with a population characterized by lower median education or income have proportionately fewer medical doctors in private practice than cities with a more "favorable" population profile. Our comments about education and income will be brief, however, since the issue of making policy inferences based on the impact of such variables on some socially desired goals has been carefully discussed in the literature (Cain and Watts 1970; Coleman 1970; Aigner 1970). Nevertheless, it is useful to point out here that in developing policy, we cannot be content with looking at correlation coefficients. Although these coefficients indicate the strength of the relationship between our independent variables and the distribution of physicians, it is not clear that they can be interpreted as describing the importance of some predictor variable for the distribution of the dependent variable. For this reason we have included unstandardized regression coefficients (b 's) in the tables showing the zero-order association between the independent variables and physician residuals. The regression coefficients enable us to discern the amount of change in one of these socioeconomic measures which would be necessary to effect some sought-after increase in the number of private doctors. Thus, according to the b 's in table 7, either an increase of about 0.3 years of median education or an increase of approximately \$125 of median income would bring about an increase of 100 physicians (with population size held constant). Knowledge of the values of these b 's enables us to evaluate our ability and willingness to sustain the costs of increasing the number of doctors at the aggregate level.

Proportion nonwhite was also shown to be negatively associated with the number of physicians in private practice. These data, of course, do not mean that, in cities with a low physician/population ratio, it is necessarily the black community that receives the least contact with private physicians. It could be that in such cities doctors are spread evenly throughout the population, affording equal access to all citizens. However, research findings indicate that physicians in private practice are seldom found in those neighborhoods which are characterized by high proportions of the poor or black (Marsden 1966; Elesh and Schollaert 1971). Thus policy that applies the concepts of medical need and access to facilities at the local (neighborhood) level may contribute to the solu-

tion of aggregate-level physician shortage observed in the ecological correlations reported here.

If the above supposition is correct, it suggests that programs directed at improving the health care of the lower socioeconomic segments of the population should respond to two sets of conditions. First, when large areas of a community (e.g., the black ghetto) are characterized by low income and education, the residents may have had little experience in dealing with medical practitioners. Programs that increase the citizens' awareness of their own medical needs as well as the availability of medical facilities may be required. This might be accomplished through programs that actually enlist the aid of the local neighborhoods in establishing and administering medical programs such as the federally funded Model Cities program. Second, the problem of access to facilities might be dealt with through public policy that encourages private physicians to locate in areas with a high unmet physician need. This might take the form of some kind of differential reward system, such as tax incentives. Alternatively, the problem of access to facilities might be dealt with through policy aimed at locating public hospitals and clinics in areas with a low proportion of physicians in private practice.

The theoretical relevance of these findings lies in the contribution they make to validating general ecological principles. Conceptualizing institutions as agencies which arise to meet aggregate needs, it was possible to account for the variation in physician distribution by focusing on the characteristics of the population itself as well as other organizational aspects of the community. In so doing, some evidence is provided for the general ecological hypothesis that organization arises as a result of the interaction of population and environment.

This evidence, however, must be considered to be of an indirect nature. For the view that organization constitutes the collective adaptations of a population to its total environment is the statement of a dynamic relationship. The validity of such a relationship cannot be established using the type of static cross-sectional research design employed in this study. In order to provide more meaningful tests of ecological principles, attention must increasingly turn to longitudinal studies of community structure. Thus a more rigorous test of ecological principles would involve investigation of the following question: to what extent do changes in the biosocial characteristics of a population or in the activities of various medical care agencies bring about a change in the number of physicians within a particular community?

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Commentary and Debate

COMMENTS ON JENSEN'S "PARENTS, PEERS, AND DELINQUENT ACTION"

In a recent paper Jensen (1972) reports the results of an empirical test of two hypotheses derived from Sutherland and Cressey's 1966 formulation of differential association theory. He tests the extent to which peer delinquent associations affect the impact of family interaction on delinquent involvement, and the extent to which delinquent attitude mediates the impact of delinquent associations on delinquent involvement. My comment is restricted to the latter hypothesis.

Concerning the study design, the sample consists of 1,588 junior and senior nonblack high school males drawn from the 1965 Richmond Youth Study. Delinquent involvement was measured in terms of a self-report inventory; delinquent associations were measured in terms of the number of close friends reported by the respondents as picked up by the police; and delinquent attitude was measured in terms of the respondents' evaluations of the police and the law.

To test the latter differential association hypothesis, Jensen first computed the zero-order gamma coefficients between the three variables (see table 1). Consistent with other past findings, the results show moderately strong positive relationships between the variables. However, as Jensen states, the critical test is the extent to which the relationship between delinquent associations and delinquent involvement is maintained when delinquent attitude is controlled. That is, if delinquent attitude mediates the relationship between delinquent associations and delinquent involvement, after controlling for delinquent attitude, the latter relationship should reduce to approximately zero. To partially control for the effects of delinquent attitude, Jensen computed the correlation between delinquent associations and delinquent involvement at three different levels of delinquent attitude. For respondents with an antidelinquent attitude the correlation is .49; for respondents with a neutral attitude the correlation is .43; and for respondents with a prodelinquent attitude the correlation is .47. As these correlations are not too different from the correlation of .60 between delinquent associations and delinquent involvement when delinquent attitude is not controlled, Jensen concludes that delinquent associations have an independent effect on delinquent involvement. He states: "However, it is clear that the effect of the number of delinquent friends on delinquency is not solely a product of socialization into competing normative standards" (p. 568).

While Jensen's analysis is certainly a step in the right direction, his logic

TABLE 1
ZERO-ORDER GAMMAS BETWEEN DELINQUENT ATTITUDE, DELINQUENT
ASSOCIATIONS, AND DELINQUENT INVOLVEMENT

	Delinquent Attitude	Delinquent Involvement
Delinquent associations43	.60
Delinquent attitude38

is subject to considerable criticism. The fact that the data are strongly inconsistent with the socialization (differential association) causal structure does not support the contention that delinquent associations have an independent effect on delinquent involvement. Numerous other causal structures are equally consistent with Jensen's data.¹

Concerning just chain causal structures, consider the theoretically plausible structures in table 2. The structures are labeled in terms of the

TABLE 2
DISCREPANCIES BETWEEN GENERATED AND OBSERVED COEFFICIENTS FOR SIX
CAUSAL STRUCTURES

EXPLANATIONS	CAUSAL STRUCTURES*	COEFFICIENTS		Dis- CREPANCIES
		Observed	Generated	
Socialization-attitude impact	D → A → I	.60	.16	.44
Rationalization-social selection	I → A → D	.60	.16	.44
Social reward- rationalization	D → I → A	.43	.23	.20
Attitude impact-social selection	A → I → D	.43	.23	.20
Social selection-social reward	A → D → I	.38	.25	.13
Social selection- socialization	I → D → A	.38	.25	.13

* D = delinquent associations; A = delinquent attitude; I = delinquent involvement.

theoretical explanations employed to account for the variable linkages. Five explanations are specified: socialization, attitude rationalization, social selection, social reward/cost, and attitude impact. Socialization refers to a process whereby attitudes are learned in group interaction; attitude rationalization refers to a process whereby attitudes are formed as an effort to rationalize behavior; social selection refers to a process whereby

¹ For a discussion of the problems of causal analysis in testing differential association theory, see Liska (1969).

associations are selected so as to maximize attitude or behavior similarity; social reward/cost refers to a process whereby behavior is constructed so as to maximize social rewards and minimize social costs; and attitude impact refers to the effect of attitude on behavior. For example, in the first causal structure delinquent attitude is conceptualized as an outcome of delinquent associations, and delinquent behavior is viewed as an effect of delinquent attitude; and in the third causal structure delinquent involvement is viewed as an effort to maximize social rewards and minimize social costs, and delinquent attitude is conceptualized as an effort to rationalize delinquent involvement. Each of these five processes or explanations has been reasonably well documented in empirical research so as to make each of six causal structures empirically and theoretically plausible.

Now, if any one or several of these causal structures is capable of accounting for Jensen's data, then the zero-order coefficient between the first and last variables should approximate the zero-order coefficient between first and mediating variables multiplied by the zero-order coefficient between the mediating and last variables. Or as previously stated, if the causal structure is empirically plausible, then controlling for the mediating variable, the relationship between the first and last variable should approximate zero. In table 2, for each causal structure we have stated the observed and theoretically generated coefficients, and the discrepancy between them. (Of course, in terms of this analysis the first and second, the third and fourth, and the fifth and sixth causal structures are not empirically identifiable or distinct.) The figures show a discrepancy between the observed and theoretically generated coefficients for all of the structures with the social selection-social reward/cost and social selection-socialization structures showing the lowest discrepancy.

On the basis of this analysis I am not implying that these two causal structures are suggested by Jensen's data. Rather, of the six chain causal structures tested, these two provide the best fit to the data. The point is that a poor fit between the data and the differential association causal structure (socialization-attitude impact model) does not by itself suggest any other model, as Jensen implies. That is, it does not suggest that delinquent associations have an independent effect on delinquent involvement. To accept this inference based on Jensen's data, it is logically necessary to assume also that other causal models (such as the five other models specified above) are theoretically and empirically unworkable. Yet, as we have attempted to argue in specifying five other chain causal models, there is no theoretical or empirical reason to make such an assumption, that is, to assume that delinquent associations and delinquent attitudes must temporally precede delinquent involvement.

To decide between Jensen's model and various of the chain models, clearly other data are needed. However, as I have stated elsewhere (Liska

1969), the solution may not lie in deciding which model is empirically correct, but in discovering the conditions for which each model is most applicable.

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JENSEN REPLIES

I agree completely with Liska that there are a number of causal models consistent with the findings he cites, as is the case with other relationships examined in the study as well. I noted quite early in the paper that "since the hypotheses to be tested in this analysis treat conditions of family life as independent variables and delinquency as the dependent variable, our analysis and interpretations are in terms of that assumed causal ordering," and that "similarly, when we test certain notions derived from the differential association perspective, the interpretation is in terms of the ordering suggested by such theorists. Again, it should be recognized that, even where the associations suggest that certain relationships do exist, the causal direction may be quite different from that assumed by a particular theorist" (p. 563, n. 3). Obviously, I was not excluding the possibility of alternative models, logically or otherwise. It appears I should have reiterated the point by qualifying my statement on page 568 to read: "Assuming the causal ordering suggested by Sutherland and Cressey, it is clear that the effect of the number of delinquent friends on delinquency is not solely a product of socialization into competing normative standards." I had hoped that my earlier footnote would make it clear that I was not ruling out the alternatives which Liska has so concisely delineated.

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A LETTER FROM EVERETT HUGHES

Dear Colleagues:

The sociological generations will be grateful to you for publication of the two wonderful documents in the September 1973 (vol. 79) issue, "The Life Histories of Thomas and Park." Since they will be frequently quoted, it is especially important that the three errors in German spelling be corrected. The errors are these:

- P. 255: *Masse und Publicum* should be *Masse und Publikum*. *Munsterberg* should be *Münsterberg* (as you have it twice on p. 256).
P. 256: *Gesselshaft* should be *Gesellschaft*.

EVERETT C. HUGHES

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Review Essay

Strategic Interaction. By Erving Goffman. Philadelphia: University of Pennsylvania Press, 1969. Pp. x + 145. \$2.95

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University of California, Berkeley

Erving Goffman is a great ethnologist. His tribe is mankind, his focus the face-to-face relations of everyday life, his method of observation a combination of naturalistic fieldwork and a wide reading in history, biography, manners, social science, and literature. He also has a point of view and a style of exposition appropriate to it.

In his preface to *Strategic Interaction*, Goffman repeats a statement of purpose found in his earlier monographs: "My ultimate interest is to develop the study of face-to-face interaction as a naturally bounded, analytically coherent field—a sub-area of sociology" (p. ix). To that end, he puts interpersonal relations rather than role relations at the center of his analysis: he traces out the morphology of social relations insofar as they turn on people's being able to perceive and to effect relevant dispositions of others and themselves, *whether or not those dispositions happen also to be rights or duties*. What distinguishes Goffman from most other students of "person perception" is his awareness that, however slightly, interpersonal relations are also role relations and they are embedded in a framework of normative constructs (constructs that are social in origin and socially sanctioned). Following Mead, Goffman believes that this framework makes interpersonal relations possible and that it also helps to shape them.

Goffman is further distinguished by the role he gives to the pursuit of self-interest. However much people seem devoted to roles and role relations, social interaction is always importantly interpersonal simply because participants enter, maintain, or reject it for personal as well as for collective ends. The portrayal of interaction as a process that occurs both because of these conflicting interests, and in spite of them, is a hallmark of his work.

The essentials of Goffman's approach appear in his first monograph, *The Presentation of Self in Everyday Life* (Garden City, N.Y.: Doubleday, 1959). First, he reminds us that human interdependence, hence social interaction, is ubiquitous and inescapable. We are shown that, however banal or routine it may be, interaction is always intelligible as an expression of universal characteristics of human nature and social interdependence. Goffman underscores this point by dissecting a series of formalistic gestures, showing each to conform to an elaborate social logic. Then he advises us to look more closely. Participants in a social relationship may depend upon it and upon one another, but they have other interests as well. As a result, they arrange their contacts to conceal these competitive

interests. Of course, everyone knows what is going on! And knows that the others know. And each helps the others to keep up appearances by pretending ignorance, even to the point of "not noticing" failures at concealment. Goffman drives home the extent to which the structure of all social interaction and all social arrangements derives from this conspiracy. He exposes and convicts on every page.

The same sort of argument appears in *Strategic Interaction*. In the first of the two essays that comprise this book, "Expression Games, an Analysis of Doubts at Play," we are told that face-to-face interaction is a fact and that some of it entails communication: "the intentional transmission of information" (p. 5). But this is never the whole story. To find what else there is, consider those situations in which people must make calculations about themselves or others in the absence of adequate communication, especially situations in which people think it is unwise to take words at their face value. Is there any situation in which we depend solely on communication in making our judgments? Probably not. If that is so, how do we manage? The question is familiar. Goffman has raised it before. So have students of the perception of persons. So has Harold Wilensky in his book on *Organizational Intelligence* (New York: Basic, 1967). What Goffman adds is the appreciation that the question is relevant for all of social interaction: we are all and always spies and spied upon. If so, espionage and counterespionage and counter-counterespionage must be important in structuring all interactions and are not just ad hoc procedures. That is the point that Goffman elaborates.

In the second of the two essays, "Strategic Interaction," Goffman ransacks literature on mathematical games, and on everyday life, in order to delimit the social conditions under which gaming occurs. He believes that these conditions constitute a special case of the more general class of expression games, the case in which "two or more parties . . . find themselves in a well-structured situation of mutual impingement where each party must make a move and where every possible move carries fateful implications for all of the parties . . . each player must influence his own decision by his knowing that the other players are likely to try to dope out his decision in advance, and may even appreciate that he knows this is likely" (pp. 100-101). And he spells out some features of move and countermove that distinguish these games from others: (a) there is a constraint to play (once one shows himself to be "in" the situation, a contest, for example, he cannot decide not to play); (b) there are a limited number of possible actions (e.g., those set by the rules of the contest); (c) a move, once initiated, cannot be taken back; (d) the payoff is fully determined once the moves are made. What are the procedures for sizing people up under these special circumstances? That is the principal question. (Goffman considered the way in which people see themselves in these situations in his earlier work, *Interaction Ritual* [Garden City, N.Y.: Doubleday, 1967].) And the concluding assertion? All social relations are to some extent strategic games in that "control of the state of a relationship is a mutually interdependent objective of the persons related"

(p. 137). Gamesmanship is a human and social universal. The formal analyses of the mathematical theory of games can be seen as calculi for the shrewd pursuit of self-interest.

So it has gone, book by book. In *Behavior in Public Places* (New York: Free Press, 1963), Goffman showed us that even the most trivial of social contacts involve our making claims on others and claims about ourselves, and he then exposed the way in which, even in unimportant encounters, we try to maximize our claims and minimize our obligations. In *Asylums* (Chicago: Aldine, 1961), he documented the perversion of any organization that is officially designated as a place of refuge and help, the turning of its resources from the service of its clients to the aggrandizement of its staff. He showed that this process was not one of corruption in any ordinary sense but a consequence of the corporate imperatives of "total institutions" and of interpersonal relations within them. *Stigma* (Englewood Cliffs, N.J.: Prentice-Hall, 1963) contained a description of similarities in the roles of all who are for any reason outcasts from any group: the deformed, the delinquent, the incompetent, the ethnic minorities, and so on. Once again, the root of the problem was double: on the one hand, the necessity in any group of defining what it is by defining those who fail to meet its standards; on the other hand, the revulsion, however concealed with sympathy or tolerance, that men experience in dealing with those who do not fit properly into a social order. Each of us, we were told, has some stigmata, each is somewhat a pariah in his own eyes and in those of certain others. *Encounters* (Indianapolis: Bobbs-Merrill, 1961) was especially rich in illustrations of pressures to be objective to the point of callous disrespect toward those who deserve our help. Finally, there was *Interaction Ritual* and its core essay, "Where the Action Is." Action embodies something of what a man is as a person. It is his handling of an unanticipated, inescapable, fateful, and contingent situation. He may seek such situations, as in a casino or a bull ring, or they may be thrust upon him. They provide a moment of truth about the person he actually is behind his appearances and illusions. We were told that every man tries to learn this truth about himself while concealing it from others and to discover the personal essence in others while concealing the attempt.

Goffman's specialty, then, is to show us that selfishness and also folly, humbug, and incompetence are inherent in all personal conduct and in all social relations. Indeed, his major contribution is to suggest that they are "in" interpersonal relations rather than "in" individuals. His is a sensitivity to our life in a social marketplace—an ecological order in which norms are just another factor to be "taken into account" as possibly useful or as constraining in the pursuit of self-interest, and in which we must subject the most noble intentions of our fellow participants to the canon *caveat emptor*. In his books there is no redemption, no transcendence. There is only the austere satisfaction of knowing the worst and knowing that it is unavoidable. And of being entertained by the stunning resilience of self-interest and of collective delusion.

To this point, much of Goffman's work has been classificatory, a teasing

apart of salient features of all interpersonal relations. *Strategic Interaction* continues that line of development. Thus we are told that there are five basic moves in expressive games: unwitting moves, naïve moves, control moves, uncovering moves, and counteruncovering moves. The treatment of one indicates the treatment of all. In control moves, a person who thinks he is observed tries to produce an impression that will be to his advantage in the eyes of the observer. This is illustrated from materials on horse racing, military tactics, espionage, the acceptance of new fashions in clothing, crime and vice, and children's games (plus an aside about protective coloration in animals). Three subtypes of control moves are identified: concealment or cover, accentuated revealment, and misrepresentation.

These illustrations, and the others that proliferate on Goffman's pages, have a significance beyond their function as particulars from which a taxonomy is abstracted. First, they establish that Goffman is observing something so distinct and pervasive that we must take it into account in any full analysis of social life. Second, by showing that the object under study—a control move, for example—can be pivotal on occasions when great issues are at stake, the illustrations persuade us to take more than formal note of that object. They persuade us to take it seriously. Third, the illustrations contrast the "true" state of the situation with polite and public definitions of its character. They show that fact and fiction are equally social constructions and equally real. Finally, by using illustrations that display the ubiquity of some aspect of social relations, Goffman draws our own experiences into the exposition. We discover that we, too, are involved in these stratagems and hypocrisies.

Goffman faces the problems of everyone who tries to study human nature or the universal nature of social relations. As he looks beyond taxonomies to explanations, these will become more acute. He wants to assert universal truths but cannot examine all instances. Sooner or later he must face directly the problem of sampling. Multiplied illustrations suffice to show that something exists, but not to exhibit the systematic connections between it and other phenomena. For those connections, Goffman will have to characterize interaction in still more rudimentary terms than he has and then derive the processes of interpersonal relations from them. The distance he has to go is clear in *Strategic Interaction* because here his formulations appear cheek by jowl with the elegant derivations of the theory of games. He does, however, make a strong case that that theory would be greatly enriched and improved by the systematic incorporation of his observations on the social relations in gaming.

The two cases in which Goffman has analyzed systematic covariance most clearly are in *The Presentation of Self* and in *Asylums*. In each case he was able, informally but plausibly, to derive certain interpersonal relations from the interests of the participants and from an enviroing system of roles. In *The Presentation of Self*, the system was that of a small community in the Shetlands, in *Asylums* it was that of a mental hospital. In both, the existing system of roles provided him with a bounded set of social relations for study and with a set of forces whose interplay he could

Book Reviews

The Unexpected Community. By Arlie Russell Hochschild. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1973. Pp. x+1973. \$7.95.

Gordon F. Streib

Cornell University

Arlie Russell Hochschild has written a stimulating book about a lower-class housing project. Her research is based upon a long period of participant observation and informed by shrewd and perceptive sociological insights.

The study is of a special housing project in a small city in the California Bay area in which 43 persons lived: predominantly rural-born, working class, white, Anglo-Saxon Protestants. Of the 43 residents only five were men. There were four Catholic residents and no Jews or blacks. The homogeneity in terms of class, race, religion, and education is very important for the analysis and for any policy conclusions which may be drawn from the book.

One of the broad hypotheses around which the book is organized concerns social integration: what are the consequences of age-integrated living arrangements (young and old together) as compared to age-segregated environments. This theme serves as the focus for several other lines of analysis: the subcommunity of the old widows and the evolution of a subculture; the nature of social bonds when people interact with each other compared to their relations with outsiders, including relatives; the significance of family and kin ties, particularly relations between mother and daughters; and, finally, the kind of social relations observed between the residents of Merrill Court and the larger world: television, newspapers, the commercial world of stores and professionals, and relations with public bureaucracies.

The study was carried out as a requirement for a Ph.D. at the University of California at Berkeley. Hochschild's entry into Merrill Court was initially as a paid employee of the city department of recreation and parks. She writes that the residents were not bothered by a later revelation that she was carrying out a sociological study. On the whole the report is balanced, objective, and sympathetic. It is evident to this reader that she was able to maintain sustained and intimate social relations with the widows of Merrill Court, and then write an engaging account of her long period of field work.

The book does not read like a Ph.D. dissertation. It is literate in the best sense of the word and has a "story line" which holds the reader's interest and thought. Its lack of "sociologese," we are told, is due to "a quiet campaign" waged by her husband, who took time from his novel to read and criticize her work. I am glad that he did.

The title of the book is intriguing and important, for the author started her research apparently thinking the old tend to be isolated, alienated, and

lonely, and she found a sense of community where she did not expect it. Here are a group of widows, living at or below the poverty level in a marginal neighborhood, who are leading interesting, active lives and do not consider themselves to be a social problem.

Hochschild had several types of readers in mind: those with a personal interest in old age, social scientists concerned with subcultures and age stratification, and practitioners who give care to the old (physicians, social workers, administrators). The triple readership poses problems for the author which no writer could ever solve satisfactorily. One device which Hochschild employs is a dual footnote system: those at the bottom of the page are for all readers and notes for social scientists are numbered and are in the back of the book. Sociologists might find some of the more interesting insights in the footnotes.

I think it is appropriate to raise some criticisms, which weaken the book as a sociological study. First, it would have been helpful if the author had given the reader a statistical summary of salient social and demographic facts. We do not know, for example, the average income of the residents, although one can guess it is low because of the housing arrangements. However, a tabular explicit statement of fact on this and other matters (perhaps in an appendix) would have made the book much more useful for serious students and teachers and for researchers who might wish to compare this book with similar studies of other settings.

Another weakness is that the author does not systematically relate her own work to that of others. Hochschild is a careful student of a wide range of phenomena related to aging and also of significant sociological issues. But she ends her book without reexamining the central theme of age segregation versus age integration. One would expect that she would try to tie together the work of Rosow, Bultena, Hoyt, and others and see how her study sheds new light on this controversy. Hochschild refers to the housing project she studied as an "exception" (p. 139) and does not bother to compare her findings with those of other old-age communities. It is perplexing that she did not relate her findings to those of Sheila K. Johnson who studied a working-class mobile home park in a nearby city (*Idle Haven: Community Building among the Working-Class Retired* [Berkeley: University of California Press, 1971]).

Another analytical weakness is that the author does not systematically analyze her data in relation to disengagement theory, the starting point for her case study (p. x). At times she is critical of this dominant perspective in the aging field for the past decade. Hochschild's work sheds new light on disengagement theory by indicating aspects which are correct and others which clearly require modification and specification. For example, there were parts of her book which are similar to the notion of differential disengagement set forth by Streib and Schneider (*Retirement in American Society: Impact and Process* [Ithaca, N.Y.: Cornell University Press, 1971]). Her observations on the way in which her informants treated the inevitability and naturalness of death certainly support one of the basic propositions of disengagement theory. If the author had taken a few pages

to relate her work to the segregation-integration issue and to disengagement theory, it would have been richer and a more valuable book for sociologists.

The most disappointing part of the book is the epilogue, in which the author becomes more of a novelist than a social scientist. In conclusion, she writes: "The most important point I am trying to make in this book concerns the people it does not discuss—the isolated" (p. 139). I find it puzzling that a sociologist should end by turning to a problem which was not the central focus of the study, rather than relating her own findings to other studies and to pertinent theoretical issues. In spite of these shortcomings, this is a very fine case study, and a book which I recommend to the three kinds of readers Hochschild had in mind while writing it.

Contemporary Community: Sociological Illusion or Reality? By Jacqueline Scherer. London: Tavistock Publications, 1972. Pp. xii+155. \$8.50.

Michael Micklin

Tulane University

As suggested by its title, this thin volume by Jacqueline Scherer is designed to assess the conceptual and empirical status of "community" as a form of social organization, although the author's scope of inquiry is limited to technologically advanced urban societies. In large part, this work is an attempt to identify the organizational bases of contemporary communal life and to establish a revised sociological perspective for community analysis.

The book consists of 11 brief chapters plus a postscript. The first chapter outlines the author's analytic strategy, which consists initially of a series of unsuccessful attempts to define modern communities in terms of geographic, numerical, and attitudinal criteria, followed by an exploration of additional aspects of community organization. These latter considerations, expanded in later chapters, include problems of establishing community boundaries, the structure of political communities, and the bases of commitment among community members. With chapters 8–10 the author's perspective begins to emerge. She argues that in modern societies people are typically involved in multibonded relationships which may be usefully viewed from the perspectives of role theory and social network analysis. Personal networks are seen as crosscutting several levels of social organization, connecting diverse sets of groups and individuals. The key to a sociological understanding of contemporary communities is thus the capacity to trace out the structuring of social networks. The final chapter concludes that traditional geographically based communities are giving way to more mobile and flexible forms of communal association. These include (1) certain types of social networks, (2) synthetic communities based upon approximations of natural or traditional communities of place and culture, (3) hybrids composed of institutions or organizations that adopt essential communal characteristics, and (4) the intense, short-lived temporary com-

munity. The postscript contains a brief plea for the organization of communities along more humane lines, emphasizing what might be accomplished through effective collective action guided by moral considerations.

There are a number of reasons why this is not a useful book. First, the underlying thesis is incredibly vague; nowhere is this concisely summarized as a set of propositions to be examined or conclusions reached. Second, the chapters are far too brief. Previous work is not systematically summarized, and the author's points are frequently elaborated by only a few sentences or brief reference to illustrative case studies. Third, the topics dealt with are not logically related to one another. Why, for example, are we led from "Community and Identification: Are Churches Communities?" (chap. 4) to "Community and Boundaries: The Beginning and End of Community" (chap. 5) to "Community and Structure, Conflict, and Power: The Political Aspects" (chap. 6)? Finally, the author gives the impression that she has discovered something new; that is, that the dominant forms of human association in contemporary society are no longer organized along geographic and cultural lines. Do we really need another book to document this already well-recognized social trend? Somehow I doubt it.

On a more positive note, Scherer's suggestion of the utility of social network analysis in community research is worth pursuing. This strategy is well known among British social anthropologists, but has received relatively little attention in this country. Network analysis seems particularly appropriate for studies that deal with interaction processes in organizational settings, whether these be residential communities, professional groups, or hospital waiting rooms.

In short, I see very little use for this book in academic courses, and do not feel that the professional community researcher will learn much that is new. I might add that the price of the book, considering its brevity, may be sufficient to prevent even the most skeptical reader of this review from examining Scherer's work himself.

Commitment and Community: Communes and Utopias in Sociological Perspective. By Rosabeth Moss Kanter. Cambridge, Mass.: Harvard University Press, 1972. Pp. x+303. \$10.00 (cloth); \$2.95 (paper).

John P. Hewitt

University of Massachusetts, Amherst

Commitment and Community seeks to provide "a historical and sociological perspective on the contemporary commune movement" (p. vii). It approaches this task on three fronts: an examination of the ideas and values that underlie utopian communities; an empirical study of commitment-building mechanisms in 19th-century American utopian communities; and an examination of the contemporary commune movement, based in part on the author's field research.

Rosabeth Moss Kanter begins her book with an effort to deal with the nature and significance of the utopian faith. Chapter 1 begins with a short

discussion of the essence of utopias, then moves to a brief history of the commune movement in America, followed by extended descriptions of two communes—the Oneida Community (1848–81) and Twin Oaks, a contemporary venture in community inspired by B. F. Skinner's *Walden Two*. Chapter 2 is an analysis of communitarian thought in which six “idealizations of communal life” are presented as basic: perfectibility, order, brotherhood, unity of body and mind, experimentation, and coherence as a group. The discussion is clear and helpful to the remainder of the book.

Part 2, “Lessons of the Past,” presents, in four chapters, the findings of an empirical study of the 19th-century American communes. The focus of this study, which entailed searching primary and secondary historical records (using a sample and methods usefully described in an appendix), is upon commitment-building mechanisms. Commitment entails the “attachment of the self to the requirements of social relations that are seen as self-expressive” (p. 66), and it is upon the development of commitment that the success of a communal venture depends. Three modes of personal orientation to a social system (instrumental, affective, moral) underlie three types of commitment. Each type can be sustained by mechanisms of detachment and attachment, which separate the individual from his past and bind him to the new venture. Thus, instrumental commitment is sustained by mechanisms of sacrifice and investment; affective commitment by renunciation and communion; and moral commitment by mortification and transcendence. Each of these six mechanisms of commitment is fostered by a specific set of social practices. Thus, for example, sacrifice, which involves members giving up something as the price of membership in a communal venture, is fostered by abstinence from food, drink, or sex, and by austerity in life style. Investment, which provides the individual with a stake in the community, is sustained by irreversible investment—the inability of a member to recover his original investment or to be compensated for his time and effort if he decided to leave. Mortification, which seeks to remove previous identities and to make the community the new and sole basis of identity, is linked to confession and mutual criticism, deindividuation, and the spiritual differentiation of community members from outsiders. Kanter compared “successful” and “unsuccessful” communities (lasting, respectively, more or less than 25 years) by examining their use of social practices that fostered such commitment mechanisms, finding in general that the successful communities were more likely to have such practices than the unsuccessful ones. No successful community used all of the mechanisms; rather, each developed its own “commitment package.”

Whether or not they were successful in Kanter's operational terms, all of the 19th-century communities eventually died—through deterioration of ideals, changing environments to which they could not adapt, erosion of membership, and the growth of skepticism about the possibility of fulfilling their ideals. Moreover, these utopian communities sought both “to express values and to implement practical concerns in a single social unit” (p. 148). Over time, success in the latter tended to occur at the expense of the former.

Part 3, “Problems of Today,” deals with the contemporary commune

movement, dividing the wide assortment of communes into retreat communes and service communes. The former are concerned primarily with escape from the technological society and exhibit diverse ideologies and social arrangements; the latter adopt an attitude of mission, seeking to serve the society by defining themselves as helpers, choosing a constituency, and trying to reform it. Drawing upon the theory of commitment mechanisms developed in her historical study, Kanter examines commitment problems and practices in contemporary communes. Her discussion, while less systematic and detailed than in the historical study, is lucid and illustrates the value of the theory. In the concluding chapter there is a balanced effort to deal with some crucial issues that pertain to utopian communities. How is the success of such ventures to be measured? By mere endurance? By the options they provide for growing up and living in the society? By their provision for social experiment? How are the costs of participation, particularly the intense group pressures and controls they generate, to be weighed against their benefits? What are the tradeoffs between individual and group needs?

It is a difficult task to display both historical and sociological skills, and to avoid doing violence to the one with the other. Kanter is remarkably successful, showing a grasp of the problems, paradoxes, and issues of commitment that everywhere confront group life, as well as sensitivity to their anchoring in specific historical contexts and groups. Her book is a product (and example) of the sociological imagination that should prove of great value in the teaching enterprise as well as in the study of utopian communities.

A Mediterranean Society. Vol. 2: *The Community*. By S. D. Goitein. Berkeley: University of California Press, 1971. Pp. xiv+633. \$20.00.

Paul Rabinow

Princeton University

Social scientists may well be skeptical of multivolume philological works on medieval society. In many instances this hesitancy is more than justified. In the case of S. D. Goitein's monumental work on the Jewish communities of the 10th to 13th centuries in Egypt (old Cairo in particular) they would be doing themselves a gross injustice and missing a magnificent feast of insight, wisdom, and enormous learning were they to ignore it. All those concerned with the Mediterranean, Jewish and Islamic culture, society, and economics—be they sociologists, anthropologists, or what have you—are strongly urged to do themselves the favor of reading this study of the communal life of a medieval Jewish community.

The sources for this work are the random letters, accounts, and documents deposited during the 10th to 13th centuries in a Cairo Geniza or lumber room. These random fragments, writings with the name of God on them were not destroyed but buried in these storehouses, were discovered

at the end of the 19th century and are now found scattered in library collections at Oxford, Cambridge, London, Vienna, Budapest, Leningrad, Washington, New York, Philadelphia, and Jerusalem. From these letters and account slips Goitein has written a work of major importance.

Volume 1 of the proposed three-volume work dealt with the economic foundations of this cosmopolitan medieval trading community. Volume 2 deals with "The Community," and the forthcoming third volume will deal with "The Mediterranean Mind." Volume 2 is divided into three sections. The first two deal with the place of the local Jewish communities within the larger ecumenical world of Judaism and the everyday life activities of the people. The third section is concerned with the relations between the Jewish community in Fustat, Old Cairo, and the Muslim state.

The place of the Christian and Jewish communities was determined by the principle that law was personal and not territorial. Thus, these communities were bound by ties of law, religion, and sentiment to the wider ecumenical communities of which they were a part, as well as to the Muslim authorities and people within whose state they lived.

Although in principle the paramount authority of the Jewish world was the Gaon, or head of the Yeshiva (in this case in Jerusalem), by the time of the high Middle Ages the importance of the local territorial head of the Jewish community, or Nagid, had greatly increased. This man was the key liaison official connecting the local Jewish community, the ecumenical authorities, and the Muslim government. As Goitein makes vividly clear throughout the book, however, this was a highly personalistic system with duties, responsibilities, and power stemming from the character of the man holding the office rather than from the office itself.

Next to God, as revealed in the Holy Law, the people had the highest authority. Everyone was regarded as belonging to the congregation, and the terms "congregation" and "community," Goitein argues, can be used interchangeably to describe the situation. Although there was a regularly constituted board of elders to run community affairs (under the Nagid who supervised a larger territory), the congregation met face to face (and frequently) at the synagogue. The synagogue was the center of local communal life, and members took an active role in watching over community affairs. The highest authority was the law, and in this highly educated community there were many who were ready to interpret it. Thus, the community itself was loosely organized, basing itself on older civic forms which it had carried forward from Roman and Hellenistic times. Goitein emphasizes and documents its flexible and "participatory" character, its love of learning and of commerce. He situates it on a middle ground between the corporational life of Graeco-Roman times and the seeming paucity of communal organization in Muslim society of the period (p. 68).

Just as the internal life of the community was shaped by religious considerations, so too were its external relations. Religious minorities formed a state within a state. Each of the three monotheistic religions claimed to be the sole possessor of the full truth, and the very existence of the other religions was a challenge or even an offense. Each religious community

"formed a nation, umma, in itself, but in every country they shared a homeland, waṭan, in common" (p. 274).

The general position of the religious minorities was both safeguarded and precarious. Islamic law protected their life and property and freedom but also demanded their segregation and subservience (p. 289). This segregation—as the law—was not primarily territorial. There were not the closed ghettos one found in Morocco. The populations lived in close proximity. Religious concepts of superiority and seclusion were countered by economic and social conditions which made for a more tolerant attitude.

Lest this picture seem too positive, Goitein shows that even in this tolerant age the main institution which linked the Jewish community to the Muslim state, the poll tax, was experienced as heavily oppressive and was greatly feared. Widespread poverty and a persistent lack of cash turned the season of its collection into a time of horror and dread (p. 381). Charity by richer members of the Jewish community often came to the rescue. Goitein points out that the rationale for this charity was not an abstract sense of obligation to the community but rather was a religiously motivated duty toward God, an atonement for sins. In really drastic periods, mass conversion to Islam of the poorer classes resulted.

The genius of this work, which must already be considered a classic, is Goitein's ability to move from deceptively simple synthetic statements (for example, "the medieval doctors of the Mediterranean area were the torchbearers of secular erudition" [p. 240]), to minute details of everyday life (how scribes sharpened their pens [p. 234]) and make the two levels totally convincing and alive. This ability stems from two sources: first, a towering scholarly command of languages and history of the Mediterranean—his statement about the doctors required detailed knowledge of Greek medicine and its impact on Muslim and Jewish physicians in the courts of Egypt and the relation of this learning to the religious erudition of the day—and second, a deep empathetic feeling for the daily life and everyday realities of these medieval residents of Cairo. This partially stems from Goitein's work with and love of the communities of Yemenite Jews whom he studied over a number of years. He writes as if he had lived in Fustat, Old Cairo, and chatted with its residents much as he chatted with his friends and informants from the Yemen.

This is to say that Goitein has reconstructed a whole world. He tells us, for example, that "by an almost comical coincidence, at a recent visit to the University Library, Cambridge, England, I came across another loan taken by the same Jewish baker of bagels, this time from a Muslim miller . . . which is attested in a document preserved in a document in the Freer Gallery, Washington" (p. 297). He talks fondly of the man as of a personal acquaintance. We soon realize that this bagel baker and his 11th-century clients are mentioned on barely legible fragments scattered in libraries around the world. The reconstruction of the economic and social realities both of the Mediterranean trading community (which was the work of vol. 1) and of this street in Cairo are the result of decades of scholarly labor. Few scholars are equipped to make this bagel baker, and the economic

and social conditions in which he found himself, come to life with Goitein's flair and intelligence.

This book is a masterpiece of philological and historical reconstruction. This is both its glory and its limitation. Few if any scholars are likely to combine the massive erudition of Goitein with his active sympathy for the everyday life of these medieval Jewish people. From this rare combination of skills and sensibilities emerges a truly monumental work of humanistic scholarship which will long stand as a rich source for others. Goitein has given us a masterful portrait of life in the high Middle Ages and exhaustive documentary material on that life. Generations of scholars will benefit from his labors.

Human Societies: A Macrolevel Introduction to Sociology. By Gerhard Lenski. New York: McGraw-Hill, 1970. Pp. xv+525. \$10.95.

Manning Nash

University of Chicago

The teaching of introductory sociology is a notoriously difficult, demanding, tedious, and necessary enterprise. Most textbooks suffer from wordy definitions, undue abstraction, eclectic theory, ethnocentrism, and illustrative material veering between the gray statistical table and the trivial home truth.

Lenski's textbook sets out to reorient the teaching of introductory sociology by taking definite stands on the theoretical debates in the discipline. He opts for a macrolevel or total society framework as against social psychological or institutional levels, a social change perspective as more productive than a static structural-functional one; comparative data heavily outweigh United States material; and a useful glossary as appendix replaces reliance on extended definitions. All of these useful and imaginative innovations are brought together by the theoretical reliance on a framework of evolutionary theory. It is a theory of sociocultural evolution that gives the book coherence and thrust.

Evolutionary theory about social and cultural systems is one of the oldest, most persistent, encompassing, and erroneous of social theories. As a pedagogical or taxonomic device, it is probably useful for ordering vast amounts of data in simple typological rubrics and arranging those rubrics in levels of technological complexity. As a system of casual explanatory value, it is a just-so story, lacking mechanism, necessity, sequence, historical connection, or empirical validity. Lenski avoids most of the grosser errors of the evolutionists, both paleo and neo. He recognizes that organic and sociocultural evolution are at best analogies, that societies are not bounded like species, that diffusion is much more important for social change than hybridization is for organic change, that a symbol system has logical and affective coherence while its organic analogue, the genetic code, has a chemical and physiological interdependence.

This level of sophistication keeps him free of the wilder assertions of

evolutionary theory, but inevitably commits him to the game of labeling whole societies as types, giving a set of traits with the types, and then arranging the types in an order of complexity, and finally asserting that the lower types change over time into the higher types. The logical fallacy of this sort of argument has been so often exposed (i.e., R. A. Nisbet, *Social Change and History* [Oxford: Oxford University Press, 1969]), that a reviewer is tempted to forego the obvious complaint that the arrangement is made *ex hypothesi* and then used to demonstrate the validity of the hypothesis. More important is the empirical vacuity of evolutionary theory. First, for example, Australian aboriginal religion, or kinship, or whatever, as an example of the hunting and gathering level did not, is not, and will not evolve to the next level of simple horticultural. Nor in history or in fact will the simple horticultural Dyaks evolve to the agrarian-state level. There is little point in noting again that evolutionary theory fails the first test of any theory: it places no restrictions on the empirical frame of possibilities.

What is needed is to account for the continual appeal of a form of theory so often discredited in the past. I suspect that a sociology-of-knowledge analysis would reveal the extra scientific components of the seductive evolutionary mode of interpretation. For the present, we are again reminded that social science is the sort of enterprise in which it is nearly impossible to down a bad hypothesis.

Lenski's stresses on change, on whole societies, on comparative data are all in the right direction, and his book will make teaching the introductory course more lively and meaningful. The evolutionary ghost that haunts his pages is but the spectral ballet of 19th-century categories, dancing while social science seeks a new paradigm to take account of a modernizing world.

Crime and Justice in a Mass Society. By Alexander B. Smith and Harriet Pollack. Waltham, Mass.: Xerox College Publishing, 1972. Pp. 258. \$3.95.

Carl A. Bersani

University of Akron

Alexander B. Smith and Harriet Pollack, a sociologist and political scientist, respectively, have fused their talents and thus offer a volume which deals with law and its enforcement within a context embracing philosophical, political, and sociological aspects. Viewing crime and justice with such a comprehensive perspective enhances the applicability of this volume's use to a wide range of fields of study, among them criminology, sociology, political science, law, and law enforcement.

In conjunction with other paperbacks, this reviewer is using this volume in his introductory criminology sections. It has been well received by these students. Among students there is a small group who are "turned on" to readings only under the most ideal conditions, for example, readability, relevancy of topics, and ideological attractiveness. Thus, the enthusiastic student reaction is no small achievement. The authors are to be congratu-

lated, considering they do not achieve this reaction by shortchanging students on an objective treatment of topics or by backing off from consistently integrating empirical studies in their discussions whenever documentation of this sort exists.

Apparently the underlying theme reflected in the pages of this volume is that neither those who are to be legally designated as our criminals nor how those so designated are viewed and processed are phenomena settled for all time. The composition of our offender population is ever-changing. The writers therefore seek to understand what the laws are and what they will become; in what manner they are differentially enforced; and by what means and under what circumstances some are selected to be designated as criminals and others not. These pages are in effect an investigation of the alignments of interest groups and social and cultural shifts as they bear on the quality of justice and on the susceptibility among segments of our population to being designated for membership in our offender grouping.

Within a sustained comparison of the theory of American justice and the actual implementation of justice which characterizes this volume, what the authors have done is to deliver this message by weaving into this comparison philosophical, political, and sociological developments. This is accomplished by ways of nine chapters which deal with the nature of law and status groups as related to social control and differential handling, and an analysis of police, courts, and corrections as bureaucratic and legal structures. Additionally, as these topics are introduced, current trends and proposed solutions are also discussed.

Throughout this very readable book, the authors (in the topics which they have chosen to elaborate) have been thorough in presentation and organization; careful in the quality of evidence they offer; and objective in assessing critical issues and implications of "Crime and Justice in a Mass Society."

These authors, however, do sometimes bypass both a comprehensive coverage and a reporting of "esoteric" facts about a topic and focus directly on the critical areas of a topic. This is illustrated in chapter 6, "The Police." Here their attention is directed to several concise areas: who are the police (background, psychological makeup, and police subculture), conditions under which police discretion occurs, the relation of police discretion to police corruption, and political direction of police activities. In chapter 9, "After Conviction: Probation, Parole, and Imprisonment," they give a passing nod to what probation, parole, and imprisonment are; spend some time on the issue of punishment; but thoroughly examine the present and prospective postadjudicatory rights of defendants. This relevant area is not handled in such a thorough fashion, if at all, in textbooks adopted for criminology courses. On the other hand, chapter 8, "The Rights of the Accused: The Supreme Court, Due Process, and Police Procedure," illustrates one of the chapters having wider coverage and providing more technical information, yet it can be understood by almost any college student. This chapter reviews such areas as the Supreme Court and the American system, search and seizure, wiretapping and electronic eaves-

dropping, confessions and the right to counsel, fair trial and the right to counsel, and double jeopardy. As in other chapters, when each of these areas is discussed, philosophical, political, and sociological implications are introduced.

The drawbacks of this book exist only if one has predetermined its intended use. The authors have not done so. It is therefore impossible to criticize what has not been included, for example, topics dealing with social and legal implications of diversion; consequences of labeling processes; and an expanded treatment of sentencing legalities, probation, parole, and imprisonment. But what they have dealt with is handled quite accurately and stated clearly.

In brief, it is a well-done book, but a certain kind of book. For it is a view of the criminal justice system through a well-rounded and meaningful perspective. Along with other selected materials, it would be very useful in varied instructional settings, or, for that matter, would also be rewarding to the general reader.

The Politics of Disorder. By Theodore J. Lowi. New York: Basic Books, 1971. Pp. xx+193. \$6.95.

Barry Skura

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Progressive changes in contemporary America might be stimulated by the same tonic that would cure her crisis of public authority. The prescription, offered by Theodore J. Lowi in this set of important essays on social movements, is a hefty dose of law and *disorder*.

Both aspects of this remedy are directed against the same *bête noir* that preoccupied Lowi in *The End of Liberalism: Ideology, Policy, and the Crisis of Public Authority* (New York: Norton, 1969): group politics (the deity of interest-group liberalism). This is a pattern of policy making through bargaining among organized groups and between them and governmental agencies. It is symbolized by the notion of politics as "the art of the possible."

The *modus operandi* of group politics is the delegation, through vague laws, of public authority to administrative bodies and their organized constituencies. Such laws leave the all-important details of implementation vulnerable to seduction by organized minorities. But it is the virtue of a democratic state that is most victimized. This is because such groups are permitted to bask their special interests in the aura of public authority and, worse yet, unaccountably exercise that authority.

In contemporary demands for decentralization, these maladies find their culture. Lowi's contrast of the Blackstone Rangers as a gang and as an agent of the war on poverty is apt. "The Blackstone Ranger organizer may say, 'Join our organization or I will beat your head in.' The community organizer will say, 'Join our organization or you will have very little say in community policy making'" (p. 79).

Group politics is lawless. This is because individuals and organized groups particularistically guide the governmental process. Informal relationships predominate over formal ones. This contrasts with Lowi's ideal of juridical democracy. In juridical democracy, clear, universalistic laws are the supreme guides to governmental policies, and political institutions abide by their formal charters.

Besides being lawless, group politics is implicitly conservative. It is the politics of quiescence: bargaining and incremental demands are the norm, with basic issues seldom raised. Since bargaining occurs only among established organizations, the unorganized are perpetual "outs." Worse yet, Lowi argues, these established organizations (as opposed to social movements, which are nascent ones) are themselves necessarily conservative. As organizations evolve, they are subject to an "iron law of decadence": "contrary" to Michels and Merton, they do keep their original goals, but operationalize these in terms of their current organizational maintenance needs. As a result, they will resist other approaches or vehicles for reaching their goals—often with the government's aid.

The politics of organized groups, then, is for Lowi the politics of lawlessness and order. In contrast, the disorderly politics of social movements is for him a potential source of change and legality. At a time when old institutions are not working, social movements may contribute to change by energetically raising the basic issues and broad social critiques that are slighted during the compromises of quieter periods. These nascent organizations moralistically expose the pathologies and inequities which the established organizations perpetuate. At the same time, social movements provide the focused stress without which stable organizations will not change. In Lowi's eyes, social movements are the force for change in established organizations, which are by their nature conservative.

He also sees social movements as potential counterweights to the lawlessness of group politics. That lawlessness centers on the dominance of informal bargaining relations over the formal decision making of constitutionally authorized agencies. Social movements often upset this hierarchy by their refusal to bargain. New rules then apply.

"The rules are: confrontation, debate, formulation, voting, policy making, and ultimately a decision that may require forceful imposition of a new policy on the society. But examine those words and those rules. *They define the formal democratic process.* A democratic government is inconceivable without full coercive imposition of laws democratically arrived at" (p. 56). Lowi argues that this activation of the formal mechanisms of decision making gives us decisions that are both clearer than those of the bargaining process and also arrived at more openly. This is because the dagger of social movements is not friendly to the normal efforts of administrators to veil their decisions and apply them at increasingly lower, less visible levels.

Lowi sees the unfettered critical energies of social movements, then, as a valuable catalyst to change. In one of the best statements of the natural history of social movements which I know (chap. 2), he shows how those

energies eventually wane as a staid organization forms out of the movement's ashes. At that point, the former movement is mere fodder for the pathologies of group politics. The author traces the development of the labor movement, trade associations, and farm organizations in illustrating these points.

Lowi sees anything that deflects the movement's energies as enervating it. As a result, he is wary of hasty efforts to respond to a movement, for he feels that even a Pyrrhic victory deflects the movement's energies. When leaders rush to "do something," they tend to pass bad laws, ones that are vague and not backed by the full coercive powers of the state. Since such laws are grist for the conservative processes of group politics, they do not change anything.

With this in mind, Lowi advises that "chaos is better than a bad program." The chaos and energy generated by a movement, when unimpeded, at least have the virtue of making the open decisions and clear laws, of which I spoke earlier, more likely. In fact, the biggest (if unappreciated) favor that decision makers can do for a movement, Lowi implies, is to goad it on by being slow to respond. Then the brew of energies becomes more potent and the pressure for resolute decision making correspondingly increases.

The result may not be what the underdogs want. But a law that is clear and backed by the state's full powers is the only weapon that they have in the struggle for major changes, Lowi argues. In contrast, the group-politics alternative is guaranteed always to be conservative. Here again law and disorder are fundamentally joined.

As should be clear by now, Lowi does have something to say. And he says it well. From a sociological standpoint, one of the virtues of this book is its rescue of disorder and conflict from their current bad press. However, it is miserly in acknowledging the relevance of much of the sociological literature on social conflict and movements. A second virtue of this book is in challenging the nostrums of "pluralist" approaches to the study of politics. It is about time that the "invisible hand" assumptions of this area were at least given a sabbatical.

Lowi is quite insightful. His blemishes lie in his untidy logic. For example, he assumes that the failure of decision makers to respond quickly to a social movement goads it and fires its energies. Under what conditions will the result rather be frustration and the movement's decline? Under what conditions do small "victories" lead to broader goals rather than the dissipation of movement energies? What about situations (such as those described by Zald and Ash) where the movement's formalization does not lead to greater conservatism? How does the movement's internal evolution, which he describes, relate to the timing and character of responses by decision makers?

Similarly, Lowi discusses social movements as nascent organizations and (all?) political groups as the products of social movements. Yet, what affects whether or not a successful movement will die without any organiza-

tional heirs? He distinguishes between the bargaining that occurs during quiet times and the confrontations fostered by social movements. But is the dichotomy so clear? He points to the "decadence" of organized groups in his obituary for interest-group liberalism. Yet, he does not tell us why the formal institutions of government, in whose belly he places his hopes for "juridical democracy," are exempt from the "iron law of decadence."

We could go on. But this in no way diminishes the fact that Lowi, as usual, has written a book that should be read. Parenthetically, we commend him for an especially good editing job.

Youth and Social Change. By Richard Flacks. Chicago: Markham Publishing Co., 1971. Pp. 147. \$6.95 (cloth); \$1.95 (paper).

Leonard Blumberg

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Competitive, entrepreneurial capitalism has been displaced by advanced corporate capitalism since World War I. There has been a bureaucratization of public and private affairs, economic and social planning are much more important, and there is a greater emphasis on the consumption of goods rather than their production. Inevitably there are contradictions in the prevailing adult culture manifested within the family and its child-raising practices. This is conceptualized in terms of "patterned maladjustment," "role strain," and various "ambivalences." A youth culture emerged as an inevitable consequence. A youth class was inevitable because so many young people continued beyond adolescence as students or in the military. These institutions made possible the segregation of youth from the general population and their physical concentration. Black youth provided the initial heroic leadership in their confrontations with injustice. The mass media have exploited the situation, but in so doing they also facilitated the development of the youth culture. Inevitable conflicts over who should control the conditions of their lives focused youth activism on the universities and the Vietnam war. Youth class consciousness began to emerge out of the struggle. All youth have not been equally relevant or active in these events; rather, the youth class has been led by a vanguard of alienated middle-class young intellectuals who were based in the elite institutions of learning.

Richard Flacks expects a continuation of trends into the 1970s. The youth culture will continue to spread to the working classes. Urban and rural youth ghettos will facilitate the development of youth class consciousness. Those of the vanguard who joined the professions will develop radical cells within "educated labor." The radicalization of sex roles will reduce the alienation of women and give strength to the youth class revolution. Blacks will be integral to the new culture and the new distribution of power that will emerge.

Flacks sees some deterrent factors which may reduce youth class consciousness and vitiate the revolution. The mass media may seduce the vanguard to meet their personal needs to achieve rather than to forward the needs of the revolution. The drug scene may drain away the potential to mobilize for community building and sociopolitical action. Finally, middle-class youth assumed that they always could find recognition in the job market if they did not achieve as artists in the mass media or as revolutionary leaders. However, the job market has sharply contracted and the consequence is existential despair. "The fear that one is nothing and destined to be nothing is the dark side of trying to avoid the constraints of conventional adulthood. It is that fear which breaks the spirit of youth movements" (p. 138) because they cannot deal with it.

Flacks proposes the establishment of communities of sharing and common purpose, now, before the revolution; and also the broadening of the revolutionary base by the inclusion of older people who now lead conventional lives. They, too, should be able to participate to some degree in the process of social change. He does not say how the latter can be done.

Flacks is trying to make sense out of events for himself as well as for other members of the vanguard. His presentation is at once an interpretation of these events and a programmatic statement of broad strategies of bring-on-the-revolutionary-change which he advocates. He rests much of his case on general knowledge, choosing to present the "big picture" and sweeping generalizations rather than careful point-by-point documentation. His material is poorly footnoted, and he gives little acknowledgment to his intellectual and empirical forebears.

There is a heavy dependence on a doctrine of cause, effect, and lawfulness as this is expressed in his belief in contradictions and that the emergence of the youth class movement was inevitable. But all this is analysis after the fact. When Flacks gets into prediction, which after all is the test of a doctrine of lawfulness, he seems to back off and to shift into a discussion of his strategies and his fear that there are significant obstacles for the revolutionary youth movement. Flacks is under no obligation to tip his hat to other ways to analyze social movements. But he is so committed to the view that he has seen the other side of Jordan that he has failed to give adequate attention to contratendencies and alternate sociopolitical movements that may exist in our society. Put another way, if one regards the emergent future as a conflict between a number of divergent possibilities, can we find some way to express the probability that any given one of these alternative futures may emerge? Is that the way to proceed with a doctrine of lawfulness in the analysis of our times? What modifications will this require of the conceptual model that Flacks uses?

Notwithstanding debates of this sort, the book advances some generalizations about sociopolitical movements that are worth further examination by other scholars. Furthermore, it does consider many of the problems that should be examined by students of social movements, albeit briefly. *Youth and Social Change* should therefore be useful as a teaching aid in undergraduate courses on social movements and related topics.

Black Students in Protest: A Study of the Origins of the Black Student Movement. By Anthony M. Orum. Arnold and Caroline Rose Monograph Series. Washington, D.C.: American Sociological Association, 1972. Pp. vi+89. \$5.00 (paper).

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Overall, this work constitutes a very interesting and well-organized effort toward ascertaining the relative influence of certain hypothesized variables affecting black students' propensity to become involved as leaders or participants in the earlier phases of the "Black Student Movement." Initially, the author draws heavily, both methodologically and theoretically, upon the works of other scholars, many of whom have concerned themselves primarily with social movements, political protests, and more traditional political behavior among whites—both students and nonstudents. It is perhaps owing to this fact that some of his more interesting findings focusing upon structural and demographic variables reveal more about certain differences between black and white student activists than about factors influencing the character and motivations of black activists per se. Of particular significance in this regard are Anthony M. Orum's findings concerning the precollege dimensions of political socialization which, in the case of the black student activist, contradict some previous findings derived from data gathered on whites. For instance, family socioeconomic status was found to bear virtually no relationship to black student activism and political participation. Other findings which contradicted the thrust of much previous research were that family stability (broken family as opposed to two-parent family structure) is only slightly associated with participation in the black student movement; that black males were more likely to be participants and activists than black females; and that neither geographic region nor the type of community in which students grew up had any appreciable influence on the likelihood of political participation and activism.

While chapter 4 of Orum's study, entitled "The Economic Roots of Protest," is ostensibly more to the point, his approach is to assess the degree to which black student activism is explained by using three theoretical perspectives typically posed as highlighting influential factors affecting the economic roots of the civil rights movement, that is, the "vulgar Marxist" explanation, the theory focusing upon "rising expectations," and the theory emphasizing "relative deprivation." His data results indicate that not one of the three theories is confirmed as contributing to the explanation of black student participation in political protest. But here again, the author's investigation is more enlightening with regard to pointing up factors which do not aid in the explanation of black student protest than it is in terms of delineating and clarifying relationships among those factors which do.

In chapters 5 and 6, more positive results are presented which do shed light on some possible links between the black student's involvement in his

college community, certain structural characteristics of the college and surrounding communities, and the emergence of black student activism. For instance, it was found that black students who are highly active in campus organizations are more likely to be participants and activists than their less involved classmates; that members of certain parapolitical campus organizations (student government, editorial staffs of school papers, and organizations concerned with world or national issues) are more likely to be involved in civil rights events than students who do not belong to such groups; that black students attending residential colleges were more likely to become involved in civil rights protests than students affiliated with commuter colleges; and that high rates of black student participation in protests are linked directly to the existence of relatively prosperous black populations in the communities where colleges are located.

In chapter 7, the concluding chapter of the work, the author summarizes his findings and discusses the various precipitants of goal and ideological transformations which occurred in the black student movement over the decade of the sixties. Ironically, while the focus of the main body of work dealing with factors hypothesized to influence black student protest dealt with structural features of the black student's precollege background and his then-contemporary collegiate environment and circumstances, the discussion of factors precipitating major changes in the black student movement barely mentioned these features at all. Rather, with regard to the latter, the concern was with the impact of personalities (Malcolm X, Stokely Carmichael, and Eldridge Cleaver), events (the failure of social institutions to implement hard-won civil rights legislature and the murder of Medgar Evers), and ideologies (separatism and black nationalism versus integration). Indeed, upon reading chapter 7, one gets the impression that black student participation in political protest might possibly be determined to a substantial degree by the existence of charismatic personalities, significant negative occurrences, and the existence of an appealing ideology alone—all other considerations notwithstanding.

With regard to the future of the black student protest movement, Orum again shifts the emphasis of his discussion—this time from one focusing upon personalities, events, and ideologies to one concerned with the question of the movement's future effectiveness in forcing desired changes in the distribution of wealth and power. Rather than concerning himself with the future effectiveness of the movement, a venture which at best constitutes an exercise in sheer speculation, it would have perhaps been more enlightening for the author to put forth some educated assessments as to probable character, motivations, and dimensions of black student involvement in political protest in the future given the findings in his study. That is, given his finding that black student activism is directly related to the economic condition of the local black population, for example, what is the future of black student participation in political protests likely to be if economic conditions for black Americans improve during the decade of the seventies? What if economic conditions deteriorate? Which variables found in his study to be significantly associated with black student protests appear to have

greatest potential of influencing the intensity and character of the black student movement in the future? As it now stands, his discussion of the future effectiveness of the movement, his speculation as to whether the Black Panthers will be more successful in capturing a large share of America's wealth and power for blacks than less militant groups have been, and his assertion that white liberals will probably continue to be hostile toward black militancy add nothing to the thrust of his study. This tack of gross unassimilated speculation may stem partially from the fact that the profile offered on the origins of black student protests actually says very little about the origins of such activities. Even the positive findings of the work are, upon consideration, somewhat less than thoroughly enlightening in this regard. A case in point is the finding that black students involved with parapolitical organizations on the campus also tend to participate in protests. While this finding is ostensibly significant, it does not contribute much to our understandings of the motivations underlying the black student movement. For example, could it be that black student parapolitical affiliation and black student protest are both motivated by clusters of factors so similar that a high degree of association between them is all but assured? Or perhaps under certain conditions involvement in black student protest merely constitutes an extension of involvement in parapolitical organizations on campus. In this case, the finding of a high degree of association between the two factors would be, again, expected, since one would essentially be researching in point of fact the degree of association of a variable with itself.

Nonetheless, *Black Students in Protest*, despite some shortcomings, constitutes a welcome and much-needed analytical addition to the numerous, basically impressionistic works on black students and the black student movement. As a sociological effort, this work is quite probably the most empirically sound study presently available centering upon the earlier phases of black student involvement in political protest.

Latin American University Students: A Six-Nation Study. By Arthur Liebman, Kenneth N. Walker, and Myron Glazer. Cambridge, Mass.: Harvard University Press, 1972. Pp. xxvi+296. \$11.75.

Alejandro Portes

University of Texas at Austin

This is both an informative book and a solid contribution to political sociology. Its main strength lies in the authors' familiarity with and concern for events in the Latin American university, reflected in crisp, vital accounts such as that of the Mexican student strike in 1968. Those with a beginning interest in Latin America will find it an interesting, readable, and useful source.

The fundamental weakness of the book, however, is closely tied to its main virtue. The authors' very proximity to their material leads to a

tendency to overinterpret particular events. Rhetoric takes over and leads to descriptions that, though powerful, are naïve. Thus, the description in chapters 1 and 2 of the academic situation of Latin American universities is a uniform tale of catastrophes which leaves the reader wondering how these institutions have been able to function at all. By pursuing this line of interpretation, the authors reinforce well-established clichés about Latin American universities without providing original observations or thoughts. While undoubtedly there is much truth to this catastrophic portrait, it leaves the reasons that most institutions in the area do manage to operate and even to provide high-quality training unexplained. How is it possible that problem-ridden institutions like the University of Buenos Aires and the University of Chile produce such good engineers and nurses that they are consistently in demand in developed countries? Why should one-fourth of the graduates of Argentine medical schools who have emigrated to the United States move into high-level academic positions (Charles Kidd, *Migration of Health Personnel, Scientists, and Engineers from Latin America* [Washington, D.C.: Pan American Health Organization Scientific Publication No. 142, 1967])? It is legitimate to ask that scholars so obviously familiar with an area further our understanding of its complexities by providing shades and nuances rather than merely reinforcing stereotyped pictures.

Three other specific problems also deserve mention. First, data analysis in chapters 3 and 4 is poor. While careful attention is paid to the problem of temporal and causal priority, little discussion is devoted to the possible spuriousness of several relationships. Moreover, the analysis is based on contingency tables interpreted over scores of pages. Limitations of tabular analysis are well known. They become especially apparent when authors attempt to compare effects of different factors on a dependent variable. With limited sample size, the analysis is restricted to a cumbersome series of trivariate tabulations. Both the problem and the data almost cry out for conventional multivariate regression.

In addition, chapter 3 is largely devoted to analysis of determinants of student radicalism and political activism, while chapter 4 is dedicated to a similar analysis of student conservatism and apathy. The two sets of dependent variables are obviously not independent, since they form the extremes of the same logical continuum. Thus, the same independent variables are brought forth to explain proportions of student radicalism in one chapter and proportions of student conservatism in the other. Such unnecessary duplication could have been avoided by a tighter methodology.

Second, systematic explanation is neglected throughout most of the volume in favor of description and narration. While there is nothing wrong with idiographic material, the stated purpose of the authors transcends this level, and the data clearly suggest the possibility of causal interpretation. Interesting explanatory notions, such as the role of partial liberalization of rules on increased campus radicalism, are advanced but remain buried in an ongoing narrative. Carried away by rhetoric, the authors propose explanations almost casually, not bothering to structure them

either through an initial framework or a final synthesis of the discussion.

Third, an obvious bias in favor of student causes permeates the book. This, in a way, comes as a welcome relief from a literature firmly grounded in antipathy toward "agitator" students in Latin America. Nor can we pretend at this stage to have entirely value-free descriptions. Yet, a volume written from this perspective could profit from a somewhat more detached stance. Though the authors make an attempt to do so at times, the general impression is one of consistent support for the students' views. For example, discussion of barriers to university entrance is developed exclusively from traditional student arguments in favor of open admission. The resulting negative evaluation of these restrictions is nowhere attenuated by mention of the results of open admission in universities that have adopted it. Argentine medical schools, which have followed such a policy for years, today graduate thousands of physicians annually who later are confronted with a saturated urban market and a complete absence of rural facilities. Unemployed or underemployed, many turn to emigration as the only alternative. Last year, 400 young Argentine physicians, products of "open admission," took the preliminary examination to emigrate to the United States.

Despite all these criticisms, the book remains a solid effort. Its authors are clearly among the most knowledgeable scholars in the area. The import of the above comments is not that the book is not worthwhile, but that it could have been much better. Even Seymour Lipset's endorsement of it as the "best existing treatment" of the topic (p. xxvi) can be accepted if by it is meant not a definitive work, but a most promising effort.

The Dependability of Behavioral Measurements: Theory of Generalizability for Scores and Profiles. By Lee J. Cronbach, Goldine C. Gleser, Harinder Nanda, and Nageswari Rajaratnam. New York: John Wiley & Sons, 1972. Pp. 410. \$12.95.

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Durkheim was clearly wrong in the injunction implicit in both *Rules* and *Suicide* that sociology should avoid both the methods and the theories of other disciplines, but the dispassionate reader of the sociological literature would find it hard to conclude that we have learned this lesson. The work of Sewall Wright on path analysis has been available since 1918, and related work in econometrics since at least the 1930s, but it was not until the 1960s that the essentially qualitative approach to causal structure of the "Columbia School" of survey analysis gave way to genuinely quantitative approaches (Simon, Blalock, Boudon, and Duncan).

Again, Spearman began writing on the effects of random measurement error in 1904; his work led to the development of an entire subfield of psychology, psychometric theory. Sociologists have had two main approaches to psychometric theory. They have either ignored it and, in effect,

assumed that the only important source of random error was sampling; or they have embraced it more dogmatically than the psychometricians themselves, who, for at least two decades, have had serious doubts about the applicability of the work on reliability deriving from Spearman. Neither of these approaches is viable. Blalock's pioneering work on errors of measurement in social research has demonstrated that measurement error makes an important difference, and Duncan's followers have traced out this reasoning in the construction of causal models.

The problem with the slavish followers of Spearman is more difficult to present in simple compass, and I shall not try to do so here. The reader who has a moderately good knowledge of statistics—say, at the level of an introduction to theoretical statistics—will be able to follow the arguments and the historical recapitulations in the book under review.

It is a brilliant, thought-provoking, deep, and valuable work. Everyone who thinks of himself as a serious methodologist, of whatever persuasion, should work his way through it (and it is work, not something that can be tossed off in an idle afternoon). Were money still available for such abstract, nonpolicy-oriented research, as we are now told it no longer is, I would urge that the section on methodology of the American Sociological Association undertake a broad and deep study of the applicability of this book to social research. Reduced as we are to the old-time ways of individual scholarship, each of us will have to do it on his own. In the end, that may be the more fruitful way.

The justification for such a monumental effort to assess the applicability of this book to social research is found in a previous work by Cronbach and Gleser, *Psychological Tests and Personnel Decisions*, which I reviewed in this *Journal* a decade ago (1958[64]:212–14). The nub of this argument is that one should try to minimize the costs of error, or the expected costs of error. It is one thing to consider the costs of admitting a substandard student to medical school or refusing admission to a potential medical genius; it is something else again even to try to assess in the most qualitative way the costs to individuals or society of falsely accepting or falsely rejecting a sociological hypothesis.

The arguments in this book are so complex and so tightly organized as to resist easy summarizing, but I think that any reasonably open-minded sociological methodologist who works his way through this book will emerge with ideas significantly different from those he started with; it is certainly worth the effort. Nevertheless, there are at least two important respects in which I find this book unsatisfactory. One is the all-too-willing acceptance of Bayesian inference at a time when it appears to have crested among theoretical statisticians. Insofar as the Bayesians or the neo-Bayesians urge us to pay attention to what our "expert judgment" tells us about our data, they are on firm ground, but when they urge that every hypothesis be put through the Bayesian mill, they verge on sectarianism. Theoretical statistics is in a ferment, or what Thomas Kuhn has called a "crisis." Bayesianism is surely not the new "paradigm" that will unify all statisticians and users of statistics in a new age of "normal science." And yet here it is, firmly

ensconced in psychometric theory, and we may surely look forward to an invasion of social research by the Bayesian hordes!

My most fundamental dissatisfaction with this book has to do with its philosophical foundations, which have a pervasive effect on its arguments. Cronbach et al. argue that one can "generalize" from any set of observed data to several different conceptual universes that might have, or could have, produced such data. In short, what we have here is a revival of the idea that there is some set of specifiable procedures—in this case, the procedures of statistical inference—for generating inductive inferences, or mechanically extrapolating from the observed particular to the unobservable general.

The authors assemble an impressive array of experts, including statisticians and philosophers of science, to testify to the validity of this argument, but they appear not to have heard of the work of Karl Popper, whose two major works (*The Logic of Scientific Discovery* and *Conjectures and Refutations*) argue convincingly, to me at least, that this simply cannot be done. I vote for Popper.

Despite this reservation, which is much more serious than my discomfort with Bayesian inference, I have learned a great deal from this book, and so will anyone else who does elaborate quantitative research or tries to teach others how to do it.

The Occupational Aspiration Scale: Theory, Structure and Correlates. By Archibald O. Haller and Irwin W. Miller. Cambridge, Mass.: Schenkman Publishing Co., 1971. Pp. 132. \$1.95 (paper).

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In this monograph Archibald O. Haller and Irwin W. Miller review the empirical evidence for a psychological variable of Level of Aspiration (LOA) and examine other evidence which tends to show that LOA is measured by their Occupational Aspiration Scale (OAS). They show that the latter has a reliability of about 0.8% and correlates about 0.6% with the best previous test of LOA (it has a slightly higher correlation with IQ). They tell us that LOA, whose relations to *n*-achievement and other similar concepts are carefully expounded, "is believed to be a psychological orientation to *enter* the occupational world at one level rather than another" (p. 101; my italics). They also say "that the OAS is a measure of *relative*, not *absolute* LOA" (p. 102), by which they appear to mean that a high correlation between OAS and actual occupational grade would validate the scale even if the mean and dispersion of aspirations differed from those of the distribution of occupational grades.

The discussion of the literature is meticulous, and positive and negative items of evidence are weighed with great deliberation. The authors are careful to point out that the predictive accuracy of the OAS cannot be assessed until the members of their sample of students have established

themselves in occupations. Evidence is presented which suggests that the OAS is unifactorial.

Although Haller and Miller run the danger of boring their readers with somewhat repetitive exposition of scrupulously garnered facts, they display a much lighter touch in the construction of the scale itself, where they have ingeniously combined complexity and brevity.

There are however two basic kinds of problem inhering, respectively, in the measurement of aspirations and in the validation of a proposed scale, and these have not been tackled by the authors. The first is the general problem of the refraction and distortion which any desire or intention undergoes as it is translated into a statement, claim, or measure of hope or purpose. Although some vocational choices are reached early and maintained unswervingly, much more typical is a gradual process of testing of oneself and one's capabilities against objective opportunities and peer-group standards, so that aspirations must be regarded as changing and developing phenomena. At the same level of analysis, we are faced with the problem of the precise status to be ascribed to the expression of an aspiration at one point in time. How reasonable is it to ascribe high aspirations to a boy who has no spur to prick the sides of his intent? What are we to make of the young man who conceals his purposes the better to savor their success or mitigate the effect of their failure?

So, when this loose behavior I throw off,
And pay the debt I never promised,
By how much better than my word I am,
By so much shall I falsify men's hopes.

The second type of problem is that of the questions begged in validation by reference to (relative) occupational position actually attained. British sociologists are perhaps more aware than their American colleagues of the "hands, that the rod of empire might have sway'd," and they are in consequence less willing to characterize a man's occupational position as an unequivocal indication of his occupational achievement, still less of his occupational aspirations.

Both these kinds of problems are inherent in the construction of a whole class of social measures. The former, for example, is a stumbling block to investigators of planned family size, and the latter is a particular case of the "better class of coffin" principle of validating measurements of a variable at one stage of the life cycle against a variable measured at a later stage.

In their future work on occupational aspirations, Haller and Miller will have to find ways of dealing both with problems of response bias and quality, and also with factors which constitute the context in which aspirations find, or fail to find, expression. These factors include not only personal capacity, but also social constraints such as sponsored mobility and various forms of occupational transmission, all of which may be supposed to impair the fit between hope or intention and outcome.

Blue Collar Workers: A Symposium on Middle America. Edited by Sar A. Levitan. New York: McGraw-Hill Book Co., 1971. Pp. xx+393. \$12.50.

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Chicago-area television viewers watched a rather curious commercial during the past holiday buying season. It pictured a clumsy, semiarticulate "working man" who demonstrated how he learned to play an electronic chord organ without any lessons. We will never know how well it succeeded in selling the product, but many people came away from it puzzled. They did not know whether to laugh at or cheer him. Although only a single incident, this character was symbolic of the image problem of the American blue-collar worker. He has become a mixture of comedy and anger, awe and disdain. It is precisely this chaotic clash of opinions that is reflected in Sar Levitan's recent *Blue Collar Workers*.

This volume has no real theme, not because the editor lacks opinions, but because he wisely assembled some of the best commentators on the subject, and the most knowledgeable people tend to disagree. Two of them even claim that there is no "blue collar problem," and argue their case well. Others among the 16 articles hold contrasting views of the role of unions, the amount of racial prejudice among the workers, the importance of the Democratic party coalition, and a variety of other subjects. The general drift of most of the articles is conservative, that is, that the "problem" is less crucial than we might think, and this will probably irritate radical critics. But in no way does it detract from the generally high quality of the arguments presented.

There are a few lapses in the volume's coverage, however, that prove annoying. Too little is said about ethnicity, which many would agree is a crucial factor. Perhaps anger with "the system" is the final stage in the assimilation process that began with docile immigrants. The fact that the recent revival in ethnocentrism manifest in the ethnic heritage legislation before Congress coincides with the growing sense of blue collar malaise may be more than just coincidence. Census data on the relative mobility of different nationality groups might be the key to this problem, but in any case the matter deserves more than a scant two pages.

Likewise, there is no geographical perspective. The dispersal of the working class to the suburbs is a recognized fact, but no contributor explored its impact. The church, the clubs, the neighborhood, and even the traditional blue collar tavern have either been transformed or destroyed in the process. The loss of these institutions, around which so much of the workers' lives revolved, is likely to be a contributing factor. Nearly two decades have passed since the flood of ant suburb, "ticky-tacky" books; perhaps we are passing through a blue collar stage of the same phenomenon.

The volume, like the bulk of similar books, lacks any meaningful sense of history. Anyone familiar with the story of the working man in the last

century has to ask himself, "Haven't we seen this before?" The "Blue Collar Blues," as one contributor calls it, did not suddenly appear with the war protests, civil rights backlash, or Vega assembly lines. Many bits of evidence, from Bureau of Labor Statistics reports to grade-B movies, suggest continuity rather than crisis; it is merely discovered from time to time. This suggests that we should not ignore the forces in society that create faddish "crises." Similarly, the differentiation between unique blue collar problems and those of the rest of the society is unclear. Lots of other people are also unhappy, so perhaps studies like this volume are dealing with only a small portion of a larger problem.

These flaws are unfortunate, but common in edited works; the larger the number of contributors, the more scattered the coverage. Indeed, the fact that the book raises so many questions speaks well of the authors' efforts. In a field rapidly filling with pop polemic, it is refreshing to see something of this high quality published. It must be regarded as an important contribution.

The Engineer in the Industrial Corporation. By R. Richard Ritti. New York: Columbia University Press, 1971. Pp. xi+266. \$10.00.

De l'O.S. à l'ingénieur—carrière ou classe sociale? [From semi-skilled worker to engineer—career or social class?]. By Claude and Michelle Durand. Paris: Les éditions ouvrières, 1971. Pp. 317. Fr. 42.00.

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Scientific inquiry is stimulated by a puzzle. Richard Ritti started with the puzzling phenomenon, in several national studies, that industrial engineers are less satisfied with their jobs than are skilled workers, foremen, salesmen, and management—and only slightly more than factory workers. He found the same pattern in a large (100,000-employee class) corporation in electronic and electromechanical systems that he calls Electromech, with a dozen geographically dispersed Research and Development laboratories ranging from about 100 to about 1,000 professionals. Whereas over 60% of service engineers and salesmen felt their job was "very good," only 37% of laboratory engineers and technicians felt this way.

The first eight chapters report the author's search for an explanation, using data from successive surveys during 1963–67 and especially a total census of the engineering population in the fall of 1963 with replies from 4,582 nonsupervisory professionals. The final two chapters integrate his insights with other literature and examine the implications.

In this high-technology corporation, whose customers are mainly institutions, the key to future growth is technological innovation of large-scale systems which must be planned years in advance, requiring the coordination of hundreds or thousands of technical workers. Large-system technology is

typical of fields like aerospace, military electronics, and computers, which differ radically (for example) from chemical plastics, where a new process might be developed by a few dozen professionals.

Ritti's central thesis is that in large-system development, job dissatisfaction for the industrial engineer is endemic. Electromech is a prosperous, attractive company with good employee relations. Dissatisfaction of engineers is seen as a function not of the leadership style of supervisors, but rather of the nature of the technology and the corresponding pattern of top-management control. Decisions on starting and stopping projects and allocation of manpower and money must be made by top echelons having no contact with the operating level.

Such processes are hardly instantaneous or efficient, and periodically engineers find themselves with little of importance to do. Hence, a major source of dissatisfaction, Ritti argues, is the sense of underutilization. Simply in terms of the amount of work they are expected to do, engineers would "prefer to do more." (This is not the same as misutilization—the perception that professional people are doing tasks at a lower level of skill. Contrary to common assumption, Ritti finds these two facets related in rather different ways to other aspects of the job.)

Ritti disagrees that engineers' dissatisfaction arises from conflict between professional and management orientations—a mistake compounded by aggregating them with scientists as "professional staff." Whereas such a conflict may exist for scientists, engineers define their personal success in terms of climbing the management ladder and advancing the company's business goals. It follows that personal achievement is defined by their ability to influence management's decisions. Underutilization—not enough to do—prevents the engineer from demonstrating what he can do for the company and frustrates his need to exert influence.

In his analysis, Ritti is careful to distinguish individual-level relationships, which appear within laboratories, from laboratory-level relationships, which appear among the percentages for each of the 12 locations. On the individual level he found that goal satisfaction was well predicted by skill utilization (each index was derived from a separate factor analysis) but only moderately by measures of influence, whereas among laboratories he found a strong ($\rho = .83$) correlation between underutilization and felt lack of "role influence" on technical actions of the company. This contrast underlies his contention that engineers' influence depends little on the leadership style of managers within the lab, and mainly on the position of the laboratory within the corporate context.

Seeking to account for differences in laboratory utilization, Ritti noted that this was not related to size, the percentage of staff with advanced degrees, or the ratio of technicians to professionals. But it was related to relative distance from headquarters; it appeared that more distant laboratories were less accessible to control by corporate management, and hence more open to influence from below.

Other broad dimensions were the "development mission," ranging from exploratory or advanced development to final design or product develop-

ment, and "management environment"—the laboratory management's openness to influence by its staff, as indicated by holding departmental conferences to discuss future activities. In a 2×2 table of the latter dimensions, mean rank on laboratory goal satisfaction was highest in the exploratory labs which used conferences, and lowest in the opposite quadrant.

With respect to practical implications, the author insists that correct diagnosis must precede improvement. He examines and dismisses some conventional solutions: more opportunity for professional activities, such as attendance at meetings, and dual career ladders (technical versus management—both based on the questionable premise of conflicting goals. He is skeptical of offsetting skill obsolescence by additional education, which assumes that the problem and solution are located in the individual, whereas in fact they lie in organizational policy and structure.

Only in the final pages does he offer some cautious administrative guidelines, warning that training cannot be effective without structures which make the desired behavior possible. For example, specialized laboratories permit greater control by top management; unspecialized laboratories permit more flexibility and reallocation of local resources. He nevertheless holds out some hope for development of "an entire management environment in which engineers and lower level managers have the opportunity to confront issues—technical, financial, procedural, or whatever—and to work out solutions based on the most relevant knowledge available" (p. 238).

Denying the myth that engineers make poor managers, he foresees a redefinition of the engineer's role to one of technical leadership over groups of technical and clerical staff, with reduction in the total number of engineers. Finally, in place of management's usual response to difficulty by tightening control, he suggests the systematic assessment of staff perceptions and attitudes through periodic surveys, with the aim of "tailoring the management environment for increased effectiveness and member satisfaction consistent with the nature of the enterprise" (p. 243).

The writing is consistently clear, informal, and persuasive. The format is attractive, with simple tables and liberal use of quotations to emphasize principles. The analysis methodology is a blending of statistical operations with "case-study" insights from feedback of results to company executives over several years. The presentation is aimed more at administrators than at scholars, who may be frustrated by the nonsystematic treatment of variables (definitions are scattered throughout the text), the use of single items to define major variables ("underutilization" and "management environment," for example), and redundancy of argument. But Ritti builds a network of corroborating evidence, and the resulting picture carries conviction.

Michelle and Claude Durand's book, *De l'O.S. à l'ingénieur—carrière ou classe sociale*, examines some large questions. One domain is that of promotional attitudes and job-related training, as reported by 1,300 employees randomly selected in about equal numbers from six occupational categories within five companies in as many industries. A second domain is that of social stratification viewed either as an open society ("horizons ouverts")

in which social levels exist but movement between them is possible, or as a closed society ("horizons fermés") consisting of relatively rigid and antagonistic social classes.

The authors explore connections between these domains. To what extent are workers able to rise in social status through promotion on the job—or does social rise occur mainly through prejob education? How do class attitudes differ at various job levels, and how do such attitudes govern promotion-seeking activity? The data supply clear-cut answers to some of these questions, but on many others the results are ambiguous.

The occupational categories, in roughly hierarchical order, were (1) laborers and semiskilled workers—"manoeuvres et ouvriers spécialisés"—from whom comes the "O.S." of the title; (2) skilled workers—"ouvriers qualifiés ou professionnels"; (3) office workers—"employés de bureau"; (4) foremen—"agents de maîtrise"; (5) technical staff including draughtsmen and chemists—"techniciens," a term implying higher status than the American "technician"; and (6) engineers and middle management—"ingénieurs et cadres," excluding top management—"cadres supérieurs." Incidentally, the French term "profession" carries no connotation of higher education and is equivalent simply to job or occupation; "ouvrier professionnel" is a skilled worker, and "cours professionnel" is a vocational or job-related course.

Two central terms were "promotional behavior" (doing something active to advance) and "training behavior." The latter ("comportement de formation," from "se former," to improve oneself) was defined simply as now taking or having taken courses since starting to work—most of which were taken with an eye to promotion, "en vue de promotion." Sizable differences were found among the six occupational categories in training and in several attitudes toward the job situation, with optimism rising in rough parallel to the hierarchical ordering given above.

A pertinent table (p. 272) lists a half-dozen attitudes that correlated most strongly with job-related training within each of the occupational categories. Curiously, the only correlated item found in all six categories was "social participation"—leisure-time activity in clubs and organizations. Respondents taking training also tended to like the jobs above them, except at the bottom level; those semiskilled workers who took training also perceived social classes as opposed, expressed pro-union attitudes, and were dissatisfied with their job. Apparently these workers were determined to get ahead in spite of their belief in a closed society, not because of their belief in an open one as observed for skilled and office workers, and foremen.

In order to analyze the social class of respondents and their parents, three broad strata ("niveaux socioprofessionnels") were defined across various sectors—industrial, commercial, agricultural, government, etc. Occupations equivalent to laborer, manual worker, or office worker were assigned to the lowest level or working class; those equivalent to foreman and technical staff were assigned to the second level or middle class; and those equivalent to engineers or management to the third level or upper class.

Some interesting results appeared on perceptions of an open or closed

society by the occupational groups within each level. Engineers/managers at the top level gave relatively "open" responses on six items covering this topic, but office workers—assigned by the authors to the working class—gave just as "open" responses. Relatively "closed" responses were given by semiskilled workers—but also by technical staff, assigned to the middle class. These results suggest a subjective class identification different from "objective" class membership, although the authors are critical of the former concept.

Analyses of intergenerational mobility were based on comparison of respondent's social class with that of a parent. Among the three job categories in the working class, a majority reported parent's status the same as their own, although one-quarter of the semiskilled and one-third of the office employees had dropped in relation to parent's level. (By definition, people in this class could not have risen relative to parent.) Among the upper three job categories, one-third to one-half were intergenerationally stable, but close to half in each category had risen from their parent's level.

The authors find the latter results comparable to statistics of the government agency INSEE which indicate that half of the occupants of jobs corresponding to their highest level had a parent at a lower level. But since these jobs represent only 6% of the working force, they offer very limited opportunity for mobility from lower classes. For the authors' sample of engineers/management, only 20% had a working-class parent whereas, if chance alone were operating, 59% would report this. Both sets of data support the view of a society relatively closed at the top level.

The authors ask whether rise in social status relative to parents is more likely to occur by means of education before starting to work, or through on-the-job promotion. Their data show contrasting mechanisms in different parts of the work force. Among foremen, intergenerational rise occurred almost entirely through on-the-job promotion, whereas promotion was progressively less important in intergenerational rise for technical staff and engineers/management. For the latter, social mobility occurred mainly before they started working, presumably through university studies.

A preliminary report of the study appeared in 1965, so the actual data may have been collected a decade ago. By today's standards the methodology appears simplistic. Central variables are defined by single items taken at face value; there is no overt concern with reliability or validity of measurement; multivariate or partial relationships are not attempted.

The authors are sometimes casual in their use of abstract labels for important concepts, with less than meticulous attention to operational definitions. Thus, the term "promotional behavior" appears liberally from page 36 onward, but not until page 255 in the section on "synthesis" is it explicitly defined. Periodically one is left uneasy as to what variables have actually been measured, and how securely.

On the positive side, the authors deserve commendation for the simplicity and clarity of their tables. In place of the usual streams of percentages, they simply plot initials of their job categories on percentage scales, showing at a glance the rank order and degree of differentiation. On a complex table

with several items as well as company location, this system conveys maximum information with minimum confusion.

Social Change in Complex Organizations. By Jerald Hage and Michael Aiken. New York: Random House, 1970. Pp. xvi+170. \$2.95 (paper).

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For several years Jerald Hage and Michael Aiken have empirically explored structural characteristics of complex organizations. Their earlier analyses reported relationships among such variables as formalization, complexity, centralization, propensity to engage in joint programs, routineness of technology, and participant alienation. Now they have made an effort to pull this research together, integrate it with the work of others, and provide a wide audience with a very readable statement directed at three questions: "What organizational characteristics affect the rate of organizational change? What environmental factors account for variations in the rate of organizational change? What is the process of adopting new programs or other changes in organizations?" (p. xii).

While all of these questions are pursued, in the first chapter they narrow the focus quickly. Social change could be thought of as alteration on any variable, but only program change is assessed. This "is defined as the addition of new services or products" (p. 13). Thus, despite the broad title and initial objectives, the central point of inquiry is much narrower—why do some organizations institute new programs more frequently than others? However, this transition is stated clearly, and beginning students can grasp the funneling process wherein researchers shuttle across abstraction levels to derive questions more amenable to empirical analysis.

Now the stage is set for particulars. Seven variables are discussed: complexity, centralization, formalization, stratification, production, efficiency, and job satisfaction. Each variable is then related to the rate of program change; that is, high levels of complexity and job satisfaction are directly related to high rates of program change; high levels on the five other variables are inversely related. While each hypothesis is discussed separately, Hage and Aiken stress interdependence among structural components through an analogy—picture a basket of nervous ping-pong balls. They pick up on this in the third chapter with a brief discussion of dynamic and static organizations. If each variable is related to the rate of program change as hypothesized, then by using all of them we might construct a profile, that is, the structural pattern indicated by scores on the seven characteristics. Imagine seven parallel lines—one for each variable. Presumably, highly dynamic or static organizations will have profiles that approximate a vertical line formed by the row of scores lying at either extreme end of all seven continua (of course, in order to have this consistent pattern, the direction would have to be reversed on the two variables which vary directly).

Similarly, this same analogy provides a way to conceptualize change of systems. That is, using the seven variables, profiles could be drawn at different points in time to ascertain whether the structure of the system was altered. What might cause such a shift in the pattern? Hage and Aiken propose that environmental stability probably is the most important quality. Thus, organizations located in highly stable environments will have more static profiles. Discussion here is brief, and minimal evidence is presented—summaries of four case studies.

The process of program change is divided into four stages: evaluation, initiation, implementation, and routinization. Following a brief discussion of each stage, a case study of a community hospital is used to highlight the types of questions confronted by decision makers at the various stages.

Up to this point the book is tightly organized. In contrast, the final chapter, as reflected in the title "Limitations, Predictions, and Problems," covers numerous topics in a rather disjointed manner. One senses at times that many if not most of the points were selected to rebut various criticisms—received or anticipated. However, the comments help to contrast the proposed model with approaches used by others, for example, human relations, goal, or technology orientations.

While numerous criticisms can be raised, six appear most critical. First, as some of the concepts become operationalized there is major transformation in meaning. For example, "amount of efficiency" (p. 26) becomes redefined as "emphasis on efficiency" (p. 51). Similarly, formalization becomes redefined as two subdimensions—"degree of job codification" and "degree of rule observation." Second, some evidence presented in support of the hypotheses relies on measurement strategies wherein survey data, for example, responses by samples of participants to questionnaires, are aggregated. Is this the best procedure to measure such structural characteristics as formalization? Third, behavioral, as opposed to normative, aspects of organizational structure are not kept distinct. For example, formalization appears to refer to a characteristic of the normative structure until "degree of role observation" is included. In contrast, centralization is defined behaviorally, that is, as the distribution of power which is operationalized through assessing the degree of participation in decision making. Fourth, adoption of new programs is discussed in black and white terms. Degree of program implementation in a behavioral sense, in contrast to official rhetoric, merits much attention—an issue that is largely sidestepped by Hage and Aiken. Fifth, while changes in structural variables within subsystems could be analyzed, the mode of analysis emphasized promotes an assumption that changes are uniform throughout all levels or divisions. Confronted with a more unstable environment, for example, might not participants seek to decentralize authority in one sector while increasing centralization in another? Thus, the simplicity of the "nervous ping-pong ball" model becomes most apparent. Sixth, while psychological reductionism is refuted (pp. 120–24), alternative sets of incumbent definitions are averaged into single scores. This strategy virtually precludes analysis of internal patterns of strains or bargaining and negotiation processes which might emerge

as participants seek to redirect, sabotage, or accelerate the evaluation or implementation of new programs. Thus, conflict, strains, and tensions are underemphasized to the point of nearly being absent.

Immediately upon its publication I used this book as one of several in an introductory course on organizations for advanced undergraduates. After plowing through numerous empirical studies and abstract summaries of alternative theoretical perspectives, many students enthusiastically reported that Hage and Aiken had "pulled everything together." "Why did we have to read all that other stuff when they have the answers right here?" But then we began a more in-depth analysis. Numerous criticisms emerged and became interwoven. Most students quickly lost their initial and rather unrealistic infatuation. Yet, as a learning strategy, the experience proved effective in helping most to grasp criticisms that had heretofore remained illusive. And for several the experience activated their curiosity—they immediately dug into other works on the topic.

In brief, this is a well-written statement that has stimulated some students to think much harder about several significant theoretical and methodological issues. The clarity of presentation permits points of refutation and rebuttal to emerge easily—a characteristic too rarely found. And even "seasoned specialists" will find much stimulation, although, depending upon their own perspective, it may have its primary consequence in the generation of new research questions rather than passive acceptance of the proposed model.

Resources and Production of University Departments: Sweden and U.S.
By Gunnar Boalt, Herman Lantz, and Erling Ribbing. Stockholm: Almqvist & Wiksell Förlag AB, 1972. Pp. 189.

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There has been lamentably little study of who gets what and why in the operation of university programs. And as competition for declining resources intensifies, as accountability pressures from state legislators, students, and others build, the need grows to understand better how resource allocation strategies influence production functions of both academic departments and higher education.

One can accordingly welcome this study of the resources-production relationships in the arts and social science departments of Sweden's five national universities. Although the data are limited to these institutions and to one point in time, the authors somewhat ambitiously address their work to the larger public and private universities in the United States as well. In part 1, a rather short general overview of 10 pages, Herman Lantz sets forth several sets of factors influencing inputs to academic departments and attempts to adumbrate implications of the work for other societies as well. He discusses in four sections how influences from society, from university administration, from academic departments, and from students all play

significant roles in resource allocation and departmental change. A number of provocative propositions are informally elaborated, albeit without supporting evidence or explicit theoretical grounding: (1) predominating societal influences in universities are a function of the stage of historical development, internal political factors at the time, and often external political relationships to the outside world; (2) society's definition of social problem areas affects financial resource allocation and student enrollment, while enrollments bear little relationship to scholarly productivity and research; (3) in America the philosophy of pragmatism has pervasive significance among those who control resources; (4) because European university presidents and deans are usually subject to election, faculty in Europe have been able to exercise greater control over administrative behavior than have their American counterparts; and (5) conflict over the allocation of departmental resources for research, teaching, and service intensifies as faculty evaluation standards shift from research performance to improvement of teaching.

Unfortunately, it is not demonstrated how these propositions follow from empirical study, or whether they are to be tested in the present study. Rather, in part 2 (pp. 17-88), Gunnar Boalt somewhat abruptly begins with correlational studies describing how internal academic variables relate to resource allocation in Swedish social science departments. Drawing on the "summation theory" elaborated in his earlier work, *The Sociology of Research* (Carbondale: Southern Illinois University Press, 1969), Boalt in chapter 2 clearly explains how his central hypothesis (i.e., that "departments and universities tend to expand either their production of students or their production of research, and few of them succeed in expanding both of them substantially at the same time" [ibid., p. 77]) can be tested using a matrix of rank intercorrelations. At the University of Stockholm, for example, 27 department variables and nine social science departments are used to generate 329 intercorrelations. The 27 variables can be divided into three groups: (a) variables pertaining to students, (b) those pertaining to recruitment and performance of researchers, and (c) those pertaining to teaching staff. Two major variable clusters indicate the not surprising finding that departments with research productivity of high quality receive the highest allocations per student, while departments emphasizing teaching receive more students.

In chapters 2-6, Boalt presents and examines rank correlation matrices for the Universities of Stockholm, Uppsala, Lund, Gothenburg, and Umea. In chapter 7 he analyzes the general interaction pattern among the variables in the social science faculties and attempts to use the data measuring different aspects of department output to demonstrate the interaction between and within each university. In chapter 8 he uses these data to compare output variables for each type of social science department (i.e., business administration, economic geography, economics, education, sociology, statistics, and political science) in the five universities.

In part 3 (pp. 89-177), Erling Ribbing presents a similar analysis of the arts faculties in the same institutions. Although there are apparent differ-

ences between Swedish social sciences and arts departments, Ribbing, using the same methodological approach, demonstrates that his hypotheses deduced from Boalt's summation theory are confirmed, that there is in fact a compensation mechanism working between teaching and research.

In part 4 (pp. 178-84), the authors summarize some 18 findings from the "unified value," "compensation," and "compromise" patterns produced by the study. A number of these are provocative and will certainly be of interest to academics anywhere.

In sum, while this study contributes new knowledge of Swedish university conditions and a number of new propositions, it is not a very successful book. The introduction, for example, fails to properly identify and define concepts, to provide a rationale and framework to give meaning to the data and analyses that follow. Sound editing would perhaps have helped produce a more unified collection of parts.

Other editing weaknesses include a misleading title (this is *not* a comparative study!) and numerous spelling and syntactical errors. This is a pity, for the work has both substantive and methodological merit.

Organization Research on Health Institutions. Edited by B. S. Georgopoulos. Ann Arbor: Institute for Social Research, University of Michigan, 1973. Pp. 418.

Rodney F. White
Trent University

Any attempt to review an edited work containing the contributions of a number of authors faces certain problems, but this book presents additional difficulties for a number of reasons.

In the first place, it is the report of a two-day conference held at the University of Michigan in May 1970, and the format includes not only the revised papers presented at the conference but also the edited critiques and participant discussions which followed the papers. This means that a reviewer is being asked to evaluate a process rather than a product.

Second, the several contributors utilize different methodological approaches, write from a variety of perspectives, and deal with different assigned topics. If one includes the interchanges from the sessions and the editor's contributions, there are relatively few questions in the area of health organizations which are not at least touched upon somewhere in its pages.

Finally, the book is addressed to two different audiences—social scientists and researchers, on the one hand, and health professionals and practitioners, on the other—so one must consider its potential interest for both groups.

The report is not just a loose collection of diverse products, since the editor, Basil Georgopoulos, was also the director of the project out of which the conference developed and solicited the papers which were presented. In addition, he acted as chairman of the conference and provided a lengthy introduction to the report.

However, the book comes across as more of a progress report on the project than a final report, since the project's objectives were rather ambitious, and were not yet brought to fruition at the time of the report. These include producing a synthesis of existing knowledge in the field of health organizations and institutions, developing an inventory of needed research in the field, and providing some directions for actions to harried practitioners in the "problem-ridden health care delivery system" (preface).

The contributors of the working papers which form the core of the book were asked to direct their efforts toward a "critique and synthesis of Hospital Organization Research in the 1960's," and each was encouraged to select some aspect of the field which was of particular interest to him. The resulting papers vary considerably, some offering a survey of the field, others proposing a new conceptual framework, and still others concerned with practical applications. Most of the authors have the advantage of considerable experience in hospitals, and some have been in full-time positions, in addition to carrying out research projects. Each paper ends with a fairly extensive list of references.

The contributors to this volume operate from a variety of theoretical perspectives running from general systems theory, with a stress on "open systems," through structural comparative approaches and ecological analysis, to the more social-psychological and traditional occupational concerns. Perspectives adopted include those of researcher, consultant, physician, administrator, patient, and community being served.

Some of the issues explored are the structuring of hospitals and health institutions, including the special characteristics of these types of organizations; mechanisms of control within the health "industry"; the roles of health professionals and administrators in these institutions; and an evaluation of the health care services which these organizations are providing.

Since one of the objectives of this project was to review the literature of the last decade, much that is in the book will not be new to those familiar with that literature, but a lot of it is now brought together in a single place. Of the newer ideas presented, I found Mayer Zald's call for the use of a "political economy framework" (p. 389) and A. R. Kovner's discussion of the administrator as change agent (pp. 373 ff.) to be the most suggestive.

Regarding the other objectives, the success reported is, at best, partial. Some authors attempted a synthesis in their chosen problem area, but the book, in general, remains unintegrated. Few of the proposals for needed research are specific, and they tend to be in fields which have proven difficult in the past, such as quality of care and determinants of turnover.

As far as application to current problems in the health system is concerned, a quotation from the editor states the situation fairly clearly: "Existing knowledge, both theoretical and practical concerning the above problems and organization behaviour more generally is still inadequate and unsatisfactory from the standpoint of structuring the organization so as to maximize the system's problem solving capacity" (p. 44).

It seems to me that the failure to include any reports on research other

than that done in the United States unnecessarily restricts the scope of the book. Also, it seems misleading to describe the project's area of concern as social-psychological knowledge when most of the reports focused on social-structural variables.

As a report of progress in the field, I found much of the book both interesting and stimulating, and it offers the reader the opportunity to be part of a process of stocktaking. However, I regard the editor's general orientation toward increasing organizational effectiveness, adaptation, and cost reduction as limiting the scholarly value of the work.

Learning from Experience: Toward Consciousness. By William R. Torbert. New York: Columbia University Press, 1972. Pp. xiii+234. \$10.00.

Dean MacCannell

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This book is on the increased understanding of self that T-groups are designed to generate. The author's theory of consciousness is placed most prominently before the reader. It is also this theory that would likely catch the eye of a sociologist, as it is built out of elements that have been introduced into our recent theorizing: phenomenology and cybernetics. Torbert's exact reason for combining T-groups, cybernetics, and phenomenology is not made explicit, so it must appear as a kind of "natural outgrowth" of the 1960s' stress on experimental social relationships, the electronic media, and consciousness raising. Torbert eventually quantifies the self-knowledge that is gained in the course of T-group sessions, and he uses phenomenology and cybernetics to align the instrument he constructs with the message of the avant-garde of the past decade: we ought to be more involved, more open minded, and less prejudiced in our daily affairs.

The model that Torbert presents divides experience into three levels, with each level controlling the next level down. *Behavior* (level 1) is monitored and directed by *cognition* (level 2), and cognition can be controlled by level 3, which is *consciousness*. Torbert suggests that most individuals are caught between levels 1 and 2 and must be helped to overcome socialization and arrive at consciousness, the realm of "genuine learning" (p. 61), "truth" (pp. 233-34), and "authentic experience" (pp. 15-17).

The theory behind books which are intended to be therapeutic is often derivative of other disciplines. Torbert's work fits this pattern. His use of cybernetics is confined to a few concepts such as "feedback" which are widely known. Evidently his studies of Husserl did not go beyond the first *epoché*. He objectifies consciousness in a way that directly opposes Husserl's findings in the phenomenology of inner-time consciousness. Since phenomenological investigation takes as its starting point that consciousness is always consciousness of something, it should be noted that Torbert's realm of consciousness is technically antiphenomenological. The book also con-

tains a critique of contemporary science on grounds that are reminiscent of Marcuse's, but this chapter (3) is too simplistic to be of interest to specialists in the sociology of science.

These flaws are, however, probably unimportant to the main intended audience of the book; people in personnel work and public relations. Apparently one of the unanticipated consequences of the recent democratization of almost every modern social establishment has been a set of new strains on the men who manage them: it is still necessary for managers to "influence" others, but some of the cruder forms of the application of bureaucratic power have been shelved. The T-group movement is part of an alteration of the overall design of power relations in the American middle class. Specifically, Torbert's book provides a justification for relatively harmless delinquent bureaucrats who must sidestep social norms and constraints in order to reach successfully their goals in business, industry, government, and more recently in interpersonal affairs. His justification is now a standard one: the need for "experiences," expanded "consciousness," and "freer" social relationships. What distinguishes Torbert's work is that he operationalizes this justification empirically, and he assembles impressive scientific and philosophical supports for his operationalization. Max Weber commented that the early Protestants did much the same.

The writings of the early Protestants signaled the coming of modernity, and, I think, books like Torbert's may be signaling the advent of a post-modern society. His descriptions of "consciousness" strikingly resemble descriptions of the Protestant experience of grace, and, in his conclusion, he reproduces a letter of his to a friend which contains a profession of faith: "So deeply woman, the universe resounds in you involuntarily in its full cacophony; your task: somehow translate it through your life into tune (as I see you so magnificently beginning to do . . . with me). . . . Nothing could be more material than to use the body for acquiring a right sensation of God" (p. 234). Sociologists are, of course, familiar with the basic ingredients of statements like this: a combination of interpersonal relations, group solidarity, and transcendent religious experience. But these elements have been transformed into their typically modern variants. Religion is stripped of tradition. Here it is reduced to a single term, "God." The group, like the horse, has become a plaything. In modern cultural movements, groups are artificially kept alive for their recreational value: T-groups, encounter groups, tour groups, rock group "groupies," etc. And no matter how serious the parties to them take them to be, modern interpersonal relations have become fleeting, accidental, arbitrary, and weightless from the standpoint of the total society. It is exactly the unimportance to modern society of *Gemeinschaft*-like relations and transcendent personal experience that makes men like Torbert so concerned about the authenticity of their relationships and experiences. The connection of solidarity and religion has been profoundly disturbed in the modern world, and Torbert has taken on himself (he is not alone in this) the difficult and probably unrewarding task of trying to put transcendence and sociability back together again.

The Drinking Man. By David C. McClelland, William N. Davis, Rudolf Kalin, and Eric Wanner. New York: Free Press, 1972. Pp. xiv+402. \$10.00.

Jeffrey W. Stone

Pomona College

The Drinking Man is the report of a series of studies designed to isolate the psychological factors that motivate excessive drinking behavior. The studies reported here do not comprise a unified research project; rather, the volume reports a series of loosely connected research projects conducted by David McClelland and others over a 10-year period. The initial studies suggested, and subsequent studies were designed to test and refine, a theory about why people drink. Four aspects of drinking behavior were examined in the course of these studies: (1) the psychological states that influence drinking behavior, (2) the effects of alcohol on fantasy, (3) the content of folktale themes associated cross-culturally with heavy-drinking societies, and (4) the attitudes and personality needs of alcoholics.

To begin with, the title of this book is meant to be taken literally. The research described here does not concern drinking women or drinking people, but drinking men. With this qualification in mind, the authors' theory rests on the finding that men have motivational power needs, a finding which was the result of personality tests administered before, during, and after drinking sessions in "natural" settings. These power needs are of two types. The first is called "socialized power," referring to the expression of male assertiveness through socially structured and organized channels, such as office holding, competitive sports, and other instrumental activities. The other "power cluster" is called "personalized power," referring to the expression of male assertiveness through personal aggrandizement and dominance. Drinking serves to increase one's power fantasies, especially power fantasies of the latter type; and when high personalized power needs are combined with low levels of inhibition, heavy consumption of alcohol is one characteristic outcome. The need for personalized power apparently derives from a combination of sources: strong cultural demands for male assertiveness, low support for an assertive male role, and perceived lack of outlets for more socialized and instrumental power expressions. According to the authors, the alcoholic drinks to reinforce his sense of personalized power. Drinking creates a subjective sense of such power through its effects on power fantasies. It can be argued that other activities also allow expression of personalized power, such as gambling or fighting, and the authors note that "drinking is one important part of a cluster of actions which is a principal manifestation of the need for power" (p. 119).

These conclusions are suggested by a series of experiments designed to measure psychological and personality states during drinking sessions in a series of bar and party sessions ostensibly set up for some other purpose. Amounts of alcohol consumed for each subject were estimated by trained observers, and various aspects of verbal interaction were coded according to a complicated scheme in part derived from the TAT. Subjects were

Harvard students for the most part, although a sample of older men was included in some of the experiments.

Realizing the limitations of such experiments and such subjects, and in an attempt to isolate the cross-cultural experiential consequences of drinking, the researchers made content analyses of folktale themes taken from societies that have been characterized as heavily drinking by psychologists and anthropologists. It is noted that members of hunting societies tend to drink more heavily than members of agricultural societies, which has been traditionally explained in terms of the economic insecurity of the former. This explanation is challenged on the grounds that hunting societies often have more stable food supplies than agricultural societies and that there is no real evidence that drinking serves to reduce anxieties caused by environmental insecurity. Instead, the authors find that folktales from heavy-drinking societies can be interpreted as expressing an interest in a magical, noninstrumental type of potency or power, which is very similar to the personalized power cluster described earlier. Furthermore, the authors find that sober societies exhibit more solidarity, stress respect and inhibition in personal behavior, and give greater support to the male role than heavy-drinking societies. In some cases, the latter expect men to be assertive yet obedient; in other cases an assertive male role is expected but not supported. In such situations, drinking does not consist of coping behavior so much as it provides the very sort of magical, noninstrumental potency experience that men are sensitized to through cultural folk themes. The authors are describing a social syndrome that encourages concern with both personalized power and heavy drinking by simultaneously requiring and undermining male assertiveness in conjunction with low emphasis on interpersonal inhibition and respect. Thus, it is claimed that a certain amount of convergence exists between the experimental and cross-cultural data, lending support to the power theory.

The claim is made by the authors of this book that their power theory explains data on drinking and alcoholism among men at least as adequately as other, more traditional theories. One of the more interesting parts of this volume is their discussion of alternative explanations in light of the evidence supporting the power theory. The major existing explanation of drinking has been the traditional psychoanalytic view that the need for dependency, nurturance, or oral gratification either causes or is affected by consumption of alcohol. Little evidence is found to support this explanation, even when the experimental data are analyzed in an attempt to find a correlation between such factors and drinking. The additional argument is made that the evidence usually cited to support the psychoanalytic theory is only indirect and can be interpreted to support other theories as well, including the power theory. Likewise, no support is found for the idea that alcohol is consumed in order to reduce anxiety, as it is only at quite high levels of consumption that anxious thoughts are found to show any decrease. This is consistent with the finding that, cross-culturally, expression of anxiety in folktales is inversely related to drinking.

Two interesting applications of the power theory are presented. The first

is a discussion of how personalized power needs might be satisfied in ways other than by drinking. Suggestions here include redirecting one's power drives toward self-control or asceticism, accepting a power outside the self (e.g., religion, Alcoholics Anonymous), accepting a less aggressive masculine ideal, and aggressive acting out of power needs. The other application involves a test of the power theory by measuring its therapeutic success. In a pilot study, an attempt was made to socialize alcoholics' power needs through a teaching program. Some temporary therapeutic gains were made, although the study was too brief and limited in scope to provide a really meaningful test of the theory's therapeutic value.

The research reported in *The Drinking Man* is interesting not only because of the power theory, but also because of its use of experimental and cross-cultural data to find support for that theory. The book, however, is likely to frustrate many sociologists. The methodology used in these studies is tedious to follow and difficult to evaluate, especially for sociologists interested in alcoholism as a form of deviance and in qualitative research methods. Those interested in the latter are also likely to cringe at the authors' lack of concern for the extent to which their research techniques intruded upon or reacted with subjects' responses in the supposedly "natural" settings. But undoubtedly the factor that will most limit sociologists' interest in this volume is the fact that it is a psychological study, and as such does not set out to look at the sociological dimensions of drinking behavior, such as the conscious reasons for drinking, the ways drinking is embedded in the meaning structures of various social situations, or the use of alcohol by competing groups as an issue or symbol in a more general struggle to define social reality and morality. If the power theory proves correct, however, it will provide an interesting psychological correlate to conflict theory in sociology. It calls our attention to the tendency of individuals to pursue their own interests and to the latent conflicts inherent in most interpersonal situations. Those interests and conflicts are typically seen as derived from social structure, but it will be interesting if in the future we can show some degree of continuity between power needs, on an individual personality level, and expressions of organized personal and group interests, on a social structural level.

Smoking: A Behavioral Analysis. By Bernard Mausner and Ellen S. Platt. New York: Pergamon Press, 1971. Pp. xiii+238. \$14.00.

Bruce C. Straits

University of Georgia

The strength of this book lies with the authors' decision to present a full and honest account of the first eight years of their ongoing program of research on cigarette smoking. It is a true research diary, replete with the numerous false starts and the infrequent successes which characterize the research experience, and not a glossed-over "cumulative record" of positive

results. Anyone who pursues or contemplates the study of health-related attitudes and behavior would do well to read this book before going on, as it is a useful guide for sorting out potent from ineffective strategies of theory and method. For other audiences, especially those in quest of a concisely organized review of the behavioral aspects of smoking, the book's organizational style may be unappealing. Descriptions of the authors' various studies, explanations of conceptual development and measurement, and reviews of the literature are scattered throughout the book with cinematic previews and flashbacks.

The first half of the book is devoted to factors which support smoking, including affective rewards, social affiliation, and role identification. Scores of surveys have disclosed that self-reported reasons for smoking display monotonous interstudy regularity: cigarettes seemingly give pleasure, reduce nervous tension, stimulate vitality, habituate, addict, enhance self-image, and facilitate social interaction. The possibility that the medley of reasons given for smoking may be peculiar to the method of data collection (usually retrospective surveys) is effectively challenged by Mausner and Platt, who extract compatible patterns of supports for smoking from a diversity of research vehicles (focused interviews, diaries of events and feelings accompanying the smoking of each cigarette, projective tests, sociometric choices, and various structured instruments).

By focusing on the environmental and intrapersonal circumstances of individual acts of smoking, the authors empirically refute a number of entrenched notions, including (1) the belief that habitual smoking is affectively neutral; (2) the concept of types of smokers, each with a specific dominant need fulfilled by smoking; and (3) the belief that intrapersonal supports for smoking are closely linked with environmental circumstances in affecting smoking behavior (an apparent exception is the relaxant role of cigarettes in tension-inducing environments). As these findings are largely based on small and unrepresentative samples (for example, college students, church groups) and occasionally unsophisticated data analysis, their generalizability may be limited.

The second half of the book reports the results of an experiment in which role playing is used to induce change in the smoking habits of male college students. Drawing upon the considerable evidence showing the success of role playing in affecting attitudinal change and the influence of physicians in modifying their patients' smoking behavior, Mausner and Platt asked their subjects to act out the role of a "patient" or a "doctor" in a scenario in which the "doctor" presents clinical evidence (e.g., reduced pulmonary efficiency) that the healthy young "patient" is damaging himself by continuing to smoke. The subjects were randomly assigned to four groups, listed in decreasing order of hypothesized postexperimental change in smoking attitudes and behavior: "doctors," "patients," "observers" who rated the performances in a role-playing episode, and "controls" who were only exposed to pretest and posttest instruments.

Reported reduction in smoking five days after the experiment was relatively high among the "doctors" and "observers," but relatively low among

the "patients" and "controls." The decreased smoking was not accompanied by substantial shifts in attitudes toward smoking, nor was the role playing effective in maintaining long-term reduction (six-month followup) of smoking. These results are subject to several interpretations, especially since there are sources of confounding in the intergroup comparisons (e.g., the "doctors," but not the "patients," were introduced to the content of the clinical tests prior to the charade; all groups except the "controls" were given a booklet on ways to change smoking behavior). Although the differential effect of the role playing on the "doctors" and "patients" might be attributed to cognitive dissonance arising from the former's antismoking advocacy, a model of defensive avoidance of fear-inspiring messages is more consistent with the lack of change in smoking among the "patients," for whom the similarity of the "patient" role to their own situation may have generated anxiety and defensiveness, and the reduced smoking among the "observers" and "doctors," whose involvement in their respective roles of observing or communicating the antismoking arguments may have lowered their defensiveness.

A wealth of tantalizing findings and inferences appear in the authors' extensive analysis of pretest correlates (personality measures, smoking attitudes and beliefs) of postexperimental smoking patterns. Of particular interest is the subjects' expressed confidence in their ability to quit, which not only effectively predicted changes in their smoking but was also systematically related to their behavior throughout the role-playing charade. In contrast, subjects who expressed strong dependence on cigarettes for the fulfillment of important needs (for pleasure, tension reduction, relief from cravings, and the like) were relatively unaffected by the role playing. These and other findings lead the authors to recommend that smoking modification programs should focus on assuaging the perceived disadvantages of not smoking rather than emphasizing the dangers of continued smoking.

Sing a Song of Social Significance. By R. Serge Denisoff. Bowling Green, Ohio: Bowling Green University Press, 1972. Pp. xii+229. \$6.50.

Edgar Z. Friedenberg

Dalhousie University

Sing a Song of Social Significance is a book that succeeds, on the whole, in its rather modest purpose of providing a detailed and accurate account of the development of politically expressive music in the United States from the Depression of the thirties to the present. I call this purpose modest because the author limits his attention to what can be clearly established from the lyrics of the music and verifiable facts in the often obscure lives of its creators. He says very little about the character and quality of the music itself, as distinct from its texts, although he writes of it respectfully and with evident liking. Nor does he relate changes in the music to subjective but fundamental changes in the social climate it expresses and

reflects, except where this refers to specific events. Denisoff notes, for example, the aggressive bitterness of Jefferson Airplane's late-sixties album "Volunteers"; and quotes the lyrics of Crosby, Stills, Nash, and Young's "Ohio," which savagely commemorates the Kent State University slayings. And he gives Steven Stills long-overdue credit for having created with the Buffalo Springfield—a great precursor of CSN&Y—the classic protest song "For What It's Worth" in response to the 1966 Sunset Strip police violence, noting correctly that this song became more popular with rock audiences nationally than any other equally explicitly political song by a rock group. (In fact, the opening bars of the music are still used to conclude Canadian Broadcasting Corporation news specials—presumably in reference to the opening lines of the song.) But Denisoff does not discuss the emotional development of the Airplane's music from the joyful psychedelic exuberance of early albums like "Surrealistic Pillow" through the darkness of "Crown of Creation" to the eventual militance of "Volunteers"; or the warmth and naïveté that characterized the music of the Springfield after "For What It's Worth" made them famous, which became transfigured into the unearthly peace of albums like Crosby, Stills, Nash and Young's "Déjà vu." Another offshoot of Buffalo Springfield, Poco, has continued in the original joyful style, but its work can no longer be called songs of social significance while the Springfield, even in their happiest period, continued to produce songs like "Broken Arrow," a very moving work that relates the mythic plight of the American Indian to that of middle-class rebellious youth—an exaggeration, certainly, in demographic terms, but not, as Kent State was later to show, in terms of the intensity of the hostilities involved. The development of rock music through the sixties tells the listener, precisely through such often heavy changes and shifts, more about how social change made itself felt, at least by those who made and responded to the music, than any other form of document.

Denisoff hardly seems interested in assessing such subjective evidence: his interests in this book are quite narrowly political and historical. The Beach Boys, for example, who kept their cool and their relevance the whole time and remained above the conflict while commenting shrewdly on it, are not even mentioned. Neither is Frank Zappa, the most coldly penetrating and sardonic social critic to appear on the rock scene; Denisoff does mention Zappa's group, the Mothers of Invention, but in a way that makes it sound like a counterpart of Country Joe and the Fish, which it resembles about as much as Iago does Masetto. Denisoff, in short, consistently separates the political and moral components of the works whose social significance he is examining; and only the former seems to interest him.

This book's major emphasis, however, is not on rock itself but on the entire development of music whose lyrics attempt to influence social or political processes. Rock, even when it is being most topical, often does not; the best rock music maintains a Zen-like openness to and reluctance to oppose actively even the most tragic events that figure in it. Denisoff, I think, finds this puzzling or distasteful as a social attitude; he can and does

deal with what he calls "anti-protest" music like some of the Beatles' anti-revolutionary lyrics, but not with music that notes, describes, and accepts evil, like Captain Beefheart's "Troutmask Replica"—a masterpiece, which goes unnoticed. Where Denisoff is most useful is in tracing the intricacies of protest music of the fifties, which reflected all the fissionable elements that composed the American Left; he is excellent in showing how protest rock is rooted in jazz and in the earlier, verbally banal but often musically expressive rock and roll of which Presley remains the symbol. The book is concrete and, despite poor use of language, readable—though Denisoff may arouse needless anxiety in his academic readers by statements like: "Doris Day's million seller perhaps captured the political tenure of the year [1959] 'Whatever Will Be Will Be (Que Sera Sera)'" (p. 30). *Sing a Song of Social Significance* is altogether a valuable reference work and solid pioneer study that will facilitate subtler and more far-reaching work on the theme of protest in popular culture.

The Hollywood TV Producer. By Muriel C. Cantor. New York: Basic Books, 1971. Pp. xiii+256. \$7.95.

Stephen F. Barsky

Temple University

The Hollywood television producer must be able to deal with the expectations of several interested parties in the course of his work. These include his co-workers, a network (or a syndication company), the Federal Communications Commission, private interest groups, the sponsors, and the viewing audience. Of these parties, probably the network is the most important; the expectations of most of the remaining groups are mediated through the network and its control of programming. Cantor concludes that the producers' co-workers and the audience, along with the network, constitute the most important influences on programming.

Graphic narrations of this balancing act can be found in Merle Miller and Evans Rhodes' *Only You, Dick Daring* (New York: Sloane, 1964) and Stephen E. Whitfield and Gene Roddenberry's, *The Making of Star Trek* (New York: Ballantine, 1968). These books illustrate the modifications in story content and form necessary in the compromise process of producing television programming.

Professor Cantor's task in this book is to analyze the process through which producers select topics for and assist in the creation of filmed dramatic programs for television. She begins with an explanation of the mechanics of TV film production and then describes the professionalization of producers. In a typology which recurs sporadically throughout the book, Cantor finds that there are three career patterns typical of producers working in Hollywood TV film. (Hollywood is the locus of much of the current television film production, although changes to other locations are apparently coming.)

These three types, derived from interviews with 59 working producers, are the filmmaker, the writer-producer, and the old-line producer. Filmmakers tend to be young and usually trained in academic settings for careers in communications. They are aiming to become independent motion picture producers. Filmmakers are essentially learning their trade and have both relatively little creative control and little dispute with agencies which attempt to control their creativity.

Writer-producers are mostly middle-aged and have experience in free-lance script writing, both for pictures and for TV. These producers see themselves as chief writers rather than as filmmakers, and seem to be the most volatile in terms of creativity and conflict with censors, networks, and production companies.

The old-line producers are the oldest and most successful of the producers Cantor studied. Their conflicts with the networks are not over ideals but rather over stories and casting. Unlike the filmmakers and the writer-producers, the old-line producers do not believe that they have subordinated their values to the need for money by producing for television. Their life work is TV, and not independent filmmaking or free-lance writing.

After discussing the interrelationships among the three kinds of producers, on the one hand, and the writers, directors, actors, and networks, on the other, Cantor uses as a case in point producers' reactions to the networks' changes in policy after violence on television became a public issue. This topic leads into a discussion of producers' perceptions of the television audience, and here Cantor finds that most producers (regardless of career line) apparently perceive their audience as rather unsophisticated and shape their program content to fit the audience. Writer-producers believe that the potential audience is a bit more sophisticated than this (i.e., more like the producers themselves) and attempt to introduce more sophisticated story and development ideas into their program content.

Finally, Cantor herself provides the "peg" for this review. In a two-page statement at the end of the text, she speaks of changes taking place in television in terms both of career lines for producers and of the relationships among the networks, producers, and the federal government. This book was written before the rule on changes in access became effective, and this change might be significant. Briefly, the access rule removed one half-hour from network access to prime-time local programming in an attempt to foster local initiative. The half-hour of local access has largely been filled by syndicated programming, and this should provide jobs for some additional producers.

The book also predates two other important changes in television productions: the introduction of videotaped (and especially live-on-tape) programming, and possible limitations on reruns by federal edict. Concurrent with these three changes in programming came changes in producers' apparent perceptions of the audience. Relatively sophisticated situation comedy programs like "M*A*S*H," "All in the Family," "Maude," and the short-lived but high-rated "Bridget Loves Bernie," as well as dramatic programs on male homosexuality and other formerly forbidden topics, seem to

indicate that producers are beginning to understand that urban and suburban viewers are not as unsophisticated as they had believed at the time Cantor interviewed them. Prime-time showing of these programs, along with almost uncut films like *Patton* and *Who's Afraid of Virginia Woolf?*, make the producers' views as portrayed in this book seem anachronistic.

In a rapidly changing area like television, this fault could not be avoided. In fact, this review itself may be mostly past history by the time it is published. But more than this, I find that Cantor has written a boring book about a fascinating topic: the mechanism whereby story ideas are made into dramatic and comedic television entertainment. I must candidly say that Cantor's book took me a long time to read; I could not pick it up and read more than a few pages at a time before putting it down and wishing that the author had been able to write more interestingly.

In sum, this is a rather dull, dated book about an important and interesting area of creativity.

Migration and Urban Development: A Reappraisal of British and American Long Cycles. By Brinley Thomas. London: Methuen & Co., 1972. Pp. xvi+259. \$13.50 (cloth); \$6.75 (paper).

Joseph J. Spengler

Duke University

This book is a sequel to Brinley Thomas's *Migration and Economic Growth* (Cambridge: Cambridge University Press, 1954), the most illuminating economic study of international migration to appear up to that time. In the book under review Thomas reexamines the validity of his earlier thesis that British and American long cycles were inversely related by mechanisms underlying international migration and capital movements in the age of mass migration. He now extends his account beyond the Atlantic community, his main concern in his earlier work, to the relation existing between international migration and internal migration and the course of urban growth.

The author draws effectively and interestingly upon the results of scholarly research since 1954 and interprets these results in terms of national and international models. This book is a notable contribution to our understanding of the part played by migration in the economic development of nations. It is also an informative presentation of the statistical tables and charts with which the book is packed. It is well indexed and includes a good bibliography.

Thomas's findings are of two sorts, those relating to the Atlantic community and those relating to brain drain and recent interrelations between internal and external migration.

Respecting the Atlantic economy, his main findings run about as follows. (1) Interaction between the American and the British economy accounts for the long swings in population, capital growth, etc., in the United States between 1870 and 1913. (2) Internal and external migration produced an

alternating pattern in British and American housebuilding cycles. (3) Between 1860 and 1913 international migratory and capital movements were highly synchronized. (4) The post-1945 course of events is a reverse of the course that terminated with World War I. (5) Monetary forces have been significant but not dominant. (6) While the Kuznets cycle which flourished during the age of mass migration, 1840-1924, expired with the termination of the migration link, demographic cycles will continue to exercise important economic and social effects. Moreover, with collapse of the international monetary regime, cyclical interaction between the United States and the European Economic Community could emerge.

In the last three of his seven chapters Thomas turns to internal migration in America and Britain, its connection with international migration, and the emergence and effects of "brain drain." Chapter 5 focuses mainly upon Negro migration in the United States, its long-run interaction with foreign immigration, and its significance for Negro natural increase and urban development. Chapter 6 examines migration and regional growth in Britain, shifts in Britain's population and their relation to building cycles and manufacturing development, the particular experience of Wales, the impact of migrants from the West Indies and South Asia (and now Africa), and problems seemingly impending.

The final chapter, on the dynamics of the brain drain and its determinants, a subject catapulted into importance with the "discovery" of human capital, deals with Britain, Canada, and especially the United States, whose large public investment program was the major source of demand for skills in 1950-70. Countries losing skilled emigrants are divided into those whose economies are not adversely affected and those whose economic growth is retarded by the outflow of skilled personnel. In the former category fall countries which train more skilled people than they are willing or able to absorb (e.g., India); in the latter, economies (e.g., United Kingdom) whose development is slowed. The United States is the prime example of a beneficiary of brain drain. Consideration is given to how the brain drain may be averted as well as to economic forces that may be slowing it down.

Negroes in Britain: A Study of Racial Relations in English Society. By Kenneth Little. London: Routledge & Kegan Paul, 1972. Pp. xi+309. \$13.50.

Harold Wolpe

Polytechnic of North London

Negroes in Britain, which first appeared in 1948 and is now republished with some, unspecified, revisions and a new introduction by Leonard Bloom, is an account of a coloured community living in a maritime setting in Cardiff. The research, which resulted in this book, had a rather curious origin. Professor Little went to Cardiff to pursue research in physical anthropology but changed his "coat and became instead a social anthropologist"

(p. vii). As he says: "The original intention was not to examine relationships between white and black, but to measure the head of the latter's children. What I immediately experienced in Cardiff was a slice of the reality about which my African friends in Cambridge had told me, and so I decided to pursue my research in a sociological form" (p. vii).

In the opening chapter the author poses a question which is central to his concern. He asks: "Are we to look upon the problem [of the study of the community] as one of behaviour in an urban environment and no more; or as a study in race relations, or, indeed, as both?" (p. 47). Contrary to the approach adopted in much of the later work on race relations, Little starts from the position that race situations cannot be treated as autonomous of the structure of the society as a whole and that, in turn, this structure cannot (in the case of Britain) be understood outside of its colonial relationships. He stresses this theoretical standpoint in his preface to the present edition of the book: "I should like to reiterate my theoretical position which is that racial relationships are to be conceived of as a function of the wider socio-politico-economic system in which they occur" (p. ix).

In his attempt to analyze the coloured community of Cardiff from this perspective, Little organizes his data around three major areas. He begins with a brief and rather disjointed general description of the "historical antecedents and occupational background of the community" (pp. 47-48), which includes an account of the coming of the coloured people to Cardiff and the resulting competition with whites over jobs, some demographic and ecological data ("the maritime setting of the community," p. 55), as well as information on social amenities and health. This is followed by a discussion of the "social structure and general sociology" (p. 48) of the coloured community. Here, the different ethnic groups which comprise the coloured community are distinguished, and various aspects of life within the community—status, etiquette, occupations and wages, social and political control, family life, religious outlook, and education—are briefly described in a rather ad hoc manner.

The third most important area is concerned with the relationship of the community "to and its position as part of the surrounding matrix of the larger British society" (p. 48). This is the point at which Little wishes to establish the crucial link which would explain the situation of the coloured community within the British social structure. It is extremely difficult to extract from the book any coherent conception of the nature of the British social structure or of this link, but Little's argument seems to proceed as follows. The author states: "Among the more important factors which condition and resolve the sociology of the coloured community there is little doubt that the attitude of the outside world in terms of *colour prejudice is the most significant*. The historical implications of this point will already be clear, *particularly in bringing about the segregation of the coloured folk in the dock area*" (p. 126, my italics). This seems to suggest that the important element of the wider social structure is to be found in the race attitudes of the British people, and the crucial issue, for Little, now becomes

one of determining how these attitudes arose. There follows an extremely unsystematic and anecdotal history-of-"ideas" account of the development of English racial attitudes towards coloured people which is related, only in a diffuse and vague way, to colonialism. There is virtually no analysis of the structural conditions of British society or of the colonial relationship.

Little's attempt to locate the Cardiff coloured community and race relations within the historical development of the British social structure thus largely reduces itself to a highly idiosyncratic description of English race attitudes, based on an arbitrary selection of data, which are held to be the cause of structural conditions (for example, segregation).

In the result the book offers little of either theoretical or empirical interest. This being the case, it might be asked why it should have been republished. Perhaps the answer lies in the fact that it was, indeed, as is claimed on the dust jacket, a "pioneering study of race relations in Britain." One wonders, however, whether this by itself is sufficient to justify its reissue.

The Blending of Races: Marginality and Identity in World Perspectives. Edited by Noel P. Gist and Anthony Gary Dworkin. New York: John Wiley & Sons, 1972. Pp. xii+289. \$9.95.

Peter I. Rose

Smith College

This book was conceived by the senior editor, Noel Gist of the University of Missouri, and Elizabeth Wittermans of the University of Hawaii. Both had done extensive research on the "marginal" peoples (Gist on Anglo-Indians and Wittermans on Indonesian Eurasians) and felt it appropriate to pool their knowledge with others interested in similar problems. For various reasons Dr. Wittermans could not participate in the editorial chores, though she did submit a splendid paper on her Indonesian research. Anthony Gary Dworkin, a colleague of Gist's and a specialist on Mexican-Americans (another "blended" category) came on as co-editor. Together they put together the final product, which contains essays of their own and Tissa Fernando's summary of "The Burghers of Ceylon," Dennis Hilary Gouveia's "The Coloreds of Guyana," Betty Watt's study of the part-Aborigines of Australia, Richard Slobodin's "The Metis of Northern Canada," a paper by Bernhard Hormann on Hawaii's mixed peoples, one by Donald Pierson on Brazil's, Peter Munch's "Race and Social Relations in Tristan da Cunha" (that famous little island in the South Atlantic) and Brewton Berry's summary of his book *Almost White*, a report on those U.S. citizens he calls "mestizos" (part red, part black, and part white).

Gist and Dworkin's introductory essay is an attempt to set the stage, to highlight the situation(s) of "racial hybrids," to explain varied reactions to them and their own responses to marginal status. The authors rely on earlier ideas of Park and Stonequist, and the well-known commentaries, critiques, formulations, reformulations, and tertiary glimpses of Goldberg,

Golovensky, Antonovsky, Green, Hughes, and H. F. Dickie-Clark (who also has an essay here, on the "Cape Coloured" of Durban, South Africa).

Offering a paradigm of marginality with operational definitions of three types (political, social, and cultural), Gist and Dworkin classify the peoples described by those participating in or, better stated, contributing to the symposium. I qualify this purposely because it accounts for one of the major problems with this book, that is, the lack of interchange among the would-be participants.

Because each of the contributors operated within his or her own schema (some quite elaborate, some quite simple), the papers themselves are very difficult to compare or to "use" as Gist and Dworkin attempt. Given the way the book came about, it is not surprising. It is unfortunate nonetheless.

What is even more unfortunate is that the editors themselves offer no closure to the volume and only five pages of interpretation—before the reader gets to the cases.

As with so many books of this type, the idea is an excellent one, the case material is well chosen and interesting, and it is good to have the work of different specialists represented between two covers. Still, the promises on the dust flap are misleading, and the volume often proves to be less than one is given to expect. The trouble is that editors of symposia seldom move beyond the stages of definition and redefinition, beyond the presentations of their illustrative material, to some sort of synthesis. That is certainly the case with *The Blending of Races*.

The Development of Creole Society in Jamaica, 1770–1820. By Edward Brathwaite. Oxford: Oxford University Press, 1971. Pp. viii+374. \$16.00.

Jay R. Mandle

University of California, Berkeley

Although written as conventional history, and therefore sharing some of the methodological weaknesses of that discipline, this book makes an important contribution to the ongoing debate about the nature of past and contemporary West Indian society. Broadly, this debate is between proponents of a "pluralist" view of West Indian society, in which competing culturally antagonistic groups are seen as being in incessant conflict with each other, and those who argue that a considerable amount of integration or Creolization has occurred, resulting in the emergence of a distinct West Indian culture and society. Brathwaite stands solidly in the latter camp, maintaining that the slaves, free blacks, and planter classes were interdependent and interacted upon each other to create a "Creole society."

Brathwaite argues that two external events, the American Revolution and what he calls the "Humanitarian Revolution," were decisive in influencing the trajectory of change in the emerging Creole society of Jamaica. The first isolated Jamaica from the other English-speaking parts of the New World and thus reinforced the pattern of colonial dependence. The second,

by which he means the movement against slavery which occurred both in Europe and the Caribbean, resulted in changes which provided an impetus to the development of viable new social and cultural institutions.

It is, however, precisely because Brathwaite does address himself to the important pluralist debate that this methodological vagueness looms as a severe handicap. In dealing with an issue such as West Indian pluralism, it would be quite desirable to have the two alternative hypotheses stated, to have decision rules with regard to the nature of evidential support for each clearly laid down, and to have a thorough inventory of the available data made. Only in this way would it be possible to determine which of the two competing analytic approaches is most consistent with the data at hand. Such a systematic method is all the more important in the context of the pluralist debate where elements of both hypotheses ring true, and often the selection of one or the other position seems to reflect merely the author's choice of emphasis rather than a careful weighing of the merits of each position.

This kind of careful hypothesis testing is noticeably absent in this study. Only briefly, and at the very end of the book, are the terms of the pluralist argument even set down. Nowhere in the substance of the work is there a careful assessment of alternative views of Jamaican society, though there is an occasional mention of Orlando Patterson's recent *The Sociology of Slavery* (London: Macgibbon & Kee, 1967), which covers much the same material from a pluralist position. The debate which is suggested throughout is not made explicit, and as a result, after reading Brathwaite one still feels that his study might only reflect an extremely skillful case of special pleading rather than an attempt at a scientific resolution of the questions at hand.

Brathwaite is quite convincing when he argues that it is a mistake to treat late-18th-century Jamaica merely as a declining appendage of Great Britain without a history and life of its own. A substantial portion of the work is concerned with the changes and reforms carried out during these years, most notably in the fields of health and education. Yet other, more basic, changes were not achieved, but Brathwaite fails to account satisfactorily for the relatively limited accomplishments in the process of modernization generally, ranging from the failure of a movement for political independence to emerge to the very limited economic diversification which was achieved.

What underlies these failures, and what, I think, Brathwaite fails to weigh heavily enough, is the fact that Jamaica's was a plantation economy and that the patterns of social relationships in the country were more determined by this fact than by any other. That Jamaica was established as an outpost of the expanding European economy to produce a tropical commodity, sugar, for a growing metropolitan demand implied a particular structural configuration for that society. Specifically, it implied the need for some coercive method of labor force control in order to ensure that the masses of undifferentiated workers, necessary for the plantation cultivation of sugar, were available to the industry. In Jamaica this pattern of labor

force control was, of course, slavery. It was this need to coerce labor in the interest of the dominant industry which at once became the defining characteristic of the society and which set the limits on reform in Jamaica, a set of limitations only barely suggested by the author.

In this book Brathwaite studies Jamaica seriously on its own terms and in so doing provides a healthy corrective to the prevailing view of an unchanging and static society. He does not do so, however, with a high degree of analytic rigor, and as a result neither the nature of the society nor the limits to change in that society are convincingly established. *The Development of Creole Society in Jamaica*, in short, is an important book providing an eloquent argument for one side of an ongoing debate. It is not, however, the study which will put to an end that discussion.

Three Styles in the Study of Kinship. By J. A. Barnes. Berkeley: University of California Press, 1971. Pp. xxiv+318. \$8.75.

Francesca M. Cancian

Stanford University

J. A. Barnes has written an extended analysis of the writings of three anthropologists—Murdock, Levi-Strauss, and Fortes—in the field of kinship. According to the preface, the purpose of this work is “to assist the transformation of social anthropology from an intuitive art to a cumulative science” (p. xv).

The first and longest essay, entitled “Safety in Numbers,” analyzes the cross-cultural method as exemplified by Murdock and his associates. Barnes views functionalist theory as an essential part of Murdock’s method. He interprets cross-cultural research as an attempt to explain similarity across cultures as “parallel adaptive change” (p. 73). Murdock’s method of analysis—namely, counting societies or cultures and applying statistical techniques suitable for use with discrete units—is seen as a special method for testing this theory (p. 98).

Barnes discusses two major issues in cross-cultural research: defining comparable sampling units (Flower’s Problem), and defining independent units or controlling for diffusion (Galton’s Problem). Murdock’s sampling techniques focus on controlling for diffusion. Barnes maintains that this is a mistake, because Murdock’s theory implies that the “strain towards consistency” underlying functional relations will continue to operate even if diffusion is possible (p. 81). The cross-culturalists should abandon the attempt to define independent, discrete units, Barnes argues. He proposes an esoteric alternative strategy in which each independent ethnographic report would constitute a unit, and the functional relations in each unit would be weighted according to the population of the unit and the persistence of the relationship (pp. 78–79). Barnes recognizes that his strategy may be unfeasible in practice; he is only asserting that this method is more appropriate for testing Murdock’s theory (p. 79).

Throughout the discussion, Barnes suggests that the attempt to define a sample of comparable, independent, and discrete social units is doomed to failure. Thus, he opens his discussion of the problem of defining the sampling unit in cross-cultural research with the assertion: "I think the problem is insoluble" (p. 66).

In my opinion, Barnes's interpretation of the cross-cultural method is a step backward for scientific anthropology. Murdock's major methodological problems have very little to do with functionalism; they are faced by anyone who tries to do rigorous comparative research. General propositions about cultures, kinship systems, or patrilineal segmentary societies cannot be tested without defining independent instances of such units, no matter what theory the researcher is using. Barnes is attacking comparative social science, not functionalism, when he suggests that we should abandon the attempt to define a set of comparable, independent, and bounded social or cultural units.

The second essay, entitled "Real Models," analyzes Levi-Strauss's approach to kinship. Barnes begins with an illuminating discussion of basic concepts. Levi-Strauss's structural models are characterized as: mechanical models of rules or ideal types (as opposed to statistical models of observed action), in reversible time (as opposed to irreversible), and based on the analysis of anthropologists (as opposed to the conscious conceptions of the people being studied). Barnes then presents a detailed exposition and criticism of Levi-Strauss's argument on generalized versus restricted exchange of women.

The major problem in Levi-Strauss's work, as Barnes points out, is validation; that is, what is the relation between structural models and concrete reality? Barnes's puzzling conclusion on this critical issue is that "Levi-Strauss provides us with plenty of explanations" (p. 170), but that his theory, like Malinowskian functionalism, is not a scientific theory because it is not testable or refutable (p. 168).

The methods and concepts of Fortes are analyzed in the final essay, "Irreducible Principles." According to Barnes, Fortes's concept of social structure or norms concerns "laws and morals, rights and duties" as they "emerge in observed social action rather than in response to abstract questioning" (p. 195). Fortes's method consists of statistically examining the relative frequency of various combinations of attributes in the population sampled, and then constructing a model of these observations and explaining its operation in terms of the values, sentiments, rights, and duties of the actors (p. 198). In an incisive analysis, Barnes points out the circularity in this approach and criticizes the way in which Fortes explains social structure by unsystematically invoking structural principles, such as "the principle of equilibrium" or "the principle of the equivalence of alternate generations."

The second part of the essay concerns issues in Fortes's analysis of kinship among the Tallensi and the Ashanti, such as the relation between lineage segments and the distinction between descent groups and descent. This discussion may be useful to specialists, who, unlike me, have carefully

read Fortes's monographs and the debates on these issues. It is not useful to the uninitiated.

The major shortcoming of this book is that it is not clear what Barnes means by scientific anthropology. His most explicit statements suggest that what he has in mind is a rigorous comparative social science. For example, he quotes with approval Fortes's statement that anthropology should follow "the model of the experimental natural sciences" (p. 180). Moreover, his comments on Fortes and Levi-Strauss assume the importance of making general, precise, and testable propositions. Thus, he comments that verifying Fortes's statements about patrilineal societies would require demonstrating the presence of the stated relationship "in all, or some societies with patrilineal segmentary social structures" and showing that "the simultaneous occurrence of these features in these societies happens, as [Fortes] says, 'irrespective of period and place'" (p. 183). If this is Barnes's position, then why is he so critical of Murdock's efforts to define a sample of independent societies? And why doesn't he unambiguously reject Levi-Strauss's approach on the grounds that Levi-Strauss does not accept empirical verification as a major criterion of truth?

Another problem with the book concerns its potential audience. It is not suitable for beginning students because it assumes too much knowledge about kinship and about Fortes's monographs. On the other hand, much of the discussion is uninformative to specialists. The most useful part of the book, for me, was the analysis of Levi-Strauss. Barnes's clear presentation of the concepts and methods underlying structural models provides a very valuable framework for understanding Levi-Strauss on kinship and for unravelling the meaning of "structuralism."

Members of Two Worlds: A Development Study of Three Villages in Western Sicily. By Johan Galtung. New York: Columbia University Press, 1971. Pp. iii+302. \$9.50.

Mark S. Granovetter

Harvard University

Johan Galtung's *Members of Two Worlds* suffers as much from its claims as from its defects. In 1960, the author directed an attitude survey in three Sicilian villages, and he attempts to determine, from that survey, why these and other such villages are unable to emerge from their underdeveloped state. Explaining the absence of something is a subtle and complex business in which caution might be advised as to one's claims of success. Modesty escapes Galtung, however, who concludes near the end of the book that "our search has not been in vain; many factors have been found, so many that it may look as if the phenomenon is over-explained" (p. 251).

This puts a heavy burden on a collection of half-hour, 36-question attitude interviews with the villagers. In his attempt to justify the claim, Galtung mines the questions exhaustively. He begins with an intelligent

discussion of the idea of socioeconomic development, leaning heavily on Colin Clark's distinctions among primary, secondary, and tertiary sectors. He then indicates that most theories of development have focused on one of three classes of phenomena: resources, values, or social structure. Galtung argues, reasonably, that a good theory should discuss all three, but then advances a curiously mechanical notion of theory construction—that a “good rule” for making an overall theory would be to combine “one theory from each class,” and that this calls for much more work “with a view to collecting and systematizing already existing single factor theories” (p. 27). Should we really develop new theories by adding up already existing inadequate ones? That seems to be the position taken in this book. Galtung does not attempt to talk about resources, and is thus left with structure and values. The great bulk of the analysis is of values, and the basic approach is to combine the concepts of three other authors in this area: from Pitrim Sorokin comes the “sensate-ideational” distinction; from Daniel Lerner, the idea of “empathy” or “psychic mobility”; and from Edward Banfield, that of “familism.” The impression is clearly given that the choice was relatively arbitrary: these were the three works “that in our minds were outstanding” (p. 28). The subsequent discussions of each concept are well done and of intrinsic interest for those concerned either with those concepts or with their connection to development. Unfortunately Galtung largely drops his critical stance toward each concept once the data analysis begins.

This analysis suffers from three difficulties common in studies based on attitude surveys: (1) ad hoc explanation of findings, (2) magnification of small differences, and (3) reification of abstract concepts.

The data analysis chapters (3–5) are filled with ad hoc explanations of cross-tabulations. The basic method is to find out whether each subject is sensate or ideational, empathic or not, and familistic or not, and then cross-tabulate these findings with other attitudinal and demographic variables. Particularly objectionable among the ad hoc interpretations are the many which make detailed inferences about the phenomenological states of the respondents without a shred of direct evidence. Having constructed a sensate/ideational scale, for example, Galtung finds that ambiguous and “don’t know” answers and low interitem correlations contribute heavily to the resulting “A-shaped” distribution. A more modest researcher might suspect that his scale was at fault, but Galtung explains that “the A-shaped distributions indicate that *there is a low degree of crystallization of attitudes*, that most people have inconsistent, confused patterns of thinking. In other words, . . . the majority are people in transition; they are members of two worlds.” If this were true it would surely be important; but in the absence of justification for such a statement it is difficult to suppress a suspicion that it may not in fact be the respondents who are confused, but rather the measure.

Small percentage differences are persistently magnified and treated as absolute differences between categories. For example, it is argued that those who are “empathic” with sectors of society beyond the village are the “movers”—that is, those who have tried to move out of the village. On

page 200, a table shows us that 35% of those in high-status tertiary occupations are "movers," as compared to 30%, 27%, and 24% of those in primary high, tertiary low, and primary low occupations. Then we are told that the movers "are found particularly in higher echelons of the tertiary sector of economic activity and much less in all other parts of the social structure" (p. 216).

The magnification of small differences also takes the form of treating whole groups as if they were characterized by one trait simply because the trait is represented more heavily in them than in other groups. Those in "primary low" occupations, for instance, are said to be "victims" of "excessive familism," to contrast them with the tertiary high (p. 241). But reference to the relevant table shows that 50% of the primary low are "familists," as compared to 42% of the tertiary high (p. 228)!

Early on, Galtung warns that his three sets of dichotomies are necessarily not "real, empirically found persons. They are abstractions that mingle and blend inside living humans" (p. 71). But this warning against reification of abstract concepts is quickly lost sight of. The sensates and ideationals move from being abstract categories to "camps" within the villages (p. 128). In dividing the sample into high and low primary and tertiary occupations, Galtung fails to point out explicitly (though one can recover the information easily from his details) that the "primary low" category consists entirely of farmhands and the "primary high" entirely of farmers. In subsequent tables, explanations depend on the abstract category name rather than on the specific occupation shared by all within each of these two categories. Not infrequently, Galtung is carried away by his categories. Having established that the primary low are predominantly (i.e., 44%) sensate, the "modern" side of the dichotomy, he concludes that their "excessive" familism (50%) must be what makes it impossible for them to "unleash their modernism at the village level" (p. 241).

Beyond these examples, which could be multiplied, there is the sticky question of how the value orientations, even if we accept their validity, interact with social structure to produce what Galtung calls "an almost diabolic combination of factors that explains almost too well the absence of endogeneous change in the region" (p. 240). The analysis of social structure is the weakest part of the study. We have an early indication that this will be the case when social structure is defined as "a set of social categories . . . and as the pattern of interaction between these categories" (p. 29). Through the book, the main connection of values with social structure is therefore made by indicating in what social categories the "carriers" of "modern values" reside, and drawing inferences from these cross-tabulations. But Galtung ultimately recognizes that such inferences depend on actual interaction between members of the categories, and therefore sketches this interaction structure. But the disparity between the amount of time and energy spent eliciting and analyzing values and categories and that on interaction structure is startling: after more than 100 pages of analysis of the survey, we are informed that interaction structure was determined "according to the consensus of a high number of informants asked about the

subject" (p. 243). We are left in ignorance concerning who these informants are, and what they were asked! This is particularly distressing since the theoretical argument which ties up the book's loose ends is one which points to the lack of interaction between the "carriers" of modern values (the tertiary high and primary low) and the existence of interaction between "the forces favoring status quo"—the tertiary low and primary high (p. 246).

It is, finally, this imbalance between the attitudinal and structural emphases which vitiates this study's claims. Are we really to believe that we can explain "almost too well" the lack of economic development with a study which slights interaction and ignores the possible sources and actual flow of capital and other resources? Can we really deal with this problem without looking carefully at the structure of regional and national markets, at the extent to which villages are embedded in and dependent on them, and at the factors which determine this degree of embeddedness? Can we really do without studying the structure of power at these various levels as well? Values and attitudes are important for development only insofar as they affect individual behavior in these concrete institutional frameworks; the great need in this area is for a study which sheds light on the interplay between such behavior and personal ideological constructs. *Members of Two Worlds*, despite its occasional virtues, does not measure up to this need.

Journey to Ixtlan: The Lessons of Don Juan. By Carlos Casteneda. New York: Simon & Schuster, 1972. Pp. 315. \$6.95.

Michael Kimmel

McGill University

Carlos Casteneda's newest book, *Journey to Ixtlan: The Lessons of Don Juan*, is a remarkable book. The third in his anthropological autobiography of his experiences with a Yacqui Indian sorcerer (*brujo*) named Don Juan offers the critical reader a fascinating account of an individual attempting to deal intellectually with a completely foreign reality, and a stimulating challenge to examine our own assumptions and methods of explanation which form the conceptual framework of our reality.

In the first book of the series, *The Teachings of Don Juan: A Yacqui Way of Knowledge* (Berkeley: University of California Press, 1968), Casteneda begins a descriptive record of an intermittent apprenticeship that was to last over 10 years. By taking voluminous field notes, he renders explicit description of his use of psychotropic plants under the direction of the aging *brujo*. Don Juan maintained that the plants were a means to endow the user with power, and to aid his entrance into another order of reality.

Casteneda dutifully records his experiences without bias. But, as if these hallucinogenically induced "peak-experiences" are not valid per se, he adds a "structural analysis" which attempts to incorporate nonrational events

into a scientific, rational schema. The structural analysis was an abysmal failure. Casteneda found himself unable to subsume all his experiences within his Western scientific consciousness. Afraid, he terminated the apprenticeship.

Casteneda resumed his apprenticeship in 1968. His second volume, *A Separate Reality* (New York: Simon & Schuster, 1971) begins when he returned to Mexico, ostensibly to show Don Juan the first published volume dedicated to the sorcerer. Although *A Separate Reality* is also concerned with description of the psychedelic experiences, Casteneda refocuses his observation on the completely novel "reality" of his teachers. Don Juan and another sorcerer, Don Genaro, demonstrated amazing phenomena (walking across waterfalls, instant bodily transportation, etc.) in an effort to force Casteneda into an irreconcilable paradox: forcing the senses to conceptualize phenomena for which he had no rational basis. Casteneda writes: "My mind refused to intake that sort of stimuli as being 'real'. I began to weep. For the first time in my life I felt the encumbering weight of my reason." Once again, in 1971, he terminated the apprenticeship.

Casteneda's observational focus on the new reality is sharpened in *Journey to Ixtlan*. The use of psychotropics was, he observes, merely one method of altering and/or stopping the ordinary flow of our perceptual interpretations of the world. In the time of this volume he ceases their use. Here Casteneda records the actual lessons taught by the sorcerer toward the annihilation of the preconceived world and the acceptance of the possibility of a multidimensional set of realities. This set, due to our assumptions, can never be fully comprehended.

Finally, Casteneda succeeds in his quest. He was able to hold both his and Don Juan's descriptions of the world simultaneously. Thus he was able to see both unmasked, each as a mere description of the world. His apprenticeship ended.

The accounts of Casteneda's odyssey from the rationalization of experience to the understanding and ultimate rejection of his assumptions governing the codification of his experience are important to sociologists for a number of reasons. The trio have gained enormous popularity among the youth market (the combined sales are almost 20,000 copies per week), for the books allow the disaffected a philosophical grounding for their alienation from American culture. This striking popularity itself merits sociological investigation. Casteneda's books are important for contextual reasons as well. They are methodologically important as an example of participant observational techniques. Casteneda immerses himself in the other reality; only in total immersion is he capable of shedding the burden of his old assumptions and adopting the new. Finally, there is an implicit challenge to the social scientist. The challenge is to our preconceptions of the social and natural world, preconceptions which limit our perception of all phenomena. Rather than attempt scientific explanation ("structural analysis") of our experiences via our preconceptions, we should attempt to understand these experiences without the "burdens of reason": our set of reference assumptions.

Although potentially a profoundly anti-intellectual (i.e., neoreligious) statement, Casteneda's solution is existential. The world exists a priori to our conceptions of it. But philosophy, he has noted, does not include the methodology of existential understanding. It is the well-defined sorcery of the Yacqui *brujo* that emerges as the praxis of phenomenology.

Environmental Control: Priorities, Policies, and the Law. By Frank P. Grad, George W. Rathjens, and Albert J. Rosenthal. New York: Columbia University Press, 1971. Pp. viii+311. \$9.00.

Philip L. Bereano

Cornell University

This is a readable book containing a good deal of information about general environmental policy issues. It is the result of a project undertaken by the Legislative Drafting Research Fund of Columbia University, funded by the Sloan Foundation and the Council on Law-related Studies via the Committee on Science and Law of the Association of the Bar of the City of New York.

The Rathjens essay deals with environmental goals and policies, specifically the setting and enforcing of environmental standards with particular reference to economic criteria. Beginning with the explication of a useful four-part taxonomy of environmental insults (p. 13), the author proposes control strategies differentially tailored to each classification. While this is a sensitive and informed overall treatment of environmental policy issues (such as at what point in an activity controls could be applied with maximum flexibility and effectiveness, who should bear the costs of pollution control, etc.), it does not offer any really new insights or suggestions. In other words, there are already in the literature perhaps a half dozen pieces of this nature. In addition, Rathjens places too much reliance on the economic criteria of *efficiency* and does not give enough attention to distributional questions (*equity*) or to political and social *efficacy* (whether the control strategy is acceptable to the public).

Frank Grad's paper on the intergovernmental aspects of environmental controls reflects his involvement over a number of years with multijurisdictional legal issues in a technological/environmental context; the interstate compact device is somewhat of a specialty of his (he has been involved in the formulation of the Delaware River Compact and the Pennjerdel air pollution proposal for the Philadelphia/Camden/Wilmington area). Although the essay does not contain much that is new or original, it pulls together and integrates previously articulated ideas nicely. It might serve as a good overall summary or a fairly sophisticated introduction to specific pollution control topics. Unfortunately, as Grad himself recognizes (p. 2), much of the underlying legal basis for the essay had been altered even before publication, rendering it significantly less useful as a result. Nonetheless, his last sections on the problems presented by various legal and ad-

ministrative arrangements and on possible governmental structures for environmental management are very worthwhile and not so ephemeral.

Rosenthal's contribution concerns the sources and limitations of federal authority for environmental management. He covers direct regulation (with a variety of possible sanctions), subsidies, grants, charges, loans, research programs, and the use of the government's purchasing power. The essay is a good legal survey, but fairly elementary from the environmental point of view. For example, there is no mention of the problem of pollution emanating from federal facilities, although this is an important dimension of the overall issue, not only in places such as Washington, D.C., but in Chattanooga, Tennessee (site of a large munitions plant), Appalachia (where the Tennessee Valley Authority is heavily engaged in strip-mining operations), and other places. It is also an area in which there is a unique source of federal authority for control operations—a housekeeping power, if you will—which has brought a number of sensitive political questions to the fore in practice, and a number of relevant executive orders into being. The discussion of seizures and forfeitures is a novel possibility, not mentioned in previous literature. In response to comments on earlier versions of his essay, Rosenthal acknowledges (pp. 217, 274–75) that most of the current intellectual ferment in environmental law is not directed toward administrative programs but toward court litigation and citizen participation.

An overall criticism of the book raised above must be emphasized in conclusion. What is needed by the environmental law movement (and others dealing with rapid social change) is the inexpensive, quick, and widespread dissemination of new ideas. The costly duplication of existing ideas and the publishing of information which is stale and incorrect are not services to the public. These three papers should have been photo-offset in June 1970 and quickly merchandised. Instead, months later, at the time of the book's publication, Grad's discussion of air pollution control was already virtually worthless because of the passage of major revisions of the Clean Air Act, major federal legislation regarding solid wastes and noise (1970), and the fascinating renaissance (if not demise) of the 1899 Refuse Act. The press of events in the real world threatens all scholarship in the environmental area, and seems to be a strong argument against the leisurely mummification of ideas between hard covers.

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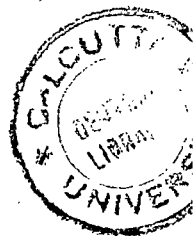
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IN THIS ISSUE

VERN L. BULLOUGH is professor of history at California State University. His most recent book is *The Subordinate Sex: A History of Attitudes toward Women* (University of Illinois Press, 1973).

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MARY F. ROGERS is assistant professor of sociology at Providence College. She is concerned, as the article in this issue indicates, with developing a general theory of power and decision making in social systems. She is also studying female volunteers with a focus on how these activities operate to satisfy work-related needs for women who feel they cannot or should not work.

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Information for Contributors

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- . 1963*b*. "Social Demography." In *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic Books.
- Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32 (February): 5–19.
- Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
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Transvestites in the Middle Ages¹

Vern L. Bullough

California State University, Northridge

Transvestism, which is usually defined in terms of psychopathology, must also be examined in terms of status gain and loss. This appears most obvious in an examination of the lives of the transvestite saints whose legends and myths help set Western attitudes toward transvestism. All of these saints were female, and by implication females could only gain by donning the clothes of the male. Males, on the other hand, lost status if they wore items of female apparel, and the only way society could justify such a loss was through attaching erotic connotations to such conduct which made it both dangerous and sinful.

Transvestism, the desire to dress in the clothes and even assume the role of the opposite sex, has so far been little studied and little understood. The very term was coined only in 1910 (Hirschfeld 1910). Later Havelock Ellis did a series of studies of the phenomenon under the term "Eonism" (1936). Since then there have been several studies (Cauldwell 1956; Savitsch 1958; Stoller 1968), but most explanations have been in terms of psychopathology and have concentrated on male transvestites. Stoller (1968, pp. 183-87) summed up much of the current thinking on the subject. He felt there was as yet no genetic, constitutional, or biochemical evidence to explain the phenomenon. Male transvestites, however, seem to have several etiological factors in common. (1) The mother has an unconscious wish to feminize her little boy, perhaps as an unconscious expression of her own homosexuality. (2) In such cases the father is either a co-conspirator by being silent and passive about the matter or is altogether absent. (3) Transvestites themselves suffer from castration anxiety, for which they compensate by making themselves into phallic women. (4) Transvestism actually is an efficient way of handling very strong feminine identification without having to succumb to the feeling that one's masculinity is being submerged by feminine wishes. In effect it allows a man to channel these feelings through his feminine dress and still be acutely aware of the insignia of his maleness, a penis. Noteworthy of almost all studies of transvestism is the fact that it is defined as psychopathological only in terms of male cross-dressing. There is a curious lack of literature on female cross-dressing, as if to imply either that the phenomenon does not exist or that if it does it is not a problem which can be explained in terms of psychopathology.

¹ Research sponsored by the Erickson Educational Foundation.

Sociologists have more or less adopted this same attitude and when they have looked at the phenomenon have done so in terms of deviant organizations (Sagarin 1969; Buckner 1964). Yet it would seem obvious that most behavior, particularly that defined as deviant, must be influenced by cultural and social structural variables, and the implications of these variables deserve the attention of sociologists. This paper, one of a series examining general attitudes toward sex, under the sponsorship of the Erickson Educational Foundation, argues that Western attitudes toward transvestism (and probably transsexualism as well) have been influenced by status concepts about the role of the sexes. The result has been to differentiate between women who dressed as men and men who dressed as women. Female cross-dressers were tolerated and even encouraged, since they were striving to become more malelike and therefore better persons. Male transvestites, on the other hand, were discouraged not only because they lost status but also because during the formation of Western attitudes most writers could find only one possible explanation for a man's adopting woman's guise, namely, a desire to have easier access to women for sexual purposes. It should be added as a corollary that homosexual cross-dressing, the so-called drag queen, probably only became a significant phenomenon when the status-loss concept associated with transvestism was accepted as part of Western culture. If this role and status explanation has any validity, then it is important that researchers in the field take into consideration the sociological as well as psychological variables in their explanation of transvestism.

One of the richest sources for examining Western attitudes and conceptualizing the phenomenon of transvestism is the lives of the transvestic saints. A survey of their lives sheds light on development of Western attitudes, and more than anything else shows the status concepts associated with transvestism inherent in Western attitudes. In theory, Western society has been hostile to transvestism, although it has always tolerated impersonation at certain periods or events such as Halloween, carnival days, and masquerade parties. The source of this hostility has been traced to biblical statements, particularly to a passage in Deuteronomy (22:5): "The women shall not wear that which pertaineth unto a man, neither shall a man put on a woman's garment; for all that do so are an abomination unto the Lord thy God." Quite clearly female cross-dressers are condemned as much as men, yet Christianity has always been more hostile to men wearing women's clothes. This part can be explained by medieval Christian attitudes toward women. In general medieval society adopted the view of the Greek philosophers that women were inferior to men (Bullough 1973b), although Christianity insisted that women as much as men were a special creation of God. This view is perhaps best summed up by the 13th-century theologian Saint Thomas Aquinas (1947, sec. 1, pt. 1,

p. 92), who wrote: "Good order would have been wanting in the human family if some were not governed by others wiser than themselves. So by such a kind of subjection woman is naturally subject to man, because in men the discretion of reason predominates."

Tied in with and overlying this view of women as creatures who were subordinate to men was a kind of mystic view of the inferiority of females. This attitude, as it entered into Christianity, was exemplified by the Alexandrian Jewish philosopher Philo, who taught that the reason the male was superior to the female was because he represented the more rational parts of the soul, while the female represented the less rational. For Philo progress meant giving up the female gender, the material, passive, corporeal, and sense-perceptive world, and taking the active rational male world of mind and thought. The easiest way for women to approach the male level of rationality was for them to deny their sexuality, to remain virgins; and the words "virgin," "virginity," "ever virginal" occur continually in Philo's references to the best kind of women (Philo 1961, 1963; Baer 1970).

It would seem logical, then, to argue that the female who wore male clothes and adopted the role of the male would be trying to imitate the superior sex, to become more rational, while the man who wore women's clothes, who tried to take on the gender attributes of the female, would be losing status, becoming less rational. This seems to be implied as early as the 4th century by Saint Jerome (1884, bk. 16, col. 567), who wrote that as "long as woman is for birth and children, she is different from man as body is from soul. But when she wishes to serve Christ more than the world, then she will cease to be a woman and will be called man." A similar concept was pressed by Saint Ambrose, also in the 4th century (1887, bk. 15, col. 1938): "She who does not believe is a woman and should be designated by the name of her sex, whereas she who believes progresses to perfect manhood, to the measure of the adulthood of Christ. She then dispenses with the name of her sex, the seductiveness of youth, the garulousness of old age." The list of similar statements could be much expanded to indicate that the Christian church to a certain extent encouraged women to adopt the guise of men and live like men in order to attain the higher level of spirituality normally reserved to males. Whether this was the actual, well-thought-out intention of these church fathers is doubtful, but there are numerous stories about saintly women who lived and worked as men. Scholars today may argue that many if not all of these saints were legendary rather than real (Delehay 1961, p. 189), but folk belief further emphasizes that transvestism among women was usually admired and only rarely punished. There are no male transvestite saints, not only because the male who cross-dressed lost status but because he was also associated with eroticism, or with witchcraft. Two examples

illustrate this. This first is reported in the 6th century by the Frankish writer Gregory of Tours.

Gregory (1927, 2:449) reported that during a revolt of some nuns in the convent of Radegunde the rebellious faction charged the abbess with keeping a man clothed in female garb and pretending that he was a woman in the convent. Everyone knew, they claimed, that he "was most plainly of the male sex; and that this person regularly served the abbess." The charges resulted in an investigation which found that indeed there was a male nun. He had donned female garb because as a little boy, according to the physician Reovalis, he "had a disease of the groin and he was regarded as incurable. His mother went to the holy Radegunde and begged her to have the case examined. The saint summoned me, and bade me give all the help in my power. I then cut out his testicles, an operation which in former days I had seen performed by surgeons at Constantinople, and so restored the boy in good health to his anxious mother. I never heard that the abbess knew aught of the matter." As a result of this testimony the charges against the abbess were finally dropped. The implication remains, however, that the only reason a man might don female garb and live in a convent was to gain sexual satisfaction from the nuns.

Later in the Middle Ages, another incident of male transvestism was reported by an inquisitor in southern France. In this second case, dating from about 1250, a number of men disguised as women entered the house of a rich farmer, dancing and singing: "We take one and give back a hundred." The verse referred to a popular belief in the powers of the good people (in Latin *bonae*) to confer prosperity upon any house in which they had been given presents. The suspicious wife of the farmer did not accept the claim of the female impersonators to be *bonae* and tried to end their revel, but in spite of her protestations they carried out all the goods from her house. Perhaps for this as well as similar reasons bishops were requested to look out for throngs of demons transformed into women (Russell 1972, pp. 157, 292, 315), which seems like a prohibition against male cross-dressing.

No such prohibitions appear against women dressing as men, particularly if they assumed the garb of a monk. Probably the archetype for the female transvestistic saints is Pelagia. Her story is rather confused and contradictory, probably because her life incorporates several conflicting legends. She is known both as Pelagia and as Margarito and is also confused with another saint, Margarita, also known as Pelagius. According to the standard versions of the legend Pelagia was a beautiful dancing girl and prostitute in Antioch who was also called Margarito because of the splendor of her pearls. She became converted to Christianity by the saintly Bishop Nonus, who acted as her patron. Not wishing to be identified with her past, Pelagia left Antioch dressed as a male, and under her outer clothes

she wore, with the permission of Nonus himself, a hair cloth undershirt. After much travel she found refuge on Mount Olivet, where she lived as a man known as Pelagius who was admired throughout the Holy Land for his asceticism and holiness. It was not until after her death that her sex was revealed. When they discovered her to be a woman her mourners are said to have cried out: "Glory be to thee, Lord Jesus, for thou hast many hidden treasures on earth, as well female as male" (Butler 1956, 4:59-61; Waddell 1957, pp. 171-88).

The other Saint Margarita-Pelagius has a slightly different story. In this case Margarita was said to have held marriage in such horror that after her betrothal she fled the nuptial chamber in male dress, cut her hair, and took refuge in a monastery under the name of Pelagius. Such were her qualities of devotion that she was elected prior (a male position) of a convent. She acted the part of a man so well that when the portress of the convent became pregnant and accused Margarita-Pelagius of being the father, the charge was believed. After being expelled from the convent, Margarita-Pelagius found refuge as a hermit in a cave, and it was only at her death, when her true sex was discovered, that she was proclaimed innocent of the crime of which she had been accused (Butler 1956, 4:59-61).

A somewhat similar story is told about Marina, the daughter of a Bithynian called Eugenius, who after being left a widower decided to enter a monastery. No sooner had he entered the monastery than he began to worry about his little daughter Marina, whom he had left in the care of a relative. He went to his abbot with his worries but in the process of telling the story changed his daughter into a son, Marinus. The abbot invited him to bring his young son to the monastery with him, where Marina continued to live as Marinus long after her father's death. One of her jobs in the monastery was to drive a cart down to the harbor to fetch supplies, necessitating an overnight stay in an inn. After one such visit a pregnant girl accused Marinus of seducing her, and Marinus, true to the code of the transvestite saints, suffered ostracism from the monastery rather than admit to her true sex. After her expulsion she and the infant boy lived as beggars at the gate of the monastery pleading to be readmitted. After some five years of this the monks at the monastery pleaded with the abbot to readmit her, and Marinus and "his son" both entered the monastery. The austerities which Marina imposed upon herself led to her death shortly after her readmission, and when the monks came to prepare her body for burial they naturally discovered her true sex. Inevitably the abbot of the monastery was overcome with remorse while the woman who had falsely accused her became possessed by demons. The demons were only driven away when the woman confessed her sin and called upon Saint Marina for intercession in heaven (Butler 1956, 1:313-14).

Though all of these stories are different, there is enough similarity that the German scholar Herman Usener (1879, p. 20) felt that they, as well as others, were simply Christian survivals of the legend of Aphrodite of Cyprus, where women sacrificed to the goddess in men's clothing and men in women's. He emphasized that Aphrodite was known also under the names of Pelagia and Marina, which would seem to give some sort of proof to his arguments. Not all scholars, however, accept such identification, and Father Delehay (1961, pp. 204-6) in particular would argue against it. The purpose of this paper is not to decide one way or another but to emphasize that it was through such popular stories that female transvestism was given sanction in the Christian West. Moreover, all the stories seem to have implications that go beyond mere human interest. Usually, for example, the woman dons male clothing at a time when she is undergoing a crisis in her life, and transvestism seems to denote a breaking with her former existence. Some of the saints go to such extremes as burning their old clothes and even visualizing themselves as males. Saint Perpetua, for example, saw herself in a dream borne into an amphitheater, stripped of her clothes, and changed into a man (Delcourt 1956, pp. 90-99). It also seems fairly obvious that women were encouraged to visualize themselves as attaining the merits of the higher sex.

Athanasia of Antioch represents still another variant of the standard story of the transvestite saints. When her two children both died suddenly and unexpectedly on the same day, Athanasia began spending much of her time praying in a neighborhood church. One day she had a vision in which a stranger assured her that her children were both happy in heaven. When she told her husband, Andronicus, of the vision, the two decided to renounce the things of this world, and leaving everything behind in their house, they set out for Egypt to serve under Saint Daniel, famed for his ability to work miracles. Saint Daniel sent Andronicus to the monastery of Tabenna, while Athanasia became an anchorite and dressed in the habit of a man. After some 12 years of isolation on the desert, Athanasia decided to make a pilgrimage to Jerusalem and during her travels fell in with another monk, actually her husband Andronicus, who was also making the pilgrimage. The two traveled together, joined in religious exercises, visited the holy places, and became so attached to each other that when they reached the place where they had met on their way to Jerusalem they were reluctant to be parted. Together they went to a monastery near Alexandria, and when they were admitted they were assigned cells near each other. Here they joyously filled their monkish duties until Athanasius (her male name) felt death approaching and began to weep. When questioned as to why she was weeping when she was about to go to God, she said she grieved for her fellow monk Andronicus, who will "miss me." The monks were asked to give Andronicus some writing of

hers, which they did, and then Andronicus, her friend and companion, suddenly realized that his friend Athanasius was really his wife (Butler 1956, 4:69–70).

There are still others whose stories have only slight variations: Saint Apollinaris or Dorotheus (Butler 1956, 1:33; all references in this paragraph are to Butler); Saint Eugenia (4:612); Saint Euphrosyne (1:4–5); Saint Theodora (3:623–25); Saint Anastasia Patricia (2:546–47); and the 12th-century Saint Hildegund (2:135). In two other cases the transvestistic experience is only incidental and temporary: Saint Thecla (3:623–25) and Saint Natalia (3:538).

Another variation of the legends of the transvestite saints is that of the bearded female saints, the most famous being Uncumber or Wilgefortis. According to tradition Wilgefortis was one of septuplets (some sources say nontuplets) born to a non-Christian ruler of Portugal and his Christian wife. Wilgefortis early decided to devote herself to Christianity and to remain a virgin, but her father had different ideas. When her betrothal to the king of Sicily was announced Wilgefortis protested to her father but he ordered her to marry the king. In desperation Wilgefortis prayed for help, and her prayer was answered by the sudden growth of a long drooping mustache and a silky curling beard. In spite of this her father pushed on with the marriage plans, but when the king of Sicily managed to see his future bride without her veil he refused to proceed with the marriage. In a fit of rage her father then had her crucified. The story has been described as having the “unenviable distinction of being one of the most obviously false and preposterous of the pseudo-pious romances by which simple Christians have been deceived or regaled” (Butler 1956, 3:151–52). In spite of this, she was much venerated, and as her story traveled across Europe she became known by a variety of names, usually derived from the term *liberata*, the deliverer. Thus in France she became *Livrade*, in Spain *Librada*, *Debarras* at Beauvais, *Ohnkummer* in Germany, *Ontcommer* in Flanders, and *Uncumber* in England. “Cumber,” possibly derived from the German *kummer*, meaning trouble, is now an archaic form, although the word “encumber” and its antonym “disencumber” are still used. At any rate, in England Uncumber became the patron saint of married women who wanted to rid themselves of their husbands. To encourage the saint to help them they brought offerings of oats, although the reasons for this particular gift are not clear (Edward 1969).

Uncumber is not alone in the bearded-saint category. There are at least two others. A Saint Galla, according to the legend, had been left a widow after only a year of marriage, and though she was young and healthy she refused to remarry because she felt that though matrimony “always begins with joy,” it “ends with sorrow.” Her physicians warned her that if she did not marry again she would grow a beard, but she

refused and, bearded, she joined a band of religious women who lived close to the Basilica of Saint Peter and spent her life taking care of the poor and needy (Butler 1956, 4:36–37). The third bearded saint is Paula, a virgin of Avila who, fleeing from a suitor she did not want, in desperation threw herself at the foot of a crucifix and implored Jesus to disfigure her. Her prayers were answered so rapidly that her suitors passed by without noticing her, disguised as she was with a full beard (*Acta Sanctorum* 1643, February, vol. 3, col. 174).²

The most famous transvestite in the medieval period, and the one who perhaps has caused the greatest anguish to Catholic historians, is the legendary Pope Joan, who supposedly ruled under the name of John Anglicus (Dollinger 1890; Cooke 1610; Wood 1931; *New Catholic Encyclopedia* 1967). Several 13th-century chroniclers wrote about her life in great detail, and during much of the later medieval period her existence was accepted as fact. A statue of her was included among the popes in the Cathedral of Sienna in the 14th century, and in the 15th century John Hus the Bohemian heretic reproached the delegates at the Council of Constance (1415) for allowing a woman to be pope. It was not until the 16th century, when her existence was seriously disputed, that she became relegated to legend rather than history. She still has an occasional supporter, although few in the scholarly world now accept her existence.

The legend is fairly complicated, and today it seems difficult to believe that people were convinced of its authenticity, but believe they did. Though there are various forms of the legend, Joan is usually said to have been born in England (hence the title "Anglicus" which is usually included in the legend). As a child she was taken by her father, a learned man, to Mainz, where she was taught to read and write. There she fell in love with a monk by the name of Ulfilias at the monastery of Fulda, and in order to be closer to him she disguised herself as a man and entered the monastery. Later she and Ulfilias traveled together as pilgrims, after a series of adventures making their way to Athens. The two, with Joan still in male dress, studied philosophy, theology, and holy and humane letters for some 10 years and acquired great reputations as scholars. Tragedy struck with the death of Ulfilias, and Joan, anxious and heartbroken, decided to return to Mainz. When she stopped in Rome on the way to Mainz, still in her male role, she found her reputation as a scholar had preceded her; and at the urging of some of her former pupils she began to lecture in Rome. As her reputation spread, she rose rapidly in the church hierarchy, becoming first a notary, then a cardinal, and, on Pope Leo's death in the 850s, pope under the name of John VIII, Anglicus. Unfortunately she still had

² *Acta Sanctorum* is the standard collection of saints' lives. I cite it in this study only where English versions are not available. Saints are listed in the month of their feast day.

a woman's sex drives, and grieving for her beloved Ulfilas, she fell in love with a Benedictine monk from Spain who was said to look like her dead lover. She became pregnant, although this phenomenon remained unnoticed until in the midst of a papal procession she entered labor and gave birth to a child. Both she and the child died shortly after, although there are conflicting versions. One version, in fact, has her child becoming pope later under the name of Adrian III. A vast amount of scholarly research has been expended on searching out the history of the period, and this paper is not the place to examine it in detail. It is important to emphasize that nowhere is there any hint of censure for her donning of male clothes, and her downfall came about not because of her transvestism but because of her "womanly weakness." Incidentally, the legend has also been used to justify an equally fallacious belief that since her time all popes have had to prove they were males by sitting in a special chair.

None of the women so far discussed basically assaulted what men regarded as their prerogatives. Pope Joan perhaps came the closest, but in the final analysis she proved no match for the "superior" male. When a woman attempted to meet men on their own terms, however, she was in trouble. This appears most obviously in the account of Joan of Arc, and her transvestism was one of the major reasons for her execution. In the original complaints against Joan it was charged that she had

a male costume made for her, with arms to match. . . . When these garments and these arms were made, fitted and completed, the said Jeanne put off and entirely abandoned woman's clothes; with her hair cropped short and round like a young fop's, she wore shirt, breeches, doublet, with hose joined together and fastened to the said doublet by 20 points, long leggins laced on the outside, a short mantle reaching to the knees, or thereabouts, a close cut cap, tight fitting boots and buskins, long spurs, sword, dagger, breastplate, lance, and other arms in the style of a man-at-arms, with which she performed actions of war and affirmed that she was fulfilling the commands of God as they had been revealed to her. [Barrett 1931, p. 152]

Later the various charges against her were summarized in 12 articles, of which two dealt with transvestism, as did two of the six admonitions directed against her. Joan eventually recanted, and as part of her recantation she promised to don female clothing. It was her resumption of male dress that led to her execution. When asked why she had resumed it, Joan answered that "she had taken it of her own will, under no compulsion, as she preferred man's to woman's dress. She was told that she had promised and sworn not to wear man's dress again and answered that she never meant to take such an oath. Asked for what reason she had assumed male costume, she answered that it was more lawful and convenient for her to wear it, since she was among men, than to wear woman's dress" (Barrett 1931, p. 158).

Joan was undoubtedly different from the other transvestite saints in that she threatened men on their own grounds and even wore battle dress. Unlike the other women, however, she was always recognized as a woman and never made an effort to be other than a woman in male garb. Superficially, at least, it was simple cross-dressing which brought about her downfall. This was emphasized in the postmortem trial which led to her rehabilitation, where the testimony indicated she had been forced to assume the male dress and in so doing had signed her own death warrant. It was claimed that, when she had arisen on the morning in question, she found that her jailers had removed all of her woman's clothes, leaving only male garments behind. Knowing that the male costume was forbidden to her, she asked for her woman's clothes, but the guards refused to return them. Rather than put on the male clothes she had returned to bed until noon, when physical necessity finally forced her to put on the forbidden male garments to answer the call of nature. Though politically the English probably always meant to execute her, it is nonetheless a fascinating comment upon the attitudes of the time that they used the resumption of male dress, something which they undoubtedly had engineered, as their basis (Scott 1956, p. 14). Quite obviously, for a woman to assume a male guise to become more holy was permitted, but to compete with men on masculine grounds such as warfare was simply not permitted. Such competition represented not a gain in the status of woman but a loss of status for men, since a mere woman could succeed at what they regarded as strictly male tasks.

Compared to female transvestism, male transvestism was nearly non-existent; but it was tolerated under two conditions: when there was only an illusion of the female and when the male in drag was performing a function that society wanted and desired but would not, because of other prohibitions, allow women to do. In effect the status loss associated with male cross-dressing was only allowed when other more dearly held values of society otherwise would have been threatened. The most obvious example of this was on the stage as drama began to revive in the later Middle Ages. In Greek times most of the women's roles had been acted by men, but the Romans, who were much more open-minded on the subject, allowed women to portray themselves. Actresses, however, received a bad name, as did the theater in general, and the early church fathers strongly condemned it. Though such condemnation did not entirely eliminate drama in such areas as Constantinople, and plays continued to be written in western Europe by such people as the nun Hrotswitha in the 10th century, professional acting more or less disappeared, although the amateur still remained (Tunison 1969).

The key forces in the development of modern drama were the medieval mystery play, emphasizing the passion of Jesus, and the morality play,

emphasizing the saints' lives. Since these plays were performed in the church, most of the actors were drawn from the ranks of priests or would-be priests. This tradition continued well after the medieval period, and occasionally we even get glimpses of some of the actors. This is the case with a young barber's apprentice at Metz who is said to have performed the role of Saint Barbara so "thoughtfully and reverently that several persons wept for pity; for he showed such fluency of elocution and such polite manners, and his countenance and gestures were so expressive when among his maidens, that there was not a nobleman or priest or layman who did not wish to receive this youth into his house to feed and educate him; among whom there was a rich widow . . . who wanted to adopt him as her heir" (Mantzius 1937, p. 89). The youth's reputation as a female impersonator was short-lived because the next year when he acted the part of Saint Catherine his voice changed and the audience was not so impressed. The young man then abandoned his acting career and went off to Paris to study for the priesthood.

Females were not entirely excluded from the mystery or morality plays, and there are occasional references to them. In general, nonetheless, it was not considered proper for a woman to exhibit herself, and in most of the plays men kept a monopoly. In the beginning, when the priests and acolytes had been the only actors, it was out of the question that women share in the performance. As more secular plays developed, such as farces, the actors for the most part were vagabonds and were regarded as outcasts. Few women appeared in these companies either. This meant that men almost exclusively played the woman's role, and they were not condemned for this, since they were already regarded as low-status persons, outside the recognized caste system. The organized minstrel companies were also usually all male (Jusserand, n.d., pp. 177-218). It was not until the 16th century in Italy that women began to appear as characters in plays portraying themselves. From surviving references it is almost possible to trace the replacement of men by women in the women's parts. For example, in Lyons the first professional actresses appear in 1548 (Gilder 1960, pp. 46 ff.). In general the movement to use women spread from Italy to France, and thence to Spain and eastward on the continent and eventually to England. It was not until the 17th century that the English fully accepted women playing women's parts. Even then, however, many of the female parts were still enacted by men. In brief, the prohibitions against transvestism, which seem to have been more socially enforced for males than for females, apparently did not apply to the stage because society put a higher value on women keeping their place in society than they did on realism on the stage. Moreover, actors as a class were considered outcasts from the social structure and therefore did not lose status

by donning women's clothes for the performance. Transvestism in essence was justified to emphasize a higher virtue in society.

Male transvestism was also permitted under certain carefully controlled conditions during festivals or carnivals in which the usual standards of behavior were laid aside. Although few of these medieval carnivals have been studied in detail, we know that in some of them women were allowed to act the male role, and men, to wear women's clothes. In the Nuremberg festivals of the late medieval period a number of male dancers wore feminine masks and probably dressed as women. In fact, it seems to be such a common phenomenon that one authority has said that disguising as the opposite sex was a custom that was "peculiar to all carnivals." The Mummers' parade in Philadelphia or the Mardi Gras in New Orleans are continuations of this old custom. A standard feature of the Nuremberg carnival was the Wild Woman, probably a man impersonating a woman, who appears in several illustrations of the 15th century (Sumberg 1966). Such parades not only removed some of the status inhibitions of society, since there were boy bishops, and kings of the varlets, but also dropped some of the sex barriers as well and undoubtedly in this respect served the same function as the Christmas office party or the Halloween and New Year's balls of today.

In theory transvestism may well have been prohibited by the medieval church, but in practice the church did not seem to be too concerned about cross-dressing, providing it took certain socially desirable forms and was rigidly circumscribed. Under such conditions, in fact, it was institutionalized. Only when it went beyond the tolerated levels and threatened the status quo, as happened with Joan of Arc, or when it took on too much erotic appeal, as in the case of a man disguising himself as a woman in order to live in a convent, was there a reaction to it. The most remarkable aspect of medieval transvestism, however, is the difference in treatment of the male and female transvestite. A female who secretly wore men's clothes was not considered abnormal. That a female might desire to be a male, in fact, seemed to be a healthy desire, a normal longing not unlike the desire of a peasant to become a noble. This did not mean that either women or peasants were allowed to cross the status lines in great numbers but that the desire to do so was accepted as a norm. Though men might dress as women at carnivals, and the lord might mix with the peasants at various festivals, the status loss in any real change along these lines was so threatening that anything more than play acting was forbidden. On the other hand, those women who were successful in changing identities were not stigmatized but instead accepted, and not a few were sainted. Throughout the literature of the time, even that written by women, such as the 12th-century Anna Comnena (1928), those characteristics in women most sought after were said to be the masculine qualities. These were the higher

qualities, while the feminine qualities were the lower ones (Bullough 1973a); and inevitably men who dressed as women had to be stigmatized and their actions linked with some type of deviant eroticism, or in more modern times with psychopathology. Such men were in our terms mentally ill, while women who cross-dressed were not regarded as suitable for study or at least have not been studied.

If this analysis is correct, then the root cause of Western hostility to transvestism is not so much biblical prohibitions but status loss. This explains why there are so few female transvestites in psychological literature, since society in fact encourages women to assume male roles as a sign of their superiority to other women, and only rarely regards such women as abnormal. Only when women threatened the male establishment by taking too overt a masculine role have they been ostracized in the past. By implication, then, if an undercurrent of status loss in assuming a woman's role still prevails in Western society, and studies of women's occupations such as nursing indicate that it does (Bullough and Bullough 1969), then we must also take into account a desire for status loss, either permanent or temporary, as a possible explanation for male transvestism today, regardless of whether it is homosexual or heterosexual.

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Family Structure and Socialization Patterns in Taiwan¹

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A social structural analysis is applied to examine the effects of three-generation family living on child socialization. The most relevant, and generalizable, feature of life in a three-generation family is conceptualized as the mother's subordination to her mother-in-law. Data from an interview study of Taiwanese mothers support the idea that persons who work under the direction of someone else will value behavioral conformity in their children and use disciplinary techniques which rely on external control rather than on appeals to the children's feelings.

Sociologists have been concerned for many years with group differences in parent-child relations, but it is only recently that serious efforts have been made to specify the mechanisms which might link social structure to patterns of socialization. Melvin Kohn's work relating socialization values to the occupational characteristics of middle- and working-class jobs is the most notable example of this trend (Kohn 1963, 1969; Pearlin and Kohn 1966). This paper is an attempt to extend Kohn's theory to another feature of social structure, family composition. The specific contrast to be drawn is between two- and three-generation households.

In briefest outline, Kohn argues that the characteristics of the occupation in which a man invests so much of his time and energy are bound to affect his view of what is important in life for himself and for his children. The man whose job is routine, repetitive, closely supervised—in short, the typical member of the working class—is likely to value conformity to external authority and to think that obedience is what children should learn. The typical middle-class occupation, in contrast, requires initiative, independent judgment, and the ability to deal with people and/or abstract ideas. The person in this type of job is more likely to value autonomy rather than obedience to those in authority, and to feel that his children

¹ This article is based on my Ph.D. thesis, "The Effect of Household Composition on the Child Rearing Practices of Taiwanese Families," Department of Human Development and Family Studies, Cornell University, 1971, and is a revised version of a paper read at the 1970 meetings of the National Council on Family Relations. The research was supported by a predoctoral fellowship from the National Institutes of Health (MH-33, 218) and by grants from the London-Cornell Project and the Cornell China Program. I am indebted to Mevin Kohn, John Meyer, Francesca Cancian, Stephen Olsen, and two anonymous readers for helping to improve the paper through their critical comments on earlier drafts.

should learn to control their own behavior. The specific child-training practices of the middle and working classes are seen as consequences of these different value orientations. For example, in reacting to a child's misbehavior, the middle-class parent will be more likely to evaluate the child's intent and the long-term consequences for their relationship, while the working-class parent will be more concerned about the immediate effects of the child's behavior, and getting him to stop whatever it is he is doing.

Kohn's evidence does indeed support his theory, not only for men, but also for their wives, although the mechanism by which a man's occupation affects his wife's values is not known. The argument developed here is that family structure may act as another important determinant of a mother's socialization attitudes and practices.² If the occupational setting in which a man spends his day can exert such a strong influence on his values, the domestic setting in which a woman's day is spent ought to have much the same effect.

In a large number of the world's societies, the ideal form of domestic arrangement is the patrilineally extended family, and in many of these, young women are subject to the authority and direction of their mothers-in-law. While the authority patterns of home and factory differ in many important ways, there is a basic similarity between the situation of a mother in this type of three-generation family and that of the typical working-class man as described by Kohn. In both cases, rewards are contingent upon following the directions of one's superior, rather than upon organizing and carrying out work on one's own. Thus, mothers in three-generation households should resemble working-class men in the high value they place upon conformity and obedience in children, while mothers in nuclear families should place more emphasis on autonomy and self-reliance. It may also be hypothesized that, consistent with the distinction between external behavior and inner feelings, three-generation mothers will use more direct (especially physical) punishment, while mothers in two-generation families will be more inclined to use disciplinary methods that appeal to the child's sense of guilt or shame.³

This analysis is not meant to obscure the fact that there exist any number of important differences between social relationships in families and on the job. The degree of emotional involvement, for example, is very

² Although the term "family structure" is a general one referring to any variation in the number, sex, and relationship of family members (and sometimes to the patterning of family roles), its use in this paper will be limited to the contrast between two- and three-generation households. "Family composition" and "household composition" will be used interchangeably with "family structure."

³ Kohn has examined the effects of specific occupational characteristics only for socialization values, but evidence for the link between close supervision and punitive child-rearing techniques has been presented by McKinley (1964).

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different in the two settings. Nonetheless, there is much to be gained in attempting to abstract from varying concrete situations general dimensions and patterns of human interaction. My argument is that dominance-submission is among these general dimensions, and that dominance might be expected to have similar consequences in both situations.

Previous research reveals a certain amount of support for the predicted effects of three-generation living on socialization, at least in the area of conformity versus autonomy. For example, two cross-cultural studies by Whiting (1959) and Whiting et al. (1966) indicated that self-reliance training is more pronounced in nuclear families, a finding which has been replicated intraculturally in Taiwan (Olsen 1966, 1973).⁴ From a slightly different point of view, Cancian's study of interaction in Mexican families showed three-generation mothers demanding greater conformity from their children in the sense that they more frequently issued orders and attempted to dominate them (Cancian 1965).

There is no previous evidence on the type of punishment used by mothers in two- and three-generation families. Both Cancian's research (1965) and that of the Six Cultures Project (Minturn and Lambert 1964) showed nuclear family mothers as more punitive, but in both cases, nurturance and punishment were combined into a single "warmth-hostility" dimension.⁵ The results of these studies are thus difficult to apply to the hypothesis that *type* of punishment used will vary according to family structure. However, Cancian's research indicated that within three-generation families, women with particularly dominant mothers-in-law behaved more punitively toward their children, a finding which may at least be related to the hypothesis that direct punishment will be used by women who are closely supervised by their mothers-in-law.

The results of these studies emphasize a theoretical point that has been implicit in the discussion so far—the idea that three-generation living is not a unitary variable from which similar effects are always to be expected. On the contrary, like social class, family composition is an index to a set of relevant experiences, and it is these experiences that form the independent variable. For my study, I have predicted that women living in three-generation families will be more inclined to emphasize conformity and obedience in their children, and to use direct forms of punishment in

⁴ One problem in relying upon the cross-cultural studies is that a child-rearing emphasis on self-reliance has also been demonstrated for hunting as opposed to agrarian societies (Barry, Child, and Bacon 1959), and nuclear families are more common in hunting societies (Nimkoff and Middleton 1960). From the reported cross-cultural materials alone, it is impossible to know whether the association of household composition and self-reliance training is spurious or not. However, intracultural replication lends more credence to the cross-cultural findings.

⁵ An extended discussion of the difficulties of interpreting the Six Cultures "warmth" dimension may be found in N. Olsen (1971).

dealing with misbehavior. These predictions emerge from my assessment of the effects of the mother's subordinate position relative to her mother-in-law. If the mother is not dominated, there is no reason to expect that her socialization attitudes and practices will be affected.

This emphasis on specific aspects of experience as the independent variables from which child-rearing consequences follow is the keystone of the "social structural" approach to the study of socialization patterns. It is in contrast to the "cultural" position, which argues that groups within the larger society maintain distinct subcultural methods of socialization that are passed on from generation to generation. While I have taken a social structural approach in this study, it is also possible to construct a cultural argument that would predict the same outcomes, one based upon traditionalism in extended families. Since the mother in this type of family shares her child-rearing activities with a member of the conservative older generation, it is to be expected that she would be less inclined to adopt "modern" socialization methods than would the more isolated nuclear family mother, working out child-care techniques on her own. Therefore, support for the structural position requires more than a confirmation of the predicted differences between two- and three-generation families. It is necessary also to rule out traditionalism as a possible alternative explanation.

Finally, it should be noted that the simple increase in numbers in the extended family might have effects on child rearing similar to those outlined above. Although expanded family size is, like presence of grandparents, a structural feature of three-generation living, the specific mechanism linking structure and socialization is quite different in the two cases. My argument is that it is the introduction of an authority figure, not simply another adult, that influences maternal socialization. Again, support for this position requires that the effects of family size be eliminated.

SETTING FOR THE RESEARCH

The island of Taiwan (Formosa) lies 100 miles off the coast of southeastern China. Originally inhabited by a relatively primitive Malayo-Polynesian people, it was settled during the 17th century by immigrants from Fukien and Kwangtung provinces, who brought with them the languages, the technology (labor-intensive wet-rice agriculture), and the family customs characteristic of that region of traditional China. These settlers founded the families now known as native Taiwanese.

In 1949 another wave of immigration occurred after the Communist victory on the mainland of China. These "Mainlanders" come from many different provinces, but most speak one or another of the northern dialects, and their customs are slightly different from those of the native Taiwanese.

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Moreover, the family disruption caused by the retreat from the mainland, as well as the fact that most Mainlanders have taken up residence in the largest cities, has left these newcomers open to the changes in family patterns we associate with "modernization," while the Taiwanese remain more traditionally Chinese in family matters. Because many of the immigrants were unmarried soldiers, there has been a fairly high rate of intermarriage between these two groups. Nonetheless, for most purposes, it is fair to regard Taiwanese and Mainlanders as distinct social categories. For this and other reasons, the present study has included only native Hokkien-speaking Taiwanese.

The population of Taiwan now numbers approximately 14 million, of whom 11 million are native Taiwanese. The island is undergoing rapid industrialization, with over half of the work force engaged in nonagricultural occupations. Correspondingly, family patterns are gradually changing, particularly among the educated urban elite. Nevertheless, many traditional values and behaviors remain.

Traditional China has often been used as a textbook example of a family system organized around the importance of the male line.⁶ Patrilineal and patrilocal, the ideal domestic unit consisted of a large joint family, with all male descendents of the patriarch and their wives and children eating and working together. Although this ideal was frequently violated in practice, particularly among the poorer peasants, most persons lived at least some portion of their lives in a patrilineally extended family. The fact that all sons were entitled to inherit an equal share of family properties encouraged the division of the joint family into smaller nuclear units upon the death of the oldest male, and sometimes before. The process of family division was a regular and anticipated one, and while accompanied by hard feelings in individual cases, was not considered improper from a cultural point of view.

Consistent with the degree of importance placed upon patrilineal succession, the status of women was low, a fact which such practices as female infanticide, footbinding, and concubinage attest to. The birth of a daughter was often a less than happy occasion, for girls were "goods on which money is lost," incurring expense in their rearing only to leave to produce sons for another family. Arthur Smith (1970) reports that young mothers were sometimes beaten upon the birth of a daughter rather than a son.

The position of the new wife, a total stranger to her husband's family, was particularly unenviable. According to Smith, "She is not received with enthusiasm, much less with affection . . . , but at best with mild toleration, and not infrequently with aggressive criticism" (Smith 1970, p. 248).

⁶ There are any number of standard ethnographies and analyses the interested reader may consult. Particularly recommended are Smith (1970), Freedman (1958), Fei (1939), and Lang (1946).

Her duty in her new family was to supply domestic labor, and of course, to give birth to male descendants. As the mother of a son, her status improved somewhat; nonetheless, "in the management of her children, as in other affairs, she is wholly subject to the dictation of her mother-in-law" (Smith 1970, p. 210). The control that mothers-in-law exerted over younger women in the family was in some cases despotic. "A great deal is heard of the tyranny and cruelty of these mothers-in-law, and there is a firm basis of fact for all that is so often said upon the point. . . . Abuse of the daughter-in-law is so common a circumstance, that unless it is especially flagrant, it attracts very little attention" (Smith 1970, p. 211). As there was usually no recourse in such a desperate situation, since husbands were morally required to support their mothers in any quarrel between the two women, suicide among young married women was not infrequent.

This description of Chinese family life in pre-Republican China continues to hold in large measure for present-day rural Taiwan.⁷ The existence of extended households is at least as common as in traditional times, and the process of family division occurs in much the same fashion. Marriages are still arranged, although now it is usual for the young couple to meet at least once before their marriage and formally approve the match that has been made for them.

The position of women, and particularly daughters-in-law, has improved considerably. Footbinding and infanticide are no longer practiced, and there exist humane societies for the protection of adopted daughters and daughters-in-law. The abused daughter-in-law now has recourse to legal remedies, and most significantly, she has options other than to remain in the extended family or leave it by death at her own hand. Industrialization has provided the opportunity for factory employment and an independent livelihood for young women.

Altogether, then, mothers-in-law no longer possess the absolute power they once did, but the subordination of the rural daughter-in-law remains real. Her emotional isolation in her new family forces her to develop good working relationships with the female members of the household, and particularly with her mother-in-law who directs her domestic activity and whom custom still demands that she please. The role of women in the Taiwanese rural family is analyzed in depth by Wolf (1972), for readers who might wish to pursue this subject further.

We have much less information about urban family life in Taiwan. From government population statistics (Taiwan Provincial Government 1968), it is clear that the proportion of married women living in extended fam-

⁷ See ethnographies by Wolf (1970, 1972), Diamond (1969), and Gallin (1966). Wolf is especially valuable for her sensitive analyses of the interpersonal dynamics surrounding family roles in rural households.

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ilies at any point in time is somewhat lower in the cities (35% vs. 46%), but the urban figures are not presented separately for Mainlanders and Taiwanese, and there is reason to believe that extended family living is more common among Taiwanese. For example, the Taichung Fertility Study revealed that whereas 96% of Taiwanese couples under the age of 40 had parents or brothers living in Taiwan (and thus the possibility of forming an extended family), only 25% of comparable Mainland couples had this option (Chu 1969; Freedman and Takeshita 1969). This same survey has also demonstrated that virtually all newlyweds live with the husband's parents, if they are available. Rural-urban differentials in the proportion of extended families, then, result either from the lack of availability of appropriate relatives (as among Mainlanders) or from an earlier division of the family (as among wealthier, more highly educated Taiwanese).

Attitudes and values concerning family life are definitely less traditional among urban Taiwanese (N. Olsen 1971). Urbanites are less likely to say, for example, that having a male descendant is extremely important, or that parents should arrange a marriage without the participation of the young people in the decision-making process. In fact, the typical mode of mate selection in urban areas appears to be the method of "choosing together," whereby the parents of a young man and woman take the initiative in arranging a meeting between their children, after which the couple enters into a dating relationship that may or may not eventuate in marriage.

The custom of "choosing together" is particularly interesting for its blend of traditional and modern elements. Parents retain control over deciding on the initial suitability of a potential spouse, but the young people have genuine veto power. And since the young couple have had some opportunity to get to know each other on their unchaperoned dates, the ultimate decision to marry is presumably based on a belief in their compatibility and on a growing affection for one another.

This intrusion of romantic love into the process of mate selection undercuts an important structural basis for the power of the mother-in-law over the new bride. The younger woman may now expect some degree of emotional support from her husband, despite the fact that both of them, if well reared, will believe they ought to respect and obey the older woman.

It should be emphasized that these comments about shifts in the nature of the mother-in-law-daughter-in-law relationship are based only upon impressionistic evidence, since the interpersonal relationships of adults in urban families have received no systematic study. It seems likely, however, that these patterns would vary by social class. Just as nuclear families are somewhat more common at higher social class levels (Chu 1969) and as women in higher-status families are more likely to express nontraditional attitudes toward family customs (N. Olsen 1971), it seems possible that

the relationship of mother-in-law and daughter-in-law might be more egalitarian in such families. On the other hand, control over economic resources has been shown in any number of studies to lead to greater family power (for example, Blood and Wolfe 1960), and thus it would be equally plausible to predict that old parents in wealthy families would demonstrate more influence over the younger generation than would be the case in poorer families. This is obviously a question on which empirical evidence is needed.

In summary, then, there are many data to indicate the persistence in rural Taiwan of family patterns common throughout premodern China. For urbanites, the available information on family life is sketchy, but suggests both change and continuity. It is certainly the case that young Taiwanese couples typically begin their married life in the home of the husband's parents, and that mothers-in-law retain formal authority over the domestic affairs of their families. The extent to which their actual power has declined, and how this might vary by social class, remains unknown, but there is much to suggest that the domineering mother-in-law is by no means an uncommon figure in urban, as well as rural, Taiwan.

SAMPLING DESIGN

As the foregoing has made clear, Taiwan is an ideal locale for studying the effect of variations in family structure on socialization, because it is a society in which both nuclear and patrilineally extended families are common, and in which women are subject to the authority of their mothers-in-law.

However, the comparison of two- and three-generation families in Taiwan poses several potential methodological hazards. A couple commonly begins married life in the home of the young man's parents, and it is the division of a large family or the death of the old parents which typically produces a nuclear household. As a result, on the average, parents in nuclear families are older, they have more children, and the children are older. If children of a given age were to be selected for study, those from nuclear families would more likely be later-borns. Moreover, the problems of providing housing and furnishings for each of its constituent small families predisposes poorer extended households to stay together longer than might their more affluent neighbors. Comparing the two types of families, then, requires controls for these possibly confounding variables. This goal was achieved through the use of a group matching procedure, employing a $4 \times 4 \times 4 \times 2$ sampling design, which is described in detail below.

The research was conducted in the capital city of Taipei, and in the village of Lu-shang, located on the island's central agricultural plain. Large numbers of sixth-grade school children were surveyed in an attempt to

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find 128 native Taiwanese families with the appropriate economic and demographic characteristics.

Family Structure

Four household categories were delineated, two- and three-generation families each being divided into situations conducive to high and low degrees of mother-in-law domination. For nuclear families, this was frequency of contact with the senior generation, with close contact defined as getting together more often than once a week. Three-generation families were divided according to whether both members of the senior generation were present, or just the husband's mother. A mother-in-law has considerably more power as the wife of the head of the household than she does when the headship has passed to her son (Wolf 1970). It is important to note that, in this study, the potential for mother-in-law domination is inferred from these differences in family structure, rather than measured directly.

An attempt was made to include only families whose grandparental situation had not changed during the lifetime of the sixth-grade sample child. This was to insure that the experiences shaping a mother's socialization practices would have remained constant over a fairly long period of time.⁸

Control Variables

Nuclear families in Taiwan tend to be more advanced in the family life cycle, and a child of any given age is less likely to be a firstborn if he lives in a nuclear family. To control for this, the sample was limited to "medium-sized" families of from four to six children, and was selected on the basis of the ordinal position of the sixth-grade sample child. Each of the four household categories contained comparable numbers of oldest, middle, and youngest children of this age. Middle children were divided according to whether or not they had a same-sex older sibling, thus producing four ordinal position categories.

The sampling design also included a control for economic position. Within each of the four household categories, there were equal numbers of families from three social classes in Taipei, and from the rural village. The social class categories were based on the prestige of the father's occupation—professionals, higher governmental officials, and owners of larger businesses were defined as the "upper-middle" class; minor govern-

⁸ Also, in order to rule out any possible confounding influence that contact with the wife's parents might have, families were chosen for the sample only when the degree of such contact was minimal.

ment officials, shopkeepers, and primary school teachers make up the "lower-middle" class; peddlers and laborers compose the "working" class.⁹ All of the fathers in Lu-shang Village were farmers. None of the mothers in the sample were employed outside the home, although some assisted in family-based enterprises. Finally, the sampling design called for equal numbers of boys and girls.

The attempt to find families that met these relatively rigid requirements was fairly successful, as can be seen in table 1. Although it was possible

TABLE 1
SUCCESS OF SAMPLING PLAN

	THREE-GENERATION		TWO-GENERATION	
	Both Grand- parents (<i>N</i> = 25)	Grandmother Only (<i>N</i> = 30)	Close Contact (<i>N</i> = 22)	Little Contact (<i>N</i> = 30)
Upper-middle class	6	8	7	8
Lower-middle class	6	8	8	8
Working class	6	8	4	8
Rural	7	6	3	6
Boys	15	15	13	15
Girls	10	15	9	15
Oldest child	8	8	6	6
Middle, but oldest of sex	7	7	6	8
Middle, same-sex older sibling	7	8	6	8
Youngest	3	7	4	8
Mean number of children	4.7	4.9	4.8	4.8
Mean number of persons in household	11.0	9.1	7.0	7.2
% families with relatives other than grandparents	60	30	5	13
% joint families (at least one married father's brother)	24	13	0	3
% families with changes in grandparental situation since birth of sample child	8	23	54	37
% families with such changes since sample child was 5 years old	8	0	36	7
Mean level mother's education (1 = none, 5 = college)	2.0	2.0	2.3	2.7
Mean level father's education	2.9	2.9	3.0	3.2
% urban families living in Taipei since before birth of sample child	100	100	94	88
% families following traditional religion ..	100	97	95	90
% urban families with servant	22	37	31	33

to locate only 107 of the proposed 128 families, the missing cells are distributed in such a way as to introduce no particular bias. However, several other deviations from perfect comparability deserve comment.

⁹ For detail on the problems of measuring social class in Taiwan, see S. Olsen (1971).

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First, despite the fact that households were matched on socioeconomic position as measured by the prestige of the father's occupation, they were not equivalent in education. Parents in nuclear families have somewhat more education than those in three-generation families, a difference that is particularly noticeable for mothers. Second, three-generation families are more likely to include other relatives than grandparents, thus confounding family size with presence of grandparents. The effects of these two unforeseen differences were controlled statistically (see below).

Finally, there is a difference which might work against the hypotheses of this study. Among the nuclear families, particularly those who were in close contact with grandparents, were many that had experienced recent divisions of their extended families. This means that many of the mothers in this group had not been living in independent families for long.

METHOD

The child-rearing practices and values of the 107 mothers were assessed by means of a standard interview schedule, similar to that used by the Six Cultures Project (Whiting, Child, and Lambert 1966). The mothers were interviewed in their own homes by Taiwanese assistants, young women most of whom were recent college graduates.

From the interview responses, items were chosen to index five socialization variables relevant to the hypotheses of this study.¹⁰ The most important of these, conformity values, was based upon a question closely resembling that used by Kohn in his studies of socialization values. From a list of seven desirable qualities, mothers were asked to choose the three they saw as most important for a sixth-grade child to possess. A high score on this dimension reflects high value placed on "obedient to parents" and "good manners," and a low value placed on "self-control" and "responsibility." The other dimensions are as follows.

Self-reliance attitudes (five items).—The mother believes that children should be permitted or encouraged to do things on their own. Example: "In general, do you think that a child ought to try to do things that are hard for him, or do you think it is best for him to ask adults for help?" It should be noted that this variable and the previous one, conformity values, are related but distinct dimensions.¹¹

¹⁰ A sixth dimension, obedience demands, was originally included in an attempt to tap a behavioral dimension which would parallel the attitudinal variable, conformity values. However, subsequent analysis made it clear that the index was measuring maternal consistency more than demands for behavioral conformity, and as a result, it is not included in this report.

¹¹ While the positive poles of the two scales (self-direction and self-reliance) bear certain similarities, the opposite poles (conformity and absence of self-reliance) are very different. The parent who values conformity desires a child who listens when

Direct punishment (seven items).—The mother indicates frequent direct punishment—in particular, hitting and yelling—for all types of infractions. Example: “When P gets a low mark on an examination, what do you usually do?”

Shame-oriented discipline (five items).—The mother uses disciplinary techniques that attempt to make her child feel guilty or ashamed. Example: “When P does something wrong, do you ever tell him that his behavior makes you very sad and disappointed?”

Affection (six items).—The mother indicates many instances of affectionate, rewarding behavior. Example: “All mothers love their children. Do you think mothers should express their affection or should they be more reserved?” Kohn’s theory does not allow predictions to a general affection variable, but several previous studies of family structure and socialization (Cancian 1965; Minturn and Lambert 1964) have included warmth or nurturance as the opposite pole of a punishment dimension. Furthermore, other conceptualizations of extended family living (Lambert 1971) have focused upon the consequences for maternal affection. Since, in this study, punishment is assessed separately, it seems important to also include some measure of affectionate behavior.¹²

In addition to these five socialization variables, an index of traditional family values was devised in order to assess conservatism in three-generation families. This was based upon five items indicating acceptance or rejection of traditional Chinese customs surrounding the family. Example: “How important is it for a family to have a male heir these days?”

The items chosen to index each of these six dimensions were inter-correlated and those which were not related to the other items were discarded. A positive response to any question was given a score of 3, and a negative response a score of 1, with 2 being reserved for the rare missing answers. A mother’s score for each index was determined by simply summing her scores for each of the component items. Thus, on an index composed of four questions, for example, scores could range from a low of 4 to a high of 12.

spoken to, follows the rules, does what adults expect of him. The parent who scores low on self-reliance attitudes, in contrast, desires a child who depends upon others for the satisfaction of his needs. The conforming child sits up straight at the table and does not use his fingers to eat his food; the child low in self-reliance lets his mother feed him. As evidence for the fact that these are separate dimensions, it may be noted that the correlation between conformity values and self-reliance attitudes is $-.18$ (Kendall’s τ), no higher than the correlations of these two variables with most of the remaining socialization variables. Of the two dimensions, conformity values is theoretically more closely related to the conceptualization of family structure advanced in this paper.

¹² The affection and direct punishment scores of the women in this study proved to be virtually unrelated ($r = -.06$), indicating that these two variables are best viewed as separate dimensions, rather than as opposite poles of the same continuum.

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The measure of association selected for this study is Kendall's τ , and no significance figures are reported.¹³ This is because the 107 mothers in this study are not a random sample of any definable universe (Morrison and Henkel 1969). As a guide to interpreting the strength of the findings, it may be useful to know that, for a sample of 107, a τ of .10 is significant at the .05 level (one-tailed test).

FAMILY STRUCTURE AND SOCIALIZATION

The findings presented in table 2 indicate support for the hypothesized effects of household composition on socialization. Mothers in three-genera-

TABLE 2
MEAN SCORES OF MOTHERS IN THE FOUR TYPES OF HOUSEHOLDS

	THREE-GENERATION		TWO-GENERATION		KENDALL'S τ^*
	Both Grand- parents (N = 25)	Grand- mother Only (N = 30)	Close Contact (N = 22)	Little Contact (N = 30)	
Conformity values	10.16	9.66	9.09	8.80	+.19
Self-reliance attitudes	9.12	9.55	9.55	10.27	-.14
Direct punishment	16.80	15.10	15.18	13.87	+.22
Shame-oriented discipline	11.08	11.86	12.09	12.43	-.14
Affection	12.96	12.87	13.55	13.43	-.06
Traditional family values	11.20	10.97	10.82	9.67	+.17

* Positive correlations indicate that high scores are associated with three-generation living.

tion families are more likely to place a high value on conformity and a low value on self-reliance; they are more likely to use direct forms of punishment, and less likely to use disciplinary techniques which appeal to the child's sense of shame. In the case of each of these variables, the importance of family structure is manifested not only in the degree of association, but in the steplike progression of mean scores from mothers having little contact with their parents-in-law to those who live together with both of their husband's parents, that is, from those having no opportunity to be dominated to those with considerable likelihood of being dominated. In contrast, family structure is unrelated to affection.

In order to determine whether these results applied equally to both rural

¹³ Tau is a rank-order correlation method appropriately applied to ordinal data. It has an important advantage over other possible measures in that it is possible to compute partial τ 's as a means of controlling for the effect of confounding variables (Siegel 1956). Figures were computed using the NONPAR CORR subprogram of the *Statistical Package for the Social Sciences*, which introduces a correction for tied ranks (Nie, Bent, and Hull 1970).

and urban mothers, the effects of family composition were analyzed within each of these two groups. In general, similar findings emerged in the two analyses, with the exception of the related dimensions of conformity values and self-reliance attitudes. The hypothesized effects of household composition on these two variables were present only among urban mothers. Because of the small size of the rural sample (22), it is impossible to say whether this particular finding constitutes a genuine exception to the general conclusion, or whether it simply reflects random variation.

Assuming for the moment that the rural mothers are in fact deviant in this respect, an explanation may be sought in other conditions of life which influence child-rearing values. Specifically, it is possible that economic factors outweigh family composition as a determinant of socialization values in this poor farming community. Data from the mother interviews confirm that nuclear families more often need to utilize the assistance of their sixth-grade children in tending farm animals and working the fields, and this type of subsistence activity is generally associated with emphasizing conformity in children (Barry et al. 1957).

In any event, because the rural and urban findings were dissimilar, the figures for the urban sample are presented separately in table 3. Excluding

TABLE 3
MEAN SCORES OF MOTHERS IN THE FOUR TYPES OF HOUSEHOLDS,
URBAN MOTHERS ONLY

	THREE-GENERATION		TWO-GENERATION		KENDALL'S τ^*
	Both Grand- parents (<i>N</i> = 18)	Grand- mother Only (<i>N</i> = 24)	Close Contact (<i>N</i> = 19)	Little Contact (<i>N</i> = 24)	
Conformity values	10.11	9.58	8.63	8.08	+.29
Self-reliance attitudes	9.33	9.83	9.90	10.67	-.16
Direct punishment	16.39	14.71	14.68	14.17	+.17
Shame-oriented discipline	11.22	12.13	12.05	12.75	-.17
Affection	14.33	13.58	14.21	14.13	-.03
Traditional family values	10.17	10.25	10.32	8.96	+.17

* Positive correlations indicate that high scores are associated with three-generation living.

the 22 rural women in fact affects only two variables; the relationship between family structure and punishment is somewhat reduced, while with conformity values it is considerably increased.

The Effects of Maternal Education

The sampling scheme used in this study was designed to control for the possible confounding effects of social class position by equating the four

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household types on class as measured by the prestige level of the father's occupation.¹⁴ Unfortunately, as was brought out in table 1, this method was not entirely successful in eliminating educational differences between parents in two- and three-generation families. It thus becomes important to determine whether the observed family structure differences in child rearing are nothing more than a reflection of differing levels of maternal education.

Education is indeed a powerful predictor of socialization values and practices in Taiwan, as can be seen in table 4. Partial correlation was used

TABLE 4
MEAN SCORES OF MOTHERS WITH VARYING LEVELS OF EDUCATION

	None (N = 28)	Primary (N = 47)	Junior Middle (N = 9)	Senior Middle (N = 17)	College (N = 5)	KENDALL'S τ^*
Conformity values	10.82	9.45	8.44	8.59	5.60	-.39
Self-reliance attitudes	8.75	9.39	9.78	11.24	11.40	+.28
Direct punishment	16.29	15.04	17.11	13.06	13.60	-.17
Shame-oriented discipline	10.85	12.15	13.44	11.94	12.60	+.20
Affection	10.46	13.11	15.78	15.89	14.80	+.48
Traditional family values	12.93	10.36	9.67	9.24	7.00	-.43

* Positive correlations indicate that high scores are associated with higher levels of education.

to control for the effects of this variable, with the results shown in table 5. With mother's educational level controlled, the association between family

TABLE 5
ASSOCIATION OF FAMILY STRUCTURE AND SOCIALIZATION,
CONTROLLING ON MOTHER'S EDUCATION

	Total Sample (N = 107)	Urban Mothers Only (N = 85)
Conformity values	+.12	+.24
Self-reliance attitudes	-.09	-.10
Direct punishment	+.19	+.15
Shame-oriented discipline	-.10	-.14
Affection	+.04	+.05
Traditional family values	+.09	+.13

structure and socialization is reduced, but not eliminated. Particularly for urban mothers, it remains true that extended family living is positively

¹⁴ A detailed analysis of social class differences in Taiwanese socialization patterns will be presented in a subsequent article (see also S. Olsen 1971).

related to conformity values, direct punishment, and traditionalism, and negatively related to self-reliance attitudes and shame-oriented discipline.

ALTERNATE INTERPRETATIONS

The results of this study support the hypothesis, derived from Kohn's theory of occupational experience and child-rearing values, that close supervision of a woman by her mother-in-law will affect her socialization values and practices in much the same way as close supervision on the job affects the values of a man.

However, the findings are also consistent with two other theories, touched upon earlier, concerning the importance of extended-family living.¹⁵ The first of these points to the general conservatism of women in three-generation families, as evidenced by their high scores on traditional family values, and argues that the socialization practices of these women are simply those that are more traditional in Taiwanese society. This may be because the mother who shares her child-care responsibilities with an older woman is less free to adopt new methods of socialization (Wolf 1970), or it may be that women in the various types of households are preselected according to their degree of traditionalism. While ethnographic evidence suggests little support for the latter hypothesis, the former is a very real possibility. If, however, the effects of traditionalism could be ruled out, this would be evidence against both hypotheses and further support for the idea that it is the experience of three-generation living that affects maternal socialization practices and attitudes.

The socialization practices that characterize three-generation mothers are in fact those of more traditional mothers, as a comparison of the first two columns in table 6 make clear. However, controlling for traditionalism

TABLE 6
ASSOCIATION OF FAMILY STRUCTURE AND SOCIALIZATION,
CONTROLLING ON TRADITIONALISM

	Family Structure	Traditionalism	Family Structure, Controlling on Traditionalism
Conformity values	+.19	+.22	+.16
Self-reliance attitudes	-.14	-.12	-.12
Direct punishment	+.22	+.18	+.20
Shame-oriented discipline	-.14	-.08	-.13
Affection	-.06	-.28	-.01

¹⁵ At least two other theories have been offered to explain the effects of three-generation living on socialization, but neither is consistent with the results of this study. Lambert (1971) is foremost among those who have argued for the influence of alternate care-

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by means of partial correlation does almost nothing to reduce the association of family structure and socialization.

It may be concluded that the more conservative family attitudes of three-generation mothers cannot account for the child-rearing practices that they employ. This offers further support for the idea that the source of child-training differences between nuclear and extended families should be sought in the different experiences that are characteristic of life in the two types of families.

One experience that might be relevant is that of living in a large group. Three-generation families not only include grandparents, but also tend to incorporate other relatives of various sorts. This is particularly true when both grandparents are present, as was shown in table 1. The original hope had been to test the effect of grandparental presence in a very controlled way by excluding other relatives entirely. But extra relatives proved to be part and parcel of the three-generation living in Taiwan, so much so that it would have been impossible to find a sample of three-generation families that included no such persons. Nonetheless, an attempt was made to minimize the selection of the larger, more complex households. The resulting sample, then, does not approach the population in complexity of family structure, but population differences are reflected in the sample.

Large, complex families demand greater cooperation and behavioral conformity, interdependence rather than self-reliance. Studying American families with varying numbers of children, Elder has shown that parents in large families have more rules and employ more direct forms of punishment (Elder 1962; Elder and Bowerman 1963).¹⁶ The effects on maternal behavior of living in a large group would seem to be precisely those which have been hypothesized to follow from close supervision.

Of course it is impossible to add grandparents to a family without also increasing its size, but the argument being advanced in this paper is that it is the addition of an authority figure, rather than simply another person, that influences maternal socialization. From this point of view, it is important to separate statistically the independent contribution of these two variables.

From table 7 it is clear that the type of punishment a mother employs

takers on maternal behavior, but this theory predicts that the presence of grandparents will be associated with greater maternal affection and less emphasis on behavioral conformity. It has also been argued that young women in patrilineally extended families are emotionally isolated both from their own husbands and from other adult women in the household (Freedman 1963; Wolf 1970), and that they turn to their children for emotional gratification. Again, this suggests greater maternal indulgence and affection in three-generation families.

¹⁶ In contrast, Kohn (1969, p. 68) finds that number of children has relatively little influence on the socialization values of fathers. The effect of family size on mothers' socialization values has not been tested.

TABLE 7

ASSOCIATION OF FAMILY STRUCTURE, FAMILY SIZE, AND SOCIALIZATION

	Family Structure	Family Size	Family Structure, Controlling on Family Size	Family Size, Controlling on Family Structure
Conformity values	+.19	+.20	+.11	+.13
Self-reliance attitudes	-.14	-.18	-.06	-.13
Direct punishment	+.22	+.13	+.18	+.03
Shame-oriented discipline ..	-.14	-.07	-.12	.00
Affection	-.06	-.23	+.06	-.23

is attributable solely to the presence of grandparents in the family, and not to family size per se. Placing a high value on a child's external conformity is related to both family size and presence of grandparents, and to approximately the same degree. On the other hand, the belief that children should be self-reliant is primarily a function of small family size, rather than the absence of grandparents. Interestingly enough, maternal affection emerges from this table as bearing a noticeable negative relationship to family size, although its original correlation with family structure was minimal.

It is not surprising that family size should influence both conformity values and self-reliance attitudes, while family structure (presence of grandparents) affects only the former. The relevant aspect of family structure has been conceptualized as mother-in-law domination, an analog to Kohn's "closeness of supervision" in the occupational world. Lack of autonomy in one's own life is hypothesized by Kohn to produce an emphasis on behavioral conformity in one's children, but his theory does not suggest, except in a very indirect way, that such parents would prefer their children to be low in self-reliance. There is, however, good reason to believe that a favorable adult-child ratio might influence self-reliance training (Whiting 1959). Since the present sample was selected in such a way as to minimize differences in number of children, variation in family size is accounted for almost entirely by the presence of extra adults. The lack of emphasis on self-reliance in large families is understandable in this context.

The final conclusion must be that three-generation living has a small but definite effect on the socialization values and practices of Taiwanese mothers, and that with the exception of attitudes toward self-reliance, this is not attributable solely to the fact that three-generation families contain more members. The argument developed in this paper is that it is domination of the mother by her mother-in-law which is crucial, but the behavior of the older women has not been measured directly. Rather, the potential

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for mother-in-law domination has been inferred from differences in family structure.

This suggests two further research strategies. First, societies which vary in cultural role of the mother-in-law could be compared. In those where the older woman is not expected to dominate her daughter-in-law, the effects of three-generation living should be minimal. Second, more direct attempts to measure the behavior of mothers-in-law should be employed, and the socialization values and practices of mothers related to some measurable degree of closeness of supervision.

SUMMARY AND DISCUSSION

To the sociologist, the term "social structure" suggests stratification; to the anthropologist, the same term means kinship. As a result of this divergence of interests, there seldom has been any attempt to abstract similar elements from the two types of structures, and thus to see similar effects on behavior.

This paper represents an attempt to extend Kohn's theory of social class differences in the child-rearing values of men to the effects of residence rules on maternal socialization practices. When custom requires a woman to live together with a mother-in-law who supervises and controls her domestic activities, the woman's daily experiences are very similar to those of the typical working-class man in the United States. In both cases, the opportunity for autonomy and self-direction is absent. This paper presents evidence that the effects of "close supervision" on socialization are similar in the two settings. In a sample carefully controlled for possible confounding variables, it was found that Taiwanese women who live together with their mothers-in-law value behavioral conformity in their children, and use direct, external forms of discipline in preference to those which appeal to the child's feelings. These results could not be explained by the simple fact of expanded group size, or by the more conservative attitudes of women in extended families.

The findings of this study contribute to the long-standing sociological debate over the sources of parental socialization practices, which in turn represents a focus for the more general theoretical controversy concerning the effects of group membership on individual behavior. The two positions, which may be labeled the "cultural" and the "structural," or "experiential," are exemplified in the socialization literature by Wolfenstein (1955) and Bronfenbrenner (1958), on the one hand, and Miller and Swanson (1958), McKinley (1964), and Kohn (1969) on the other.

The "cultural" position rests on the idea that societies and subgroups within societies maintain self-sustaining cultures that incorporate values for children and specific child-rearing techniques. These attitudes and

behaviors need not be static; indeed, cultural values may include a positive orientation toward change and progress, predisposing parents to accept new methods of child rearing as advocated by qualified professionals. Moreover, new methods might spread, through a process of cultural diffusion, to groups in less direct contact with expert child-rearing advice.

Bronfenbrenner's classic article, "Socialization and Social Class through Time and Space" (1958), although devoted primarily to reporting upon his prodigious effort to make sense out of the conflicting results of numerous studies relating social class to socialization, also includes a theory of change based on the cultural approach. Bronfenbrenner was able to show that the behavior of middle-class parents over the years followed closely the advice being given by various types of child-rearing experts. The behavior of working-class parents had also changed over time, but at a rate that lagged behind the middle class, presumably because it took longer for the communications regarding new child-rearing methods to reach them. In drawing these conclusions, Bronfenbrenner relied on the analysis by Wolfenstein (1955) of several succeeding editions of the U.S. Department of Labor Children's Bureau Publication, *Infant Care*. Wolfenstein herself had noted that the shifts over time in the advice presented in these manuals seemed to parallel general changes in the attitudes of Americans toward work and play.

The crucial causal agents in this approach are ideas, and the most important intervening variable is the extent to which a group is in contact with new ideas. This theory is most persuasive in considering the powerful effect that educational level exerts on virtually any social attitude one might wish to examine.

In contrast, the structural approach sees ideas as derivative, with some aspect of experience as the first link in the causal chain. Structural arrangements are seen as creating sets of experience that shape perceptions of reality. In explaining differences in socialization values and practices, it is one or another feature of occupational experience that has received the most attention. Thus, Miller and Swanson (1958) hypothesized that changes over time in the child-rearing practices of middle-class parents could be accounted for, not by shifts in the advice of experts, but by changes in the nature of middle-class jobs. As the typical middle-class occupational activity moved from entrepreneurial to bureaucratic, they posited, new behaviors and value orientations were required which would be reflected in parental socialization practices. While their data were insufficient to support this prediction, Miller and Swanson's approach (but not their specific hypothesis) is one that has been echoed in the work of McKinley (1964) and particularly Kohn (1969). It is Kohn's theory which has been explicated in this paper.

However, this paper takes an important new step in considering a fea-

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ture of social structure entirely distinct from social class and its attendant differences in occupational experience. Family structure (presence or absence of grandparents) has been shown to affect socialization in a manner remarkably similar to that demonstrated by Kohn for social class. This suggests that the theory is one of wide applicability, and that other analogous aspects of social structure should be sought. Possible extensions might be found, for example, in a comparison of various types of educational or political structures.¹⁷

Furthermore, in examining family structure, it was possible to rule out alternative explanations based upon the cultural approach. Because of the existence of a domestic life cycle in Taiwan, the average married woman moves over time from an extended family to a nuclear family, and then, as her sons marry and have children, to an extended family situation again. All the while she lives in a communications network composed of both extended and nuclear families at a social class level comparable to her own. Although it was found that women in extended families do tend to express more conservative attitudes toward family life, it was also shown that this could not account for the socialization values they hold or the disciplinary techniques they employ. Furthermore, with educational level held constant, family structure continued to affect socialization attitudes and practices.

Thus, it is neither in a woman's past family circumstances nor in her current culture that we will find the explanation for the effects of family structure on socialization. Rather, it is to be found in some aspect of her present life experiences.

Although the results of this single study must necessarily be regarded as tentative, they demonstrate the utility of attempting to improve our understanding of the relationship of social structure to socialization by applying Kohn's theoretical viewpoint to various aspects of social structure.

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¹⁷ A suggestive finding from the political domain has been presented by Stephens (1963, p. 372). His cross-cultural evidence indicates that severe obedience training is much more likely in societies with the type of political organization he terms a "kingdom" than it is in tribal societies.

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Instrumental and Infra-Resources: The Bases of Power

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The definition of power, which is increasingly incorporated in sociological analyses, remains ambiguous. This paper attempts to elucidate the nature of power in two ways. First, the concept is explicated in terms of instrumental resources and infra-resources. The term "infra-resources" has been developed to incorporate perceptual and circumstantial aspects of power into theoretical analyses. Second, a strongly systemic orientation to power is developed in order to provide a sound theoretical basis for departures from traditionally static analyses of power and power-relevant phenomena.

THE SIGNIFICANCE OF POWER

Because it is so integral a part of social reality, power is clearly a phenomenon of great significance for sociologists. Irrespective of the substantive area which preoccupies a sociologist, we are likely to consider the reality of power at some point in the analysis of social phenomena. Role differentiation in the family involves power differences. Socialization, indoctrination, and brainwashing also involve the use of power. Social movements and social change may be viewed from a power perspective. Concern with social deviance and social control necessitates a consideration of power. The analysis of social institutions—education, government, religion, for example—may entail the use of power as an explanatory variable. No matter what field of specialization is involved, power is likely to be considered in any thorough scheme which attempts to account for the dynamics of the social reality under investigation. In attempts to explain the nature or dynamics of a particular social phenomenon, power may be incorporated as a causal factor. The study of power, then, is "part of the larger study of the determinants of human behavior" (Cartwright 1965, p. 3). It is important to stress, however, that power phenomena are only one of those that affect human behavior; power is not the sole determinant of all human behavior (McFarland 1969, p. 12). In fact, it may be that as an explanatory variable power operates most strongly in explanations of short-lived phenomena and situations (March 1968, p. 280). In the long run, many other factors may be at least as important in explaining observed effects.

Despite the key role of the concept, though, there is little agreement as to how power should be defined. Furthermore, power, influence, control,

and other related terms seem to be definitionally indistinct among scholars. It is the broad purpose of this paper to explicate the term "power" in a clear manner. In addition, the phenomenon of power is discussed in a strongly systemic context in order to illustrate that power structures are only as stable as the social systems in which they exist.

A DEFINITION OF RESOURCE

Among the various definitions of power there are numerous permutations, yet the definitions generally seem to represent attempts to capture the idea that power has something to do with the way people affect or have the capacity to affect other people in a direction which is compatible with their own wishes or preferences. Person A, if he has power, can cause either (1) a change in person B's behavior or (2) a change in the probability that person B will behave in a given manner. This is fundamentally the nature of what is commonly labeled "power."

Differences among definitions appear to be a function of disagreements on three basic issues about power. First of all, there is the question of the fundamental nature of power. The two basic positions on this issue are that power is an ability of an actor or, alternatively, that power is an individual's exercise of some ability. Second, theorists tend to focus their definitions on different systemic levels, which include the individualistic, the dyadic, and the systemic (Clark 1968, pp. 45-46). The first orientation focuses on the individual with no definitional concern for other levels in the system. The second involves defining power in terms of a relationship between two actors, one of whom is by definition the more powerful within the relationship. Finally, the systemic orientation involves, in varying ways, defining an individual's or a group's power with reference to some given social system(s). A third divisive element among power theorists has to do with which elements or variables are most central to a conception of power. A careful examination of these three issues will constitute the basis for a definition of power.

Power Is an Ability

The first issue for consideration is whether power should be defined in terms of an ability or the exercise of an ability. In no way can these phenomena be regarded as equivalent, yet the term "power" has been applied to both of them. The use of the same term to refer to qualitatively different phenomena is unfortunate because it generates confusion and serves to impede progress toward a general theory of power. Power must be conceived as one or the other of the two phenomena.

If power is equated with an ability of some sort, then to say that a

person has the power to do something is to say that he can do that thing (Wagner 1969, p. 3). In this conception there is no implication that the person will do that thing. The most that may be ventured is that the person might exercise his ability. By definition, then, "ability" does not denote social action. However, the ability to change another person's behavior is a socially significant phenomenon. It is important because people can and do estimate one another's abilities and then adjust their actions on the basis of those estimations. The perceived or real ability to influence can, then, affect outcomes, even when the exercise of that ability is not undertaken. Furthermore, the process of assessing others' abilities to influence may lead over time to the development of relatively durable reputations for power in stable social systems. These estimates can augment a person's basic ability to influence to the extent that people tend to respond not only to the person's "objective" ability, but also to his reputed ability. Most important, people do differ in their abilities to affect others, and these differences mean that the more powerful will, if they choose to act, affect outcomes to a greater degree, if all other factors are equal. Thus, the ability to influence others would appear to be a phenomenon worthy of terminological and definitional attention. It would seem impossible to solve this first issue, then, by arguing for the abandonment of either of the two main conceptions of the nature of power. The problem becomes one of deciding which phenomenon is to be labeled "power."

An examination of the attributes of the word "power" in the English language may provide some basis for the choice at hand. "Power" is a noun. A person may have power or submit to power, but he cannot "power" another person. There is no verb form directly derived from or related to the noun "power" (Dahl 1969, p. 80; Wrong 1968, p. 677).¹ If one wishes to use the term "power" to communicate action, he must use the term with a verb; exercise power, wield power, use power. In English, then, "power" is not conceived in dynamic terms. Rather, it appears that "power" is used in the sense of an ability or a capacity which might be manifested in action. Thus, "power" is unlike the terms "influence" and "control," which can function as both verbs and nouns. Faced with the choice of using "power" to refer to an ability or using it to refer to the exercise of an ability, I would conclude that the former usage is more compatible with the structure of the English language. Thus, on the first issue I shall follow those who have defined "power" in terms of an ability, a capacity, or a potential.

¹ The verb "empower" provides an exception to this generalization. However, "empower" denotes giving or transferring power rather than "powering" and thus is not comparable to the verb "influence."

The Individualistic Orientation

The second issue which merits consideration concerns the systemic considerations in conceptions of power. One orientation focuses on the individual with no explicit reference to the social relationship or system within which power is possessed (Clark 1968, p. 45). Such an orientation typically "focuses on the degree to which a single actor achieves his desired goals" (Clark 1968, p. 45). For example, power may be defined as "the capacity to carry out, by whatever means, a desired course of action despite the resistance of others and without taking into consideration their needs" (Bredemeier and Stephenson 1962, pp. 49-50). If this type of definition were used to rank people along the power dimension, questions would arise: aside from the problem of determining a person's goals, there is the more significant problem of explaining how differences in the abilities of two people to achieve their goals are equivalent to the power difference between the two people, assuming that both people overcome equal amounts of resistance. Specifically, if person A and person B overcome equal amounts of resistance but A achieves his goal and B does not achieve his, is it fruitful to define A as having more power than B? To the extent that a focus on personal goals is present in a definition of "power," it would seem impossible to compare actors' amounts of power in any reliable fashion. Furthermore, this type of definition does not clearly suggest the social nature of power or the significance of cultural and structural variables in affecting the dynamics of power. Thus, this orientation to power is clearly inadequate.

The Dyadic Orientation

A second major approach in defining power is to specify its meaning with reference to the relationship between two actors. Unlike the individualistic orientation, the dyadic approach focuses on an actor's power in relation to a given other actor. This approach necessarily places "power" in a relational, and hence social, context. Dahl's definition of the power of actor A over actor B is perhaps typical of this type of definition. He (1969, p. 80) says that "A has power over B to the extent that he can get B to do something that B would not otherwise do." There are several reasons why a definition of this sort is not satisfactory. First, if the dyadic orientation is couched in terms such as Dahl's, there is little definitional basis for discussing A's and B's power relative to each other under conditions other than those specified by the set of "somethings" referred to in assessing A's power. The definition says that A's power is greater with respect to at least one "something," but there is no clear basis for concluding that A's power is generally greater than B's even within their relationship. Again, there is the problem of how this definition could be

used in ranking actors with respect to power. Second, the definition may be interpreted as meaning that A is able to cause a change in B's behavior from what B would like to do to what A prefers that he do. However, A's capacity to affect B's behavior cannot be considered a sufficient condition for the change in B's behavior, since B must decide whether or not to act in the direction of A's preferences. Thus, "at most A's intervention is a necessary condition for B's action and therefore is an incomplete explanation of it" (Wagner 1969, p. 5). It is necessary, then, to take into account definitionally the possibility of bargaining between A and B and the possible reciprocity of influence between the two (Harsanyi 1969, p. 232). In its typical form, then, the dyadic orientation is not sufficiently suggestive of the numerous factors which render power more than a unilateral and simple relationship between two actors.

The Systemic Orientation

The systemic orientation consists of two analytically distinct approaches. In one version of the systemic orientation, power is conceived as a property of the system; in the other version power is conceived as "a system-relevant property not readily transferable to another arena" (Clark 1968, p. 46). The first version commonly occurs in the context of a functionalist view of power and emphasizes a system's capabilities of realizing its goals or the ability of a unit of the system to attain a systemic goal. For example, power may be conceived as "the ability of a system to utilize and mobilize resources for the achievement of collective goods" (Gamson 1968, p. 12). In other terms, power is "the generalized capacity of a social system to get things done in the interest of collective goals" (Parsons 1958, p. 206). With reference to an individual or group within the system, this type of systemic orientation takes a form such as the following definition: "The power of a unit is its capacity through invoking binding obligations to contribute to collective goals, to bring about collective goal-outputs that the 'constituents' of the processes of the collective action in question desire" (Parsons 1964, p. 46). That this kind of definition taps an extremely significant aspect of social power is undeniable. When power is by definition a property of the system, the individual actor constitutes only a basic unit in the system, only one part that may have a share in this systemic property. For the study of economic development or differential nuclear capabilities, it may be quite useful to speak of the power of a system to achieve its goals. However, for the study of how power arrangements affect decision- and policy-making processes, the utility of this kind of conception is questionable. It would seem more fruitful to reverse the above conceptions and ask how power, as a system-based individual or group phenomenon, affects the system as a whole.

Furthermore, to define an individual's power in terms of systemic goals is unnecessarily restrictive, aside from the methodological difficulties involved. People differ greatly in the amounts of power they have. Some may directly affect the decisions which result in goal achievement for a particular system; others may have a capacity for only indirectly affecting such decisions but may have the ability to influence their friends and/or acquaintances. While there are different degrees of power involved in these cases, it seems unlikely that the social processes involved are qualitatively different. Thus, the first type of systemic orientation—power as a property of the system—is of doubtful utility to scholars interested in working toward a general theory of power.

In the second type of systemic orientation, power is regarded as a phenomenon closely related to particular systems and not easily transferable to other systems. This kind of orientation typically emphasizes "actors operating in one or more status positions within a specific social system" (Clark 1968, p. 46). A person's roles and his overall status in the system are considered central aspects of his capacity to influence others. For example, power may be defined as "the *capacity* or *potential* of persons in *certain statuses* to set conditions, make decisions, and/or take actions which are determinative for the existence of others within a given social system" (Schulze 1961, p. 20). Attempts to understand power, then, are thought to necessitate a consideration of the positions that a person occupies in a given social system. Unlike the individualistic orientation, this kind of systemic orientation does not absolutize an individual's power, but views it as relative to specific social systems. Furthermore, this orientation implies that a change in one's social roles may significantly alter the amount of power at the individual's disposal. This is an important implication, because it forces the scholar to consider power in dynamic terms. This orientation appears to offer a useful perspective from which to study power. Thus, I shall regard power as an individual or group phenomenon which must be examined relative to specific social systems.

Resources

The final issue is what elements should be included in a definition of power. Several lists of such elements are available (Abramson et al. 1958, p. 15; March 1968, p. 269). However, rather than discuss this issue in general terms, it would seem more beneficial to examine my theoretical decisions up to this point and determine what they suggest about the elements to be included. Thus far I have made two choices, namely, that power is defined with reference to ability and that it is regarded as a system-based phenomenon. First let us turn to a discussion of the implications involved in defining power as an ability or capacity or potentiality. As Parsons

(1964, p. 41) has indicated, "capacity" "means essentially the command of facilities necessary for effective action." In other words, ability rests on the holding of those factors that are necessarily involved in the externalization or manifestation of the ability. If one has no access to or control over such factors, he does not have the given ability. If power is defined as a kind of ability, it is then necessary to include in the definition some reference to those factors which are part and parcel of the ability. Those factors might be called "resources."

Nuttall, Scheuch, and Gordon (1968, pp. 352-53) have defined a "resource" as "anything which allows one actor to control, provide, or apply a sanction (positive or negative) to another actor." This conception of resources does not equate resources with potential sanctions. In addition to potential sanctions, the definition includes those factors which are necessary for the actualization of the ability to sanction. Consider time, for example. An actor may have control over sanctions which are applicable to another actor, but if that application requires the consumption of a great deal of time which the actor can ill afford, then the availability of adequate time becomes a necessary factor for the actualization of the actor's ability to apply the sanctions. In other words, time is a resource, since it is one of the factors that allow the actor to apply the sanctions.² Similarly, it appears that energy is a resource. Both time and energy occur in finite quantities and cannot be assumed to be constantly and limitlessly available to those who are in control of potential sanctions. In including time, energy, and other such factors as resources, the definition cited above is more useful for an understanding of power than are conceptions which equate resources with potential sanctions.

If resources are considered for the purpose of understanding power more fully, however, Nuttall et al.'s definition is deficient in one major respect. Their definition limits the term "resource" to two broad categories of phenomena, factors that can be used as sanctions and factors that permit the application of sanctions. If power broadly involves the ability to affect the behavior of other persons, it appears that there are other factors that do not fall into either of the categories specified above. The

² There may be some who would argue that this is a matter of motivation; for example, if an actor really wants to apply the sanctions, then he will make or find the time to do so. However, people can do only a finite number of things within a given period of time. It may sometimes be the case that a person may really want to take on another commitment but cannot spread his time any more thinly. Furthermore, under some circumstances one must "have the time" at the right time or the opportunity to sanction passes him by. Ultimately, a case can be built for a motivational treatment of this issue by claiming that a person can renege on his previous commitments in order to provide the time for what he really wants to do at the time he wants or needs to do it. However, many persons—perhaps most—probably do not perceive the aspect of ultimate choice in this kind of situation. It seems reasonable, then, to regard time as a resource.

most obvious is information. In some cases information may be invoked as a potential sanction (e.g., blackmail). Often, though, information is communicated for the purpose of changing another's view or vote. When information is so used, it need not involve sanctions, but it may involve a change in the recipient's behavior. Unless one maintains that the receipt of information is inherently rewarding or punishing, its use for purposes of persuasion does not necessarily involve sanctions. Thus, Nuttall et al.'s definition seems to exclude an important aspect of power.

Necessity seems to demand a rather broad definition of the term "resource." The following represents an attempt to include those cases that are logically excluded from other definitions of the term. *A resource is any attribute, circumstance, or possession that increases the ability of its holder to influence a person or group.* (To influence is to change the probability that a person or group will adopt the behavior preferred by the influencer as a result of his actions toward that person or group.) This definition indicates that attributes may be resources. Examples of this case would be reputation for power, high social status, physical attractiveness, and charisma. Circumstances that could be resources include such things as holding political office, access to influential persons, a flexible work schedule, and committee memberships. Possessions that could constitute resources are such things as wealth, land, and ownership of a mass medium. These three cases are not thought to constitute different "kinds" of resources in the sense of having qualitatively different effects on the influence potential of their respective holders. The three terms are used in the definition only to cover all those phenomena that may be resources under various conditions.

INSTRUMENTAL RESOURCES AND INFRA-RESOURCES

There are two distinct kinds of resources implied by the definition above. I have labeled these "infra-resources"³ and "instrumental resources." Instrumental resources are those that can be activated or invoked as a means of attempting to influence. In a given situation, they are the resources that are utilized to change the probability that a person or group will act according to another's preferences. In simple terms, *instrumental resources are the means of influence; they can be used to reward, punish, or persuade.*

Infra-resources are those attributes, circumstances, or possessions that must be present before the appropriate instrumental resources can be activated or invoked; in that situation they are the preconditions or prerequisites without which instrumental resources are useless. For example, information may be an infra-resource. Let us assume that person A has

³ I would like to thank Dr. Hwang Joe Kim for his suggestion of this term.

instrumental resources that could affect the vote of a local governmental body on a zoning issue. Let us further assume the following conditions: (1) person A has no personal access to the members of that governmental body; (2) the governmental body will have a final public meeting on the issue tonight at 8:00. Unless A receives information about the meeting, he will not activate his resources in that situation and hence cannot affect the outcome. In this situation, then, information about the meeting is an infra-resource.

In a variation of the above case, let us assume that A does know of the meeting but has a pressing commitment at 8:00 tonight. In this instance, time is an infra-resource that A lacks. Unless he can send an effective delegate to the meeting, he lacks an infra-resource that would permit him to use his power in that instance. In a final variation of this case, let us assume that A has the appropriate instrumental resources, has information about the meeting, and has the time to attend but does not perceive the relationship between his own interests and the zoning issue. If self-interest is assumed to be a factor in motivating participation, then the ability to perceive the possible effect(s) of pending decisions on one's interests is an infra-resource. Lacking it, A will probably not attend the meeting, and hence his instrumental resources remain unactivated relative to that situation.

In some instances money may be an infra-resource. For example, a person may need to cross the country in order to set the conditions necessary for the effective use of his instrumental resources. If the would-be influencer has no money for such travel, he cannot activate his instrumental resources for optimal use. Thus, money can be an infra-resource. It should be noted at this point that most resources can be either instrumental or infra-resources, depending on the nature of the influence situation. Money, for example, may be used to set the conditions for activating instrumental resources (an infra-resource), or it may be the means of influence, as when one promises a sizable campaign contribution if the candidate will "soft-pedal" ecological issues (an instrumental resource). In general, infra-resources are situationally specified; it is the nature of the situation that defines which infra-resources are necessary. Money and access to influential persons and information, for example, may be infra-resources in some situations but not in others. These same resources may also be used instrumentally. Some infra-resources, however, such as time and energy, are necessary to nearly every attempt to influence, and are incapable of being instrumental resources.

Some further examples may serve to illustrate the distinction between instrumental and infra-resources. Knowledge and expertise are highly significant resources, albeit subtle and taken-for-granted ones. In highly specialized systems it is often necessary to seek the advice of experts

(Bross 1965, p. 30) in order to predict the consequences of an action and to control those consequences. Politicians, for example, need two distinct kinds of knowledge—technical knowledge in specific policy areas, and “political knowledge of the relative strength of competing claims and of the consequences of alternative decisions on a policy issue” (Truman 1963, p. 334). Persons who can furnish these kinds of knowledge for public officials hold valuable instrumental resources. The person with specialized knowledge, then, can save the politician time and effort, and also enable him to proceed with greater confidence (Thibaut and Kelley 1959, p. 109). Not only can the expert affect the choices people make, but also the very perceptions people have of the alternatives open to them (Bloomberg, Sunshine, and Fararo 1963, p. 111). Thus, knowledge and expertise may serve as instrumental resources.

Knowledge of a less specialized sort is also important. Some insights into politics and the governmental process would seem to be a prerequisite for political participation. People hesitate to participate in what they do not understand (Friedrich 1950, p. 238). In this sense knowledge may constitute an infra-resource with respect to the use of certain influence channels.

While “knowledge” refers to general propositions, “information” refers to isolated facts (Berelson 1966, p. 493). The possession of information about a situation or issue is generally a prerequisite for action (Reissman 1954, p. 79). Information about local events and situations is not, though, distributed equally among the people who might be interested (Mowitz and Wright 1962, p. 2; Sykes 1951). The differential distribution of information may result in some people’s being oblivious to the fact that there even exists a situation or event in which they might like to become involved and apply their resources. Among those who are involved, the distribution of information may have a significant effect on their varying images of the problem or task at hand (Snyder 1958, p. 29). In other words, the possession of relevant information can affect one’s ability to use his instrumental resources. Furthermore, information is exchangeable and may thus be either an infra- or instrumental resource.

A wide circle of friends and acquaintances is also a resource. Much of the influence one is capable of wielding on a personal basis (as opposed to a positional basis) requires some kind of personal relationship between those who influence and those to be influenced. Previous contact with others is often a prerequisite for using one’s power in relation to them. Thus, a large number of friends and acquaintances may constitute an infra-resource. Like all resources, this one is unequally distributed in most social systems (Foskett 1959, p. 323).

Having friends and acquaintances can also be an instrumental resource. For example, person A may bargain for a favorable outcome from person

B by agreeing to influence his acquaintances in a direction desired by B. In a sense, then, person A can activate such a resource in order to elicit a desired response from B. Of course, these possibilities exist only when B needs or desires some particular outcome which necessitates indirectly influencing person A's acquaintances.

These are but a few of the instrumental and infra-resources relevant to an understanding of differential capacities for influence that might be used to illustrate the difference between the two types of resources. In both categories of resources, however, it is clear that resources are unequally distributed among the members of any given social system. It is often implicitly assumed that infra-resources such as time are equally available to all persons in a given situation and that potential participants differ only with respect to their control over instrumental resources. These are simplistic, untenable assumptions. The notion of infra-resource is useful in understanding why persons do or do not activate their relevant instrumental resources in specific influence situations. Specifically, perceptual and circumstantial aspects of potential influencers' situations are fundamental factors affecting participation.

POWER AND SOCIAL SYSTEMS

It will be useful to define "power" at this point. Power is the holding of some number and some quantity of attributes, circumstances, and possessions that increase the ability of the holder to influence a person or group.⁴ In short, *power is the ability, which derives from the requisite resources, to influence.*

Having defined power in terms of ability, I shall now consider the systemic orientation that was selected earlier. In studying power two systemic aspects are of paramount importance—the distribution of resources in the social system and the values that characterize the system. The distribution of resources provides a crude indicator of the different levels of need and of influence ability in the population. Given an unequal distribution of resources, those toward the below-average end of the distribution may have more unmet needs and be more influenceable; at the same time the relative degree to which they lack resources handicaps them as participants in systemic exchange processes. On the other hand, those who have excessive amounts of resources have the upper hand in the exchange system. They are able to transform part of their "excess" into influence by trading some resource(s) for the favorable outcome from

⁴ The term "holding" is used in this definition, even though "control" might seem a more appropriate term for use here. I believe that the term "control" should be reserved for specialized and careful use in conjunction with terms like "power" and "influence."

bargaining with others for changes in their behavior. This is not to say that, given a resource distribution, one can draw conclusions about how and when and to what degree those resources will be used. Rather, the distribution of resources reflects levels of influenceability in the population of the system.

The distribution of resources must be studied with reference to specific social systems, since distributions vary from one system to another. At any given point or period of time the distribution of resources in a social system constitutes the power structure of that system. A number of aspects of power structures are important. Assuming that resources of high generality (i.e., wide applicability) are known for a given system, its power structure must be examined vertically; that is, it is important to know the shape of the distribution and the distances between broadly defined levels in that distribution. Horizontal considerations are also significant. At any given level people may differ in the amounts of specific resources which they hold, even though their overall power ratings fall within a specified range. Vertical distinctions provide the basis for discussing the possibility of influence between levels; horizontal considerations provide data for assessing the possibility of influence within levels. It might be hypothesized that effective influence is more likely to occur among people within the same level than among those from different levels. This is to be expected because people at lower levels are by definition unlikely to have enough resources to be deemed worthy of exchange by those at higher levels. Furthermore, to the degree that those at the same level in the power structure are differentiated with respect to the specific resources they hold, they can exchange with each other in a profitable manner. Those with excessive amounts of one or more resources may bargain in such a way that, while retaining a large amount of those resources relative to the total population, they can augment their possession of those resources that they do not have to the same degree. This is not to say that all exchanges will be intralevel. The fact that many resources are cumulative makes such a hypothesis untenable.⁵ Nevertheless, the distribution of resources and its effect on who influences whom are important phenomena that must be taken into account in the study of power. Because the distribution varies among social systems, the system itself must be confronted in a thorough analysis of power.

System Values

The social system is also relevant because of the values associated with it. Nearly anything can be a resource in highly idiosyncratic situations, but

⁵ This is the case because resources, being cumulative, can be pooled. Thus, a collectivity of low-power persons may pool one or more resources and effectively influence a high-power person.

those with a characteristically high degree of generality are those related to specific social systems and to the value schemes associated with them. Let us briefly examine why this tends to be the case. The person who holds a resource may offer it, in whole or in part, to another person in return for some behavior or resource(s) suited to his purposes or desires. The proffered resource must be something that the other person needs or wants. The more widespread the need or desire for that resource, the greater the generality of the resource. Across societies there are some human needs that do not change. Food, shelter, and clothing are basic necessities. People are also interested in their health and status (Lenski 1966, p. 37). It is clear that people also seek "creature comfort" and salvation and affection (Lenski 1966, p. 38). Insofar as these are relatively universal needs and/or desires, it would seem that those resources which substantially facilitate their achievement would exhibit a high degree of generality. In this connection two problems arise. First of all, the rank ordering of the few needs and desires considered thus far varies from one social system to the next. For example, in one social system salvation may be much more highly valued than creature comfort, and in another social system the emphasis placed on creature comfort may greatly outweigh the attention given to salvation. Second, factors that contribute to such things as status and salvation also vary from one system to the next. For example, wealth, lineage, number of children, sanctity, and other circumstances vary in the extent to which they augment status across social systems. Thus, in order to observe accurately and to account for resources with a high degree of generality, it is imperative that the values of the given social system be carefully examined.

System Norms

Social systems are also important in the analysis of power because of norms that may affect the exchangeability and generality of resources. Norms may develop concerning the circumstances under which certain resources may or may not be used. For example, the norms that govern jury deliberations militate against outside influence on the 12 persons. Persuasive skills and high social status may be resources among jury members, but money and other inducements from partisans not on the jury are generally without value in this situation because of the norms which prevent their application. There are also norms which proscribe the use of certain resources with respect to specified targets of influence. For example, one is not supposed to bribe public officials. Nor is one supposed to use physical force—a resource—to obtain sexual gratification. Although deviation from the normative order always exists to some extent, it is

nonetheless important to consider norms when assessing the applicability of resources.

Roles in the System

Roles within a social system must also be considered in approaching the topic of power. A person's roles affect his power. The various institutions that make up a community, for example, are not equal in prestige or perceived importance. A high position in a prestigious institution can afford the opportunity for greater influence. Furthermore, the incumbents of some roles have special rights and privileges that constitute resources. Finally, the members of a social system are interrelated by a series of complementary roles. The system of role interrelationships involves to a considerable extent superordinate and subordinate positions or, alternatively, high-power and low-power positions, if it is assumed that the occupants of superordinate positions are given the means to exercise their prerogatives. To examine the variety of roles in a social system is, then, a task likely to be worthwhile for the power analyst.

It should also be noted that the relevance of social systems in studies of power rests in part on the fact that an individual's power usually fluctuates as he moves from one social system to another, for example, from the family to the job or from one community to another. Because power is based on and linked with roles, acquaintanceship patterns, credibility, and other such phenomena, one's power does not remain constant as system boundaries are crossed. Obviously, resources are often transferred across systemic boundaries, but the patterns of power are more likely to develop and maintain themselves primarily in and around specific social systems.

Finally, social systems must be examined to understand why power structures change. A power structure is not a static phenomenon, but rather a pattern that can be observed at a given moment or during a specified period of time. The dynamism of power structures rests largely on systemic changes. Normative change, change in the prestige of institutions, the addition of large numbers of "outsiders" to the system, changes in values, and the like can change not only the distribution of resources, but also the very nature of what constitutes a resource in the system.

CONCLUSION

Power, then, is a system-based holding of resources. My discussion has implied that the term "power" cannot be used in any categorical or absolute sense. This point merits explicit attention, however, since the term

"power" has often functioned categorically (Oppenheim 1960, p. 437). Wrong (1968, p. 677) has maintained that "power, when thought of as a capacity, is a dispositional term." As I have treated power, there is no basis for asserting that power is an individual disposition. My orientation suggests that power is necessarily a matter of degree and that its existence is closely bound up with social systems. Even when, theoretically, a person simply maintains the status quo with no increase or decrease in his own resources, power cannot be conceived as a dispositional term. The longer that person maintains his own resource position without any change, the more likely it is that his reputation for power and the expectations others have of him will be modified, thus changing the amount of his power. Furthermore, changes may occur in the system that tend to increase or decrease his relative power. Thus, even in this extreme (and unrealistic) case, power cannot be said to be a fixed or stable disposition. Rather, it is a social phenomenon susceptible to the ebb and flow of the social life in which it is embedded. In sum, the elements of power are indeed people, social organizations, and resources (Bierstedt 1950, p. 736).

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Social Selection and Stratification within Schools¹

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This paper examines the degree to which stratification within schools contributes to the prevalent association between socioeconomic background and educational outcomes of students. The institutionalized mechanisms for social selection and differentiation, such as tracking and curriculum assignment, are assessed in 48 urban public high schools. Little evidence is found for overt class bias in placement. The relationship between socioeconomic status and curriculum assignment is largely mediated by verbal achievement. Stratification patterns entail segregation and the allocation of resources within schools, however. The paper concludes that educational outcomes depend more on the criteria of selection than on the process within schools.

The most comprehensive treatment of social mobility to date (Blau and Duncan 1967) presents evidence that education is the critical intervening variable in the intergenerational transmission of status. Father's occupational prestige and education were found to contribute only modestly to a son's occupational attainment when education was controlled. Although socioeconomic origins clearly facilitate educational attainment, education was interpreted as mediating and diminishing the effects of background. This basic model of occupational attainment has been elaborated to further specify how socioeconomic background influences educational outcomes through achievement test scores, grades, and aspirations (Hauser 1969, 1970; Duncan, Featherman, and Duncan 1972) and through social psychological variables such as the influence of significant others (Sewell and Shah 1967, 1968*a*, 1968*b*; Sewell, Haller, and Ohlendorf 1970; Sewell 1971). While such additions have contributed much to our knowledge of the processes operating, they are based on samples from specific geographic regions and focus primarily on student characteristics rather than the institutionalized mechanisms for selection and differentiation within schools. The perspective adopted in the present analysis is that social selection is a process operating within schools and that aspirations and attainment are in part structured by such academic differentiation. Rather than passively implement student aspirations and ambitions, I shall argue that schools structure attainment selectively through differentiation and the allocation of rewards. Consideration of the internal mechanisms through

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which pupil sorting and selection occur is a pertinent means of assessing the criteria and outcomes of educational stratification.

One crucial mechanism for academic differentiation and selection is the high school curriculum. The linkage between high school graduation and college attendance is fundamental to differential attainment and is in part structured by access to a college preparatory curriculum. The comprehensive public high school is the norm in American education (Conant 1967), and most schools maintain separate curricula for college-bound or vocationally oriented students. Estimates based on a national sample of students (Flanagan, Cooley, et al. 1966; Jencks et al. 1972) indicate that 85% of the high school seniors in a college preparatory curriculum enrolled in college the following year, compared with 15% who were in another curriculum. Examining the determinants of curriculum assignment allow one to assess the relative importance of background variables and ability in predicting differential attainment within and between schools and to evaluate both the equity of outcomes and the degree of discrimination prevalent.

Tracking and assignment policies typically segregate students within schools and define an academic hierarchy through which certain rewards may be allocated. The general conclusion that resources do not determine achievement differentials between schools (Coleman et al. 1966; Jencks et al. 1972) ignores stratification patterns and access to resources within schools and necessarily understates the effects of such resources. If access to better teachers, counseling, and highly motivated, academically oriented peers affects achievement to any degree, such resources should operate between curricula within schools as well. Inferences on the effects of resources in a nonexperimental study are hazardous, and it is possible to assess the importance of differential allocation within schools to only a limited degree. The point, however, is that an examination of stratification patterns within schools seems a more fruitful method of assessing the role educational institutions play in social selection and the allocation of educational resources.

RESEARCH REVIEW

Educational differentiation and grouping are widespread practices in both elementary and secondary schools. The National Education Association (1968a) reported that 85.4% of all secondary schools group extensively by ability between classrooms. The literature on the effects of ability groupings in education is voluminous but quite contradictory. Early studies tended to favor ability grouping and homogeneous classrooms, but so did the researchers, often quite explicitly (Billett 1932; Ekstrom 1959). Ability grouping is a popular dissertation topic in education, and many

studies have been conducted in university high schools or other atypical environments. The duration of many experiments is frequently one year, and there is evidence that a Hawthorne effect biases initial results (Borg 1964). Many studies did not inspect within-group variances in achievement but looked for mean differences between groups; one could argue that this confounds results if the lowest students in every group gained the most. Many studies attempted to match students after the experiment or disregarded the possibility of severe regression effects. Few studies controlled the degree of information provided teachers; if the mechanism promoting differential learning is teacher's expectations (Rosenthal and Jacobson 1968), this would be a crucial flaw. It is clear that most teachers prefer homogeneous grouping and that most prefer teaching average or high-ability students (National Education Association 1968*b*). The rationale that grouping increases achievement at any level, however, is not supported by the most careful research (cf. Goldberg, Passow, and Justman 1966; National Education Association 1968*a*).

Sociologically, the experimental literature is largely peripheral because the studies have artificially grouped pupils using criteria established by the researcher instead of examining the outcomes of actual practices. Most of the sociological research on educational selection has been case studies of communities or particular schools. Conclusions, therefore, may not be applicable to social processes in a more complex, differentiated system. A highly germane summary of early findings in Yankee City, Old City, and Hometown can be found in *Who Shall Be Educated?* by Warner, Havighurst, and Loeb (1944, p. 64). Warner et al. argue that "the social class of a pupil and his family has considerable influence in the choice of high school curriculum" and that "the tendency for social class standards to overrule ability leads to a good deal of reshuffling or attempted reshuffling by the high school teachers." A study by Brookover, Leu, and Kariger (1968) of seventh graders in a large midwestern town furnished data for the plaintiff in *Hobson v. Hansen* (1967), finding that lower-class students were more likely to be misclassified and were often downgraded with respect to ability level. Hobson argued persuasively that in Washington, D.C., tracking was initiated in response to court orders for redistricting and served to perpetrate racially segregated classrooms in nominally integrated schools (cf. McPartland 1968). Cicourel and Kitsuse (1963, p. 23) studied Lakeshore High School, a large suburban school with strongly academic standards, and concluded that "administrative decisions are crucial for the process by which students are qualified for college entrance. These decisions may be independent of the students' college-going aspirations, and they significantly control the flow of students through the several curricula of the High School." Such assertions require

broader support and more precise quantification than can be provided by case studies, however provocative the results.

DATA AND METHODS

The analysis is based on data collected by the *Equality of Educational Opportunity* survey conducted in the fall of 1965 by James Coleman and his colleagues. The sampling schedule and response rates for the survey have been well documented elsewhere (Coleman 1966). The original analyses were based on students randomly sampled from each grade level surveyed and weighted according to the sampling probabilities for school strata. Due to nonparticipation by several large districts and incomplete returns from others, the final sample was not random; utilizing the original school weights for a sample of students, therefore, did not seem useful. For the present analysis, a subsample of high schools was selected from the total population of schools, and all students enrolled in these schools were included. Since it was desirable to compare two cohorts of pupils, ninth graders entering high school and twelfth graders in the final year, only four-year high schools were included and only those schools with returns for both the ninth and twelfth grades as well as from the principal. The study was further restricted to comprehensive metropolitan schools located outside the South, since the data were generally more complete for these schools and the student bodies larger. With these limitations, the analysis consists of the total subsample of urban, non-Southern high schools with a grade span of four years. There were 48 such schools available, enrolling 15,384 twelfth graders and 15,894 ninth graders.

The schools selected do not constitute a random sample of any particular universe, nor do the students. The sample does represent a large and heterogeneous population of urban non-Southern high schools and is perhaps more representative of American secondary education than smaller studies conducted within one district or school. Distributions of the total sample by sex and father's educational attainment within racial groups compared quite favorably with current population reports for urban pupils enrolled in public high schools in 1965 (Heyns 1971). Nonwhite students are overrepresented in approximately the same degree as the original survey. The 48 high schools selected are larger, slightly more affluent in terms of facilities and staff, and more likely to be racially integrated than the "typical" American high school. Such limitations imply that generalizations based on this study should be made cautiously.

The methodological techniques utilized were primarily regression and analysis of covariance. The models to be presented refer to within-school equations derived using a method very similar to that recommended by

Hauser (1969). Path analyses summarize the relationships studied, allowing a rigorous quantitative decomposition of variance, with multiple causal relationships explicitly portrayed (Duncan 1966). Linear, additive relationships among variables are assumed which operate in a specific causal sequence through a series of recursive equations. In the present analysis, three socioeconomic background variables, father's occupation (O), father's educational attainment (E), and the number of siblings (S), are considered to be logically prior, unanalyzed effects which influence verbal achievement (V) directly and curriculum placement (C) both directly and indirectly. Socioeconomic background and verbal achievement are assumed to be antecedent to curriculum placement (C), grades (G), and student aspirations (A).

All variables included in the analysis were obtained from student self-reports. Response rates varied between 84.9% and 100.0% for the twelfth-grade sample. Correlations were computed pairwise, maximizing the information available. Father's education (E) was coded as the number of years of schooling completed. The number of siblings (S) was coded as the actual number of children in the family. Father's occupation presented some difficulties, since respondents were asked to choose among census titles, and many students did not respond. Duncan's occupational prestige index seemed inappropriate for an urban population with high nonresponse rates. Each occupational category was instead scored as the proportion of fathers graduating from high school in that occupational group. Family income was not available for students in the survey and could not be included in an occupational index. The resultant scale correlated with Duncan's socioeconomic index quite highly ($r = .92$) across students in the survey. The chief difference between the two scales was a relatively higher value imputed to the status of farm occupations, which was not surprising given the strictly urban location of schools. The occupational scale used yielded slightly higher zero-order correlations with other variables included in the analysis and had the advantage of being internally consistent for the sample.

The dependent variables included were raw scores on the verbal achievement test (V), which ranged from 0 to 60 points. Curriculum placement (C) was coded 0 for all non-college-preparatory students and 1 for students in a college preparatory curriculum. Grades (G) were self-reported averages for all courses taken, coded from 1 to 5. Aspirations (A) were educational aspirations and were coded as the actual years of schooling aspired to by the student. Table 1 presents the means, standard deviations, and proportion of variance between schools for the variables included in the analysis.

The within-school parameters were computed by fitting equations of the following form:

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$$V_i = b_0 + \sum_j^{47} b_j D_{ji} + b_{48} O_i + b_{49} E_i + b_{50} S_i + U_i, \quad (1)$$

where V is the student's verbal achievement, O , E , and S are background factors, and D_j are 47 dummy variables, one for each of the $(N - 1)$ schools in the sample. The unstandardized regression coefficients in such a model are identical to those which would be obtained by using deviations from school means for each of the variables within schools. The standardized coefficients, used in the path analysis, were obtained by standardizing the coefficients obtained in equation (1) by the within-school ratio of the standard deviations for the appropriate dependent and independent variables.

The analysis of covariance provides an elegant method for decomposing the variance into between- and within-school components, allowing one to evaluate within-school processes separately from processes operating between schools. The substantive rationale in the present example is that curriculum placement and grading are social processes occurring within schools and should be analytically distinct from the effects of being in a particular school. The probability of being placed in a college preparatory curriculum thus depends on both the student's specific position relative to others within the school and the relative size of the college preparatory curriculum compared with other schools.

Utilizing the analysis of covariance in this manner involves assuming that all school effects are additive and that interactions between school and processes within schools do not exist, that is, assuming the process is the same in every school. The test for such an assumption is equivalent to testing for differences between within-school slopes, for each pair of variables, and for all partial relationships analyzed. The proportion of

TABLE 1
MEANS AND STANDARD DEVIATIONS OF VARIABLES FOR TOTAL POOLED SAMPLE
AND PROPORTION OF VARIANCE BETWEEN SCHOOLS, TWELFTH GRADE

Variable	Mean	SD	Proportion of the Variance between Schools (E_{yx^2})
Verbal ability	36.633	12.834	18.126
Father's occupation	55.151	18.702	13.073
Father's education	11.656	3.414	14.417
No. of siblings	2.860	2.263	8.204
Grades	3.421	0.910	5.433
Curriculum placement	0.520	0.500	10.827
Aspirations	15.014	2.222	10.951

variance explained by interactions in the present model was in every case significant but in no case greater than 6.2% of the total variance. In general, the first-order interactions were larger than the sums of squares explained when other explanatory variables were added to the equation; they may represent nothing more than heterogeneity in the data.

Although the magnitude of observed interactions was not large, the crucial question is the substantive importance and the interpretability of interactive effects. In the present case, differences between schools in within-school zero-order slopes were not systematically related to each other, to school size, or to school location. In addition, models computed for each school separately and averaged over all schools in the sample did not alter or distort the pattern of effects analyzed. For these reasons, the within-school coefficients were chosen as representative of the effects studied and as a convenient tool for interpreting a large amount of data.

THE BASIC MODEL OF STRATIFICATION WITHIN SCHOOLS

The initial objective is to ascertain the degree to which curriculum assignment reflects social class background versus tested ability. The zero-order within-school correlations for twelfth-grade students are presented in table 2. The basic model of stratification within schools assumes that socioeconomic background influences the verbal achievement scores of pupils and that background variables in conjunction with achievement scores influence placement. The effects of social class on placement operate directly and indirectly through verbal achievement. Figure 1 presents the basic model of stratification within schools.

The relationships depicted in figure 1 can be summarized quite briefly. The social class background measures are highly interrelated; yet each has a unique effect on achievement scores. Taken together, social class accounts for less than 10% of the within-school variance in twelfth-grade achievement. The importance of verbal ability in predicting curriculum placement is immediately apparent, with a direct effect of .44. The unique effect of verbal ability on curriculum placement explains 17.6% of the variance, or slightly less than 65% of the total variance explained. The unique effects of social class explain 3.2% of the total variance in curriculum placement. The joint effects of verbal achievement and social class are somewhat larger, accounting for nearly 25% of the total variance explained. The total effect of socioeconomic status, however, is still less than the unique effects of tested verbal ability.

The critical aspect of stratification within schools which figure 1 was designed to test was the relative magnitude of socioeconomic status and verbal achievement in predicting track placement. In order to argue that discrimination within schools accounts for the relative success of middle-

TABLE 2
WITHIN-SCHOOL ZERO-ORDER CORRELATION MATRIX, TWELFTH GRADERS IN 48 HIGH SCHOOLS

	V	O	E	S	G	C	A
V) Verbal ability	1.0000	.2049	.2267	-.2014	.3712	.4933	.4250
O) Father's occupation	1.0000	.4653	-.1311	.1524	.2382	.2399
E) Father's education	1.0000	-.1489	.1611	.2596	.2785
S) No. of siblings	1.0000	-.1105	-.1661	-.1550
G) Grades	1.0000	.3081	.3007
C) Curriculum placement	1.0000	.5607
A) Aspirations	1.0000

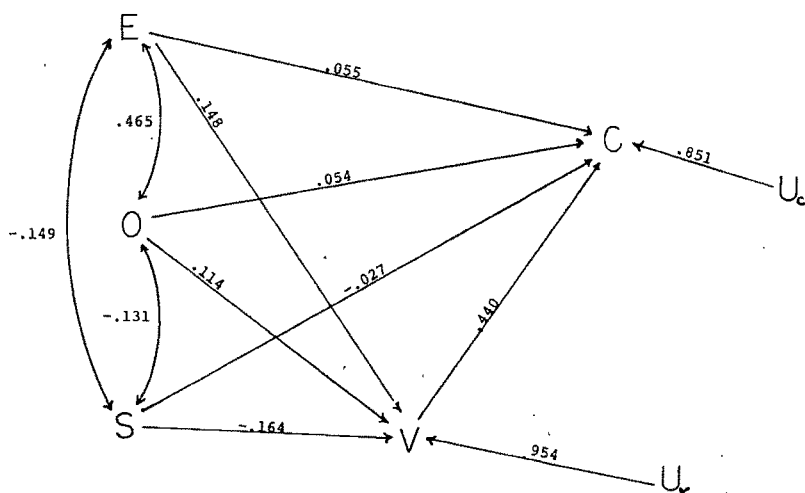


FIG. 1.—Path diagram of within-school model, twelfth grade, for the determination of verbal achievement and curriculum placement. (See text for definitions of variables.)

class students in curriculum placement, one would expect a sizable unique effect of background on placement when verbal ability is controlled. Such an effect is not present. Social class accounts for 9.9% of the variance in placement within schools, which testifies to the class segregation existing. However, most of the effect operates through differences in tested ability rather than independently. The small bias present in favor of advantaged students is significant; yet it does not seem sufficiently large to argue that considerable social class discrimination is prevalent in selection mechanisms within schools. This conclusion is at odds with several polemics on the subject of tracking (cf. Schafer, Olexa, and Polk 1970; Howe and Lauter 1970).

As with most analytic models, large numbers of interesting and relevant variables have been omitted. The total within-school variance explained in curriculum placement is only 27.5%. It is possible, indeed likely, that the addition of other factors, such as teachers' recommendations and pupil motivation or ambitions, would affect the magnitude of the coefficients observed. In order to argue that social class background per se is understated, however, one must demonstrate that other socioeconomic indicators relatively orthogonal to both verbal achievement and the background factors measured here have a sizable influence on curriculum placement. While this is possible, it does not seem likely.²

²The model presented pertains only to twelfth-grade pupils. It is clear that a large number of pupils drop out of school before reaching the twelfth grade and that the model may be biased by this fact. The identical within-school equations were computed for ninth-grade students in the same schools in an effort to control such a bias. The data for ninth graders were considerably more problematic, since 31.8%

Selection and Stratification in Schools

The relationships between background variables and events occurring in schools can be ordered unambiguously by temporal priority. For a variety of school measures, however, causality can be debated. Figure 2

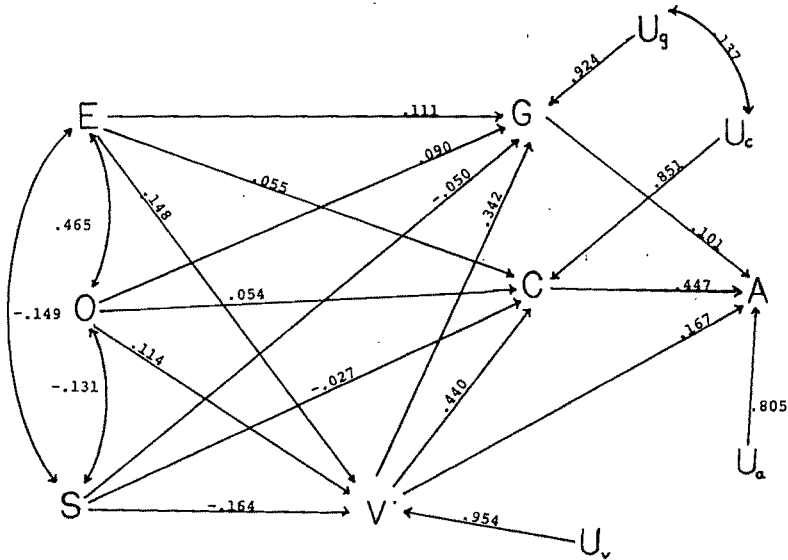


FIG. 2.—Path diagram of within-school model for the determination of verbal achievement, grades, curriculum placement, and aspirations. (See text for definitions of variables.)

includes the within-school prediction of grades and aspirations in the model of stratification within schools. Grades represent the self-reported averages over four years of high school; I have no measure of the reliability of these reports. Grades are cumulative temporally, while curriculum placement presumably occurs at one point in time. The causal relationship between curriculum and grades is ambiguous, because grades may be either a criterion for assignment or a reflection of preferential placement. Since college preparatory students typically take different courses, with either more lenient or more rigorous grading standards, it is difficult to argue the direction of effect with assurance. The within-

of the freshmen surveyed did not respond to the question regarding curriculum placement, presumably because they had not yet been assigned a curriculum. For those who did respond, the relative magnitude of socioeconomic background and verbal achievement was essentially the same as the effects observed in the twelfth grade. The aspirations of ninth-grade students were lower, on the average, than those of twelfth graders. While it is clear that attrition represents a source of negative selection, there is no evidence that assignment per se influences dropping out. Ninth-grade equations predict curriculum placement less well, which may be due to greater unreliability in responses; however, the pattern of effects leads one to identical conclusions regarding the processes and the impact of socioeconomic background.

school zero-order correlation is positive and quite strong ($r = .31$), suggesting that college preparatory students earn higher grades throughout high school. The model, however, depicts background and verbal achievement as predicting both grades and curriculum assignment independently.

The estimates obtained for the within-school determination of grades reveal a pattern quite similar to curriculum placement. Table 3 decomposes

TABLE 3
UNIQUE AND JOINT EFFECTS OF SOCIAL CLASS BACKGROUND ON VERBAL
ACHIEVEMENT, CURRICULUM PLACEMENT, AND GRADES WITHIN
SCHOOLS (PROPORTIONS OF VARIANCE EXPLAINED)

Dependent Variable	Unique Effects of Verbal Ability	Unique Effects of Social Class	Joint Effects
Verbal	9.03%	...
Curriculum	17.60%	3.20%	6.73%
Grades	10.68%	0.92%	3.10%

the variance into unique and joint effects in the prediction of verbal ability, grades, and curriculum placement.

To the degree that the model is accurately specified, verbal achievement is a considerably more important predictor of grades than socioeconomic background. The within-school outcomes when one uses test scores as the criteria of differential ability seem largely meritocratic. Ascriptive characteristics operate indirectly through differential tested ability.

The students' educational aspirations are also problematic with respect to causality. Although aspirations are measured at the same point in time, no doubt many students formulate aspirations prior to the twelfth grade. The logic of interpreting aspirations as the outcome of schooling and family influences is twofold. First, we have no evidence on when aspirations were formed or how they may have changed prior to measurement. Second, aspirations are interpreted as the student's best guess of how far he will go educationally, based on cumulative experiences to date.

The determinants of aspirations portrayed in figure 2 include verbal achievement, grades, and curriculum placement. The models that included paths between aspirations and social class background were deleted because they provided little additional explanatory power. Most of the effects of social class background operate indirectly through the relationship to verbal scores, grades, and placement. Direct effects, although significant, were in no case greater than .09. Entering the socioeconomic background factors into the equation increased the coefficient of determination less than .015.

Selection and Stratification in Schools

The interpretation of such results is straightforward. First, the importance of schooling as the mediating factor between family background and aspirations seems unequivocal. Such trivial unique effects of socioeconomic background on aspirations have been previously reported in the literature. Sewell and Shah (1968b) found that socioeconomic background contributed less than 6% in the prediction of college plans when intelligence and parental encouragement were controlled in a sample of high school seniors in Wisconsin. In the present example, we can predict aspirations almost as well with three measures of social processes within schools as we can if we also know father's education, occupational attainment, and size of family.

The magnitude of the direct effect of curriculum placement on aspirations should be noted. The zero-order relationship within schools was .561, accounting for 31% of the variance. Although I have presented the model of stratification within schools assuming aspirations do not influence placement, it is clear that to some degree schools respond to differential aspirations and serve to implement them. In the present sample, 84.1% of the students report they are enrolled in the curricula of their choice. While this probably overstates the degree of choice present, assignment and aspirations are, to some extent, reciprocally determined.

Several studies have suggested that aspirations are more important than verbal achievement in predicting college attendance (Sewell and Shah 1967; Flanagan et al. 1966, 1971). It is possible that schools play a more decisive role in the stratification system through encouraging and implementing aspirations than through altering patterns of achievement (Sewell and Armer 1966; Nelson 1972).

CURRICULUM ASSIGNMENT AND COUNSELING

With the growth in size and diversity of educational institutions, the role of the counselor has developed, often with autonomous decision-making powers (Armor 1969). School counselors serve, as Cicourel and Kitsuse (1963) put it, as "validating agents" and both evaluate and help plan vocational choices and careers. Cicourel and Kitsuse argue that the counselor largely determines curriculum assignment, even though students in principle have options and choices. For many students, decisions about college applications, scholarship competitions, and vocational programs are based solely on information provided by counselors. While counselors are nominally expected to advise students, the professionalization of the guidance counselor has often involved, as well, interpreting standardized tests, evaluating students, writing letters of recommendation, and imposing disciplinary sanctions.

Counseling is viewed here as a service to students and a logical

mechanism through which selection and placement occur. I will concentrate on only two aspects of students' relationships to counselors: the self-reported number of visits to counselors and the amount of positive perceived encouragement to continue educational training. Counseling is, in one sense, a resource which schools provide to students; the allocation of a counselor's time and the amount of encouragement offered are in some sense a measure of the school's investment in the student's future. If the counselor differentially allocated his time or encouragement to students based on the social class or race of the student, one would argue that the counseling process within the school was discriminatory to some degree. Since counseling is linked to the selection process, one would expect the counselor to encourage more talented pupils or perhaps discourage students who do not have the family resources to finance an expensive education. The present analysis concentrates on the determinants of access to counselors and on perceived encouragement. The focus will be twofold: on counseling as a mechanism for selection and on counseling as a school resource differentially allocated to students. The two perspectives are not unrelated, and both have implications for assignment to a college preparatory curriculum.

The relationships to be examined are between curriculum placement, grades, verbal ability, social class background, and counseling. Ordering the variables in a causal sequence presents some difficulty, since the relationships between grades, ability, placement, and counseling are not temporally distinct. The tactic proposed is to assess the relative importance of background variables and verbal ability in predicting encouragement and number of visits to the counselor and then enter the intervening variables, such as grades and curriculum. The logic is that if the social class background variables are not directly related to access or encouragement, the effects of differences in placement, or grades, should reduce the relationships substantially. If the background variables persist, then we can conclude that counseling is differentially allocated within schools on the basis of social class. Second, we shall examine the relative magnitude of the determinants of both accessibility and encouragement. If the student's curriculum placement is strongly related either to frequency of contact with counselor or to amount of encouragement received when ability and grades are controlled, the conclusion seems tenable that curriculum placement is a mechanism of differential allocation of school services, or at least of the school resource of counseling and advice. The model to be presented is illustrative rather than literally causal, since the variables cannot be unambiguously ordered. The path diagram is presented to clarify the strategy and direction of the present analysis when assessing the determinants of counseling. The paths of prime con-

cern are those linking socioeconomic status and counseling through curriculum placement.

The relationship between the number of times an individual saw a guidance counselor during the preceding year and the respondent's social class background accounts for less than 1% of the variance within schools. When curriculum placement, grades, and verbal ability are controlled, the net increment to within-school sums of squares explained by background is less than 0.3%. The effects of curriculum assignment, verbal achievement, and grades on access to a counselor are generally larger. The total variance explained within schools is only slightly less than 2.0%, while the differences between schools in number of times a student saw a counselor account for approximately six times as much of the variance as measured variables within schools. The focus is on the relative explanatory power of social class, however, and not the total proportion of the variance attributed to it. Curriculum assignment, accounting for 0.8% of the variance, is the single largest predictor of the number of times a student saw a counselor. While it is difficult to argue that the model begins to account for the observed variance among students, the conclusion that socioeconomic background as measured here is not a strong predictor seems tenable. The strength of the association between curriculum assignment and number of visits would seem to indicate that the counselor's time is differentially allocated between curricula, independent of grades or ability level.

The amount of perceived encouragement a student receives to continue his education beyond high school is the second major counseling variable to be considered. Again, the objective was to assess the relative importance of social class background in predicting perceived encouragement, both with and without controls for school variables. Social class accounts for slightly less than 5% of the variance within schools in perceived encouragement; the unique effects of social class when ability is controlled account for less than 2% of the variance. The combined effects of social class add less than 0.3% to the within-school variance explained when ability level, grades, and curriculum assignment are all entered into the equation. The effects of social class, although persistently significant, seem quite small when intervening variables are considered. For this reason, the direct effects of social class were excluded from the model depicting the determination of encouragement and number of visits to the guidance counselor. The model presented in figure 3 summarizes the relationships.

The net impression is that social class background is mediated within schools primarily by curriculum and verbal ability and does not exert a strong direct influence on either talks or encouragement. To the extent

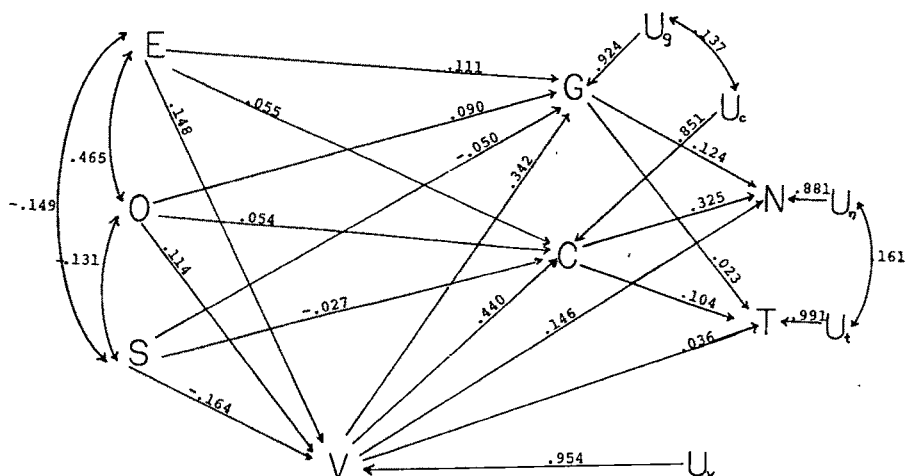


FIG. 3.—Path diagram of within-school model for the determination of encouragement (N) and visits to counselor (T) by background variables and school experiences for twelfth-grade students. (See text for variable descriptions.)

that counseling represents a service or resource allocated within schools, the student's curriculum placement is a more important determinant of the differential allocation than either grades or ability level. Curriculum assignment appears, therefore, to be an institutional mechanism within schools for the selective distribution of encouragement and counseling, and perhaps other resources as well.

There is a considerable literature attempting to relate the allocation of school resources to differential achievement by social class (Coleman 1966; Bowles and Levin 1968*a*, 1958*b*; Mosteller and Moynihan 1972; Jencks et al. 1972). The results are generally negative, in part because the variance in achievement is largely within schools, and the measures of school resources and facilities are presumed to affect school means, not individual children. Such disparate items as a science laboratory, a school library, or access to bright, highly motivated peers have on occasion been called "resources," and are presumed to affect achievement. What seems irrefutable is that such resources are differentially available within schools, as well as between them, and that they cannot affect all students equally. If such resources influence student achievement, equations utilizing only the variance between schools necessarily underestimate their explanatory power. For most school resources it is difficult to ascertain who actually uses or benefits from them. The variables I have chosen to evaluate the distribution of resources are counseling access and encouragement. Although less than ideal, the analysis suggests that differences within schools in curriculum placement strongly influence the allocation and usage of school services, such as counseling. Logical arguments for dif-

ferential access to other school resources depending on curriculum seem quite plausible. Whether resource allocation or differential usage directly influence achievement within schools is still largely speculative, since the causal ordering of such variables remains ambiguous; the point to be made is that they might.

DISCUSSION AND CONCLUSIONS

The purposes of this study were to develop and explore empirically the implications of academic stratification within schools. Curriculum placement was presented as an important institutionalized mechanism for social selection and channeling as well as allocation of rewards. The determinants of placement allow an assessment of the selection process operating within schools and a preliminary effort to determine how institutional decisions affect student attainment.

The well-documented persistence of ascriptive characteristics in determining student outcomes has often been cited as *prima facie* evidence for educational discrimination. The critique of schools is frequently argued from precisely this premise; that is, schools have failed to equalize life chances for the children of the poor. As Clark (1968) argues, "American public schools have become instruments in the blocking of economic mobility and in the intensification of class distinctions." The persistence of social class differentials is a necessary but not sufficient cause to argue that schools are in part responsible for perpetuating socioeconomic inequality. A more pertinent test of the proposition that discrimination is a source of differential attainment, I have argued, is an examination of the selection processes operating within schools. The contention that class bias operates independently of differential achievement cannot be supported by the present research. The principal determinant of curriculum placement and grades is verbal achievement test score, not father's occupation, education, or family size. Although background is related to placement, it is almost entirely mediated by differential achievement. The first conclusion is that educational stratification largely results from differential performance on achievement tests. The socioeconomic composition of curricula would scarcely change if students were selected solely on the basis of achievement tests. Perfecting the meritocracy, therefore, will not alter the degree of socioeconomic inequality within the sample of urban public high schools studied. It follows that, in order to alter outcomes, questioning the criteria of placement is more relevant than focusing on the process.

Second, curriculum assignment differentiates and labels students academically. To some degree, educational tracking or any system of stratification ensures segregation and differential access to school resources and personnel. Equality of educational opportunity is clearly related to such

issues. The inherent paradox is that American public schools are charged with the task of social selection and channeling while they are simultaneously expected to fulfill the democratic mandate of equal opportunity. If one is persuaded that differentiation generated by achievement tests is meritocratic and more equitable than other sources of inequality, one would conclude that social selection within high schools is operating adequately. It seems equally important, however, to question both the function and the degree of differentiation.

Although conclusive evidence is clearly lacking, institutionalized mechanisms for selection may reinforce achievement differentials and shape student aspirations over time. An assessment of the process and determinants of stratification within schools seems a fruitful approach to studying the linkage between educational institutions and status attainment in American society.

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Commentary and Debate

ON "MILITARY SPENDING AND ECONOMIC STAGNATION"

The Baran-Sweezy theory of "monopoly capitalism" may not be "correct," but Szymanski's "tests" (1973) of propositions putatively derived from their theory are, in my view, essentially irrelevant. Whether or not Szymanski's "findings" were consistent with his interpretation of their theory (a theory to which I myself have serious objections), these could neither confirm nor disconfirm their theory of monopoly capitalism. I do not deny that certain propositions may be derived from the Baran-Sweezy theory which can and should be tested by the most sophisticated methods of quantitative analysis; Baran and Sweezy themselves urge such work based on their theory (1966, p. 1) and present 22 quantitative tables (all but one of which are time-series data) as one type of evidence in support of it. Their theory is simultaneously abstract and historical (sociological) and requires social and multivariate analysis of a multiplicity of relationships and factors, of which "military spending" is but one, highly limited, specified, variable.

Their theory of monopoly capitalism, stated, as Schumpeter might say, with "desperate brevity," is that a capitalist system dominated by large corporations tends to produce a rising "economic surplus" (investment funds or "savings") in excess of profitable private investment outlets. This disparity between potential investment funds and available investment outlets, on a continuing basis, poses a problem ("contradiction") for the system, for it tends, therefore, toward chronic stagnation: the relative absence of sufficient "'effective demand' (to use the Keynesian term) to ensure full utilization of either labor or productive facilities" (Baran and Sweezy 1966, p. 143). Among their many indicators of this tendency, Baran and Sweezy utilized time-series data on changes in the level of national income; employment; labor-force participation; unemployment plus military-related employment; growth of "excess capacity" (or the utilization of productive capacity); the duration, depth, and distance between business cycles; and profits as a share of national income.

There is a tendency toward stagnation, they argue, which must be offset by counteracting tendencies or the system would not function—it would fall into prolonged depression and crisis. The counteracting tendencies (alternative modes of surplus absorption) are interacting tendencies having different weights and consequences depending on their periodic interrelationship and on the specific historical and social context in which they operate: the international competitive footing of other capitalist

countries and their relative level and phase of development, particularly economic concentration ("monopoly"); peacetime, war, its aftermath; the correlation of class and political forces in the country; the specific options open because of the international situation; and so on. Theirs is *not*, as Szymanski claims, a theory that is "purely economic" (1973, p. 14). Indeed, much of their theory of counteracting tendencies, or alternative theoretically possible modes of surplus utilization and their rejection of them as likely occurrences, is based on reasoning concerning the material and ideal interests of different interest groups, social classes, and class segments of our society. Thus, when Szymanski concludes that whether or not "welfare" alternatives can displace military-related activities as a major counteracting tendency depends on "the present constellation of forces in American society" (1973, p. 14), he is merely reiterating the very argument of Baran and Sweezy. They do not deny its theoretical possibility but its practical probability. (Another Marxist, who outlined a similar theory even earlier than Baran and Sweezy, namely, Joseph Gillman [1958, 1965, esp. chap. 13], argued that to effectively absorb "savings," the "welfare state" would have to be built from taxes on wealth rather than on popular consumption. This would entail a struggle against the wealthy; the latter prefer expenditures on war and preparation for war because, Gillman argues, it gives them profits, protects their investments, and expands domestic and foreign markets. Expenditures for public benefit, however, may accustom the people to such prerogatives as employment and general well-being, thereby restricting the sphere of private capital when and where it conflicts with the public interest. This, as I read it, is also the argument of Baran and Sweezy [cf. also Sharpe 1966].)

The most central proposition in Baran and Sweezy's theory has to do with the alleged stagnation tendencies inherent in a system of monopoly capitalism over time. Neither "monopoly" (industrial or market structure; aggregate economic concentration) nor time-series data to indicate trends or time-order relationships appear in Szymanski's article. Baran and Sweezy focus on (1) methods by which output is restricted, and (2) and most important (in their theory), methods by which demand is stimulated. And these are discussed within the specific sociohistorical context of American capitalism over the past several decades. Szymanski asks no historical questions; there is no sociological reasoning in his article. The Baran-Sweezy theory requires such questions and answers. For example, is it correct to ask what proportion military expenditures are in countries of differing sized GNPs, without taking into account whether military-related expenditures were a practical alternative mode of surplus absorption, or even a necessary one, given the country's historical situation? Can Japan and Germany, defeated and occupied countries, the former having been deliberately prohibited, the latter encouraged by the occupying powers to

rearm—and both of which have been spurred by postwar reconstruction which has provided vast investment outlets—be simply compared on, and inferences made from, a bivariate contingency table? Can Israel, in conflict with neighboring Arab states, be compared with Canada, on the probability of large military expenditures? Were Baran alive, and Sweezy inclined, I am certain they would reject the idea as absurd. Can the United States, which became, by default, the dominant military power (and “policeman”) in the capitalist world after World War II, be correctly compared with Canada, with its open space and American ownership of much of its industry, or Australia, with increasingly dominant foreign ownership, without taking such historical and social structural contingencies into consideration in one’s analysis?

Among the most important alternative modes of surplus utilization, or counteracting tendencies to stagnation, in the Baran-Sweezy model, that is, means by which markets are created and expanded, are: (1) war and the preparation for war, and particularly its aftermath (when the backlog of pent-up demand and the need for new types of plants and other industrial changes create new and interacting private investment outlets); (2) “the sales effort,” or the interpenetration of the production and sales structure—of advertising expenses, public relations, design and creation of built-in obsolescence, frequent style changes, and product differentiation; (3) the system of mortgages and consumer credit (installment buying, credit cards, financing); (4) the consequent proliferation of hosts of “*unproductive* workers” engaged in what may or may not be actual production (Though their activities may not, from an economic standpoint, result in the creation of *value*, they may be *necessary*, under capitalism, to its creation: those engineers, technicians, and designers, whose work is spent on creating short-lived products or appealing to new consumer wants and needs stimulated by other aspects of the sales effort; the gamut of functionaries of capital, other than the owners of capital themselves: lawyers, stockbrokers, bankers, public and private bureaucrats, who live *from*, rather than create, the surplus.); (5) government expenditures, both for direct purchase of goods and services and as transfer payments (including welfare payments, unemployment compensation, farm and business subsidies), of which military expenditures may be (as in the United States) a decisive component; and (6) “epoch-making innovations” (clearly a notion derived from Sweezy’s teacher, Joseph Schumpeter), such as the automobile and railroad in the past, which spur economic development over a long period of time through direct and multiplier effects.

Clearly, therefore, Szymanski is patently wrong when he says that Baran and Sweezy assert that “*only military spending* can alleviate stagnation under monopoly capitalist conditions” (1973, p. 12; my italics). They assert no such thing, though of course under specific conditions,

and depending on what sorts of expenditures, military spending will be significant. Indeed, Baran and Sweezy specifically reject the proposition that military spending *must* reduce unemployment. In past world wars, the preparation for war involved mass-produced military hardware and the conscription of millions. However, they state, "There has been a sharp shift in the character of goods and services purchased by military outlays. . . . This change in the composition of military spending means that a given amount of military spending employs far fewer persons than it used to. In these circumstances, even very large increases in military spending, while *enormously profitable to the big corporations*, may have relatively little effect on investment and employment. . . . The huge military outlays of today may even be contributing substantially to an *increase of unemployment*; many of the new technologies which are by-products of military research and development are also applicable to civilian production, where they are quite likely to have the effect of raising productivity and *reducing the demand for labor*" (1966, pp. 214-15; my italics).

Further, Szymanski refers to Melman's view (1965) that, in Szymanski's words, "military spending results in great misuse of resources," and hinders economic growth; and he concludes that "Melman's theory would seem to explain the data better than that of Baran and Sweezy" (1973, p. 10). Yet the very focus of the theory of "monopoly capital" is on the way in which "misuse of resources" or "waste" is inherent in and functional to the system. It is, in fact, through the misuse of resources or the "diversion of a vast volume of resources" (Baran and Sweezy 1966, p. 139) into *unproductive* activities necessary to provide profitable investment outlets, rather than their socially rational use, that militarism, the sales effort, financial interest, and other modes of surplus absorption assist "monopoly capitalism" to function. "Waste" is a principal conceptual component of Baran and Sweezy's central concept of "economic surplus." Their focus is precisely on what they say "modern economics . . . reject[s] as unscientific," namely, the "distinction between useful and useless output, between productive and unproductive labor, between socially necessary costs and surplus. Modern economics . . . wants no confrontation of reality with reason" (p. 134; also see the appendix on "estimating the surplus").

In their book, Baran and Sweezy often assume too much and explain too little; their style of discourse is discursive (sometimes prolix), often expansive, and occasionally hyperbolic. They took insufficient care with precise, or at least clearly articulated, definitions of concepts. However, serious study of their book reveals a coherent (if not elegant), systematic theory—which I have tried to outline here elliptically and schematically. Szymanski is to be commended, as are the editors of *AJS*, for devotion to serious issues and his attempt to analyze them empirically. However, such

devotion must be coupled, as I know he must agree in principle, with great care that one's methods are adequate to one's problems; and that one does not, unwittingly, contribute either to the misunderstanding of a complex and original theory (with which many *AJS* readers are doubtless unfamiliar) or to the creation of what Robert K. Merton (1959) has termed "pseudofacts."

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A DEFENSE OF BARAN AND SWEETZY

In his recent article in the *AJS*, Szymanski (1973) has taken issue with one of the central ideas of Baran and Sweezy's *Monopoly Capital* (1966), namely, that military spending in monopoly capitalist economies has eased the unemployment situation in these economies and has promoted the prosperity they have exhibited, especially in the United States. In his research, Szymanski found only limited support for the "easing of unemployment" and contradictory evidence regarding economic prosperity. He examined the economic performance of the industrialized capitalist nations since World War II and concluded that "military spending, instead of leading to economic growth, actually appears to be a cause of at least relative stagnation" (1973, p. 6). Before reform-minded individuals too readily accept the Szymanski argument, let me raise some questions which I believe are of importance and which may rather drastically undermine Szymanski's conclusions and the methods he used to arrive at them.

First, a minor point: Szymanski is not quite accurate when he states that Baran and Sweezy argue that military spending is the only mode of spending available to federal governments under capitalism. Baran and Sweezy, in fact, point to other areas of spending, such as highway and freeway construction. Military spending is simply regarded, however, as the best area of federal expenditure given the profit priorities of monopoly capitalism.¹

Second, Szymanski's use of only the 1968 military spending figures when relating military spending to the economic growth variable is questionable, since the latter data cover the 1950-68 and 1960-68 periods. Why did he not use all of the 1950-68 military spending figures? It is important to note here as well that Szymanski's correlations are initially quite low and are interpreted as military spending *causing* low economic growth. Without a causal model, it is just as permissible to give an opposite interpretation to the inverse relationship—that low economic growth (economic stagnation) leads to a nation turning to military spending to buoy up its economy.

Third, for a radical sociologist, Szymanski's approach is rather undialectical. He seems to assume that each of the industrialized capitalist nations in question is at the very same point in their economic history, that is, that they have begun to face up to surplus absorption problems at the same time. He completely ignores the Marxist law of uneven development, which states that countries and regions are blessed with different natural resources, geographies, institutions, and histories and, therefore, have proceeded at varying rates and paces. These differences, or unevenness, in their development cannot be safely ignored, particularly when the historical period in question is rather short; in the present case the period is but two decades. It is important to remember, then, that the American economy came through World War II unscathed, whereas Western Europe, and to a certain extent Japan, had rather heavy reconstruction to undertake. Such reconstruction requires large capital outlays for heavy-duty goods and plants which subsequently make a major contribution to economic growth. Thus, one could only expect such war-torn economies to exhibit higher growth rates than did the United States or Canada.

Fourth, the above discussion has touched on what seems to be the most serious econometric weakness of Szymanski's article. He relies on zero-

¹ For some rather convincing discussions concerning modern industry's primary concern with profit maximization, see Baran and Sweezy (1966, pp. 20-28) and Tanzer (1972, pp. 7-14, 19-26). That the military business is a very profitable one has been shown in the works of Bohi (1973), Kaufman (1970), and Stevenson (1973), among others.

order correlations, thus ignoring the impact that other independent variables have on the economic growth rate. A fuller, and probably more accurate, examination would consider the following. First, economic prosperity in the United States may be assumed to have been dependent on military spending. Weisskopf (1972, p. 374) has presented empirical evidence which "reveals a remarkable correlation between economic prosperity, government expenditures in general, and military expenditure in particular." Second, a prosperous American economy has been crucial to the economic prosperity of Western Europe and Japan, since the United States is a major importer of their goods. Third, large amounts of capital investment have been channeled by American business into Europe rather than into American domestic enterprises, thus furthering European economic growth and productivity and hindering their own growth and productivity in the United States. In addition, such investment has made these other capitalist countries into better competitors vis-à-vis American business (Tanzer 1972, pp. 167-87; Block and Hirschorn 1972, pp. 15-19).

Finally, Gurley (1972, pp. 12-13) and Kindleberger (1967) have traced the supergrowth of Western Europe to supersupplies of labor which have kept the wage rates low and the profits high, have stimulated capital spending as a result, and have advanced, therefore, the economic growth of these industrialized capitalist nations.

In brief, then, when Szymanski uses economic growth rates as an indicator of economic prosperity, he would do well to consider the numerous variables which affect such growth. The only such extensive study, to my knowledge, has been that of Kidron (1970). He uses a multivariable approach and concludes that military spending has been vital for the economic well-being of the Western capitalist countries since World War II. A study that uses econometric models and goes beyond Kidron would be of much help to social analysts and would fill a major void in the literature.

Although I have attempted to raise some rather serious questions about Szymanski's research, I think that history itself will raise these questions even more sharply. What we see now in the monopoly capitalist countries is a growing monetary crisis partially rooted in the worsening of America's balance of payments, increasing competition among these countries, and economic stagnation throughout their economies. I believe that these countries will increase their military spending in order to buoy up their economies and to develop their military forces in order to protect their foreign economic interests. In my view, such consequences will fully validate the Baran and Sweezy thesis.

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AN ALTERNATIVE MODEL TO SZYMANSKI'S

What is at issue in Szymanski's (1973) paper? Basically, whether current attempts by liberal and radical forces to decrease arms spending risk or encourage economic stagnation, with all the social turmoil this may imply. Also, it is valuable as an attempt to test Marxist theories of worldwide social dynamics. Hence, it is an extremely important article, and a full discussion of its assumptions and errors is in order.

The article makes two dubious assumptions—that nations are autonomous and that the economy is a distinct subsystem. Let us look at each of these in turn.

1. *Nations are autonomous.*—Szymanski ignores the work of such scholars as Kidron (1970) who present analyses of the arms economy based on an international economy. The use of a national autonomy model seems particularly hard to defend in view of the evidence for there being an international investment system as shown by the emerging debate on multinational corporations, or by the statistics of international direct investment. Thus, the estimated foreign production due to direct investment by the countries Szymanski studies amounts to approximately \$300 billion in 1971—which was considerably larger than the GNP of Japan (United Nations 1973, p. 159). Furthermore, the world scope of the current series of financial crises, manifested in threatened currency inconvertibility, protectionism, and international monetary conferences, and

the worldwide effects of the Russian-American wheat deal should indicate that an analysis based on the assumption of economic autonomy can be misleading. Unfortunately, Szymanski presents us with no reason to believe that this assumption is appropriate for his problem.

2. *The economy is a distinct subsystem.*—Thus, Szymanski ignores questions of differences in the political power of the reformist working-class parties in the countries he studies. This leads him into a serious mistake in his discussion of “nonmilitary government spending.” Upon finding that nonmilitary government spending in 1968 is associated with low unemployment (1964–68) and high growth in GNP per capita (over both the 1950–68 and 1960–68 periods), he assumes that nonmilitary spending prevents economic stagnation.¹ However, an alternative explanation, that in countries with relatively buoyant economies labor parties will be more successful in their demands for various nonmilitary expenditures, is equally appealing. Thus, when the assumption that there is an analytic wall between economy and society is relaxed, we find that Szymanski’s crucial result about the effects of nonmilitary spending is quite unconvincing.

Szymanski also fails to discuss the economic effects of international power relations (which are heavily affected by arms expenditures). There is a classical Marxist analysis of imperialism which holds that the tendency to stagnation is offset by investment in less developed countries and that this investment is made possible, in part, by the maintenance of powerful armed forces. Yet Szymanski, writing in a Marxist tradition, ignores the possible effects of the analysis of imperialism on his findings. He almost seems to think that the economic development of Taiwan has not been affected by the political implications of the Seventh Fleet, but only by its purchases. This is clearly due to his assumption of an analytic wall between politics and economics, and shows the dangers of such assumptions.

Szymanski also makes two errors which are not traceable to his assumptions about system boundaries. First, he claims that “military spending in the absence of nonmilitary spending does have a major impact on reducing stagnation” (p. 12). Yet table 3C shows us that in countries with low nonmilitary spending, those with high military expenditures are more stagnant than those with low military expenditures. They average 3.0% unemployment as against 2.2%, and their GNPs per capita grew at an average rate of only 2.4% as against 4.5% in 1950–68, and of 3.3% as against 4.3% in 1960–68. Second, Szymanski’s view of the economy may have very tight boundaries, but they include very little area. He ignores countless variables which have been held to influence growth rates and which are economic in any perspective. Thus, differences in industrial

¹ His earlier finding about the stability of military-spending ranking from year to year mitigates the error in stating that later events caused earlier ones.

base, or in investment rates of earned profits, or of economic concentration (to mention but a few), might well be more powerful independent variables than arms spending, or might be crucial intervening variables.² The point is that Szymanski ignores these variables, and we thus do not know their effects.

I have now cast doubt on the author's assumptions, showed he made an error in interpreting his tables, and pointed to variables he omits. This may greatly weaken his case, but it does not in itself show that his conclusions are wrong. Given the importance of the questions Szymanski addresses, I want to conclude by offering an alternative model and by suggesting that research as to whether one of these models is correct will greatly increase our understanding of world socioeconomic dynamics.

The alternative model is that of an international system with long-run tendencies toward stagnation. Several offsetting forces have been developed, chief among them being investment in underdeveloped areas and arms expenditures. This then implies that a decrease in the international sum of arms budgets threatens to bring about economic stagnation. However, arms expenditures are determined by national governments, so we are faced with international stability being dependent on a number of national decisions. Yet, as the work of Melman (1965) implies, the long-run growth of national productive technology is spurred by concentrating investment and research in civilian rather than military efforts. Thus, the system has a basic instability: each nation is motivated to decrease its arms expenditure in a process of competitive disarmament (limited only by the military needs for protecting foreign investments, past and future, and the homeland—and these needs decrease as other nations' expenditures decrease), which then tends to destabilize the system as a whole.

This model is a very hard one to test, and this is not the place to test it. As a piece of evidence that is worth considering for future research, note that it is quite able to deal with the case of Japan. For Szymanski, Japan's high growth rate during a period in which both its military and nonmilitary government budgets were low is puzzling. For the model presented here, it is an immediate corollary that any country which was able to devote its technical and investment resources to the productive spheres of the economy during a period when the other major economic powers were putting theirs into relatively nonproductive military budgets will grow disproportionately fast. Thus, since the stability of the system was guaranteed by other nations' arms expenditures, Japan or any other country politically able to opt out of the arms races of the world could use this stable world economy as an environment in which to maximize its growth rate and to decrease its unemployment without itself having to

² They also may mask the effects of arms budgets. Thus, Szymanski's case might have been strengthened by consideration of these variables.

use government spending as a stimulus. And, of course, such a country will appear anomalous to any theory based on national economies as the basic systems to be studied.

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A REPLY TO FRIEDMAN, STEVENSON, AND ZEITLIN

I am indebted to Professors Friedman (1974), Stevenson (1974), and Zeitlin (1974) for their criticisms of my article on the effects of military spending and economic stagnation and for giving me a chance to draw out the implications of my original argument. I wish to divide my response into two parts. First, I will attempt to answer a number of the specific objections presented to my argument; and second, I would like to deal with what I detect to be the fundamental thrust of their arguments—the question of the primary contradictions of American capitalism.

All three critics suggest that I argue that Baran and Sweezy maintain that *only* military spending can absorb the economic surplus. I argue no such thing. They are of course quite right in pointing out the other mechanisms of surplus absorption listed in *Monopoly Capital*. What I did maintain was that Baran and Sweezy argue that military spending has been the primary cause of the prosperity of the U.S. economy since the end of the Great Depression. The principal argument of chapter 6 of their book, as well as of much of chapter 7, is that only military spending has in fact prevented economic stagnation. It is to the test of this proposition that my paper was directed. Baran and Sweezy argue again and again that it is military spending that has made the difference: "The difference between the deep stagnation of the 1930's and the relative prosperity of the 1950's is fully accounted for by the vast military outlays of the 50's. . . . The percentage of the labor force either unemployed or dependent on military spending was much the same in 1961 as in 1939. From which it follows that if the military budget were reduced to 1939 proportions, unemployment would also revert to 1939 proportions" (1966, p. 176).

According to Baran and Sweezy, because of the structure of vested interests in American society civilian spending cannot play such a role. Indeed it is to this proposition that chapter 6 of their book is dedicated:

It would be possible to run through the gamut of civilian spending objects and show how in case after case the private interests of the oligarchy stand in stark opposition to the satisfaction of social needs. Real competition with private enterprise cannot be tolerated, no matter how incompetent and inadequate its performance may be: undermining of class privileges or of the stability of the class structure must be resisted at any cost. And almost all types of civilian spending involve one or both of these threats. There is just one major exception to this generalization in the United States today and it is very much the type of exception which proves the rule: government spending on highways. [P. 173]

Clearly, Baran and Sweezy argue that the one difference between prosperity and depression in the postwar era has necessarily been the level of military spending. It is also true, as Zeitlin points out, that Baran and Sweezy argue further that it is not likely that military spending will indefinitely be as successful as it has been over the last generation in absorbing the surplus. There are, they argue, limits to the effectiveness of such spending, limits determined both by the changing nature of military hardware and the growing opposition to increasing military expenditures. Such limits to the role of military spending in surplus absorption can, according to the argument of *Monopoly Capital*, very possibly lead the system into economic stagnation and a return to the conditions of the Great Depression. It is highly unlikely according to Baran and Sweezy that, given the constellation of power in monopoly capitalist society, any adequate substitute for expanding military spending can be found within the parameters of capitalism. Zeitlin confuses this argument concerning the limits of military spending with the analysis of the actual postwar role of military spending. The two arguments must be kept separate.

Friedman, Stevenson, and Zeitlin all challenge the use of the comparative method to test the Baran-Sweezy hypothesis. Stevenson and Zeitlin argue that the unique qualities of one or another of the advanced capitalist countries means that these countries cannot validly be compared. Their argument here would seem to reduce to the position that because of differences among the advanced capitalist countries they cannot be compared with one another—hardly a defensible position. The logic of comparative analysis is based on the very reasonable assumption that, when the proper controls are made to exclude the effect of extraneous variables, the variance of a dependent variable is explained by the variance in the independent variable. If someone claims that the effect on the dependent variable is really due to a factor other than the one initially claimed, a proper test of the original hypothesis must include a control for the other factor to see if the original effect is still as strong as before. Rather than

rejecting the comparative method because a third variable is suggested, the third variable should be controlled for.

Friedman argues that nation-states in the capitalist world are not autonomous. Trade, investment flows, money flows, etc., among capitalist countries make them in many ways one unified system. However, in many basic ways capitalist countries can be considered to be autonomous enough to serve as units for comparative analysis. Whether or not we must consider the system as a whole or its constituent nation-states to be the unit of analysis depends on a concrete examination of the variables under consideration. Further, the factors which relate the various units of the capitalist system to one another can be controlled for as third variables in any comparative analysis where it is claimed that they have a major effect on the outcome. The existence of trade or foreign investment by no means implies that we must reject the comparative method. For example, if it is argued that foreign investment in Third World countries provides outlets for the surplus thereby allowing military spending to be reduced without the consequence of stagnation, we can control for level of foreign investment in Third World countries and then test for the relation between military spending and economic stagnation. Likewise, if it is argued that a surplus of exports over imports is a functional substitute for military spending, we can control for this factor. We can of course also control for the level of military spending and examine the relationship between trade surplus or foreign investment and economic stagnation, if what we are interested in is the causes of economic stagnation rather than the effect of military spending (the concern of my article).

To see if the various factors suggested by Friedman, Stevenson, and Zeitlin are in reality the cause of the results I found in my paper—that is, to see if the argument of Baran and Sweezy might not be correct after all—we are obliged to examine the relation between military spending and economic stagnation controlling for the social, structural, and historical factors suggested by my critics. The relationship must be examined holding constant “uneven development,” the deliberate prohibition against military spending, real hostility against a country, foreign domination, level of destruction in World War II, level of exports to the United States, the relative amount of U.S. investment in a country, foreign investment in underdeveloped countries, the presence of a strong labor party, etc.

It should be pointed out that I originally controlled for “uneven development” by holding GNP/capita constant. The relations with such a control were not reversed. When we control for deliberate prohibitions against military spending (i.e., when we exclude Japan and Germany) the relationships we initially found between military spending and economic stagnation were weakened, although confirmation still cannot be found for the Baran-Sweezy hypothesis. When Israel is excluded (controlling for

real external hostility) the relationship we initially found is strengthened. When foreign domination is held constant (removing Australia, Canada, and New Zealand) no significant difference is seen in the observed relationship between military spending and economic stagnation. When the countries without strong labor parties (the United States and Canada) are excluded again no difference is observed in the relationship between military spending and economic stagnation. Foreign investment in underdeveloped countries in all cases results in a greater repatriation of profits than new investment. Hence, in no case does foreign investment in the Third World in fact substitute for military spending as a source for absorbing the surplus (see Baran and Sweezy 1966, pp. 104-8).

To examine the effect of destruction during World War II we separated the countries which saw heavy fighting during the war (Austria, Belgium, France, Germany, Italy, Japan, and the Netherlands) as well as Israel from the others and examined the relationship between military spending and economic stagnation in each set. Israel was considered together with the countries with a high level of military destruction because, although it did not suffer destruction during the war, its exceptionally rapid population growth in the postwar period provided an equivalent nonmilitary surplus outlet. If Stevenson's objection is valid we should expect that, in the countries with a low level of destruction, military spending would be strongly related to economic growth, while in the countries with a high level of destruction (plus Israel) no such relationship may exist since alternative sources for surplus absorption are available. The results are presented in table 1.

It can be seen that the Baran-Sweezy hypothesis as measured by economic growth rates cannot be upheld even when the level of destruction is controlled for. However, there is a significant difference between the countries with high and low levels of destruction in the direction predicted by Baran and Sweezy. Military spending has an adverse effect on the countries with high levels of destruction but has no significant effect on the countries with low levels of destruction. It does appear that, where adequate production outlets for the surplus exist, military spending hinders the rate of growth, but where no other adequate outlets exist military spending does not have such an effect. But in neither case is there a positive relationship between military spending and the growth rate. In both sets of countries, however, military spending does have the predicted effect on the rate of unemployment.

When we divide the advanced capitalist countries into high and low halves according to the ratio of their exports to the United States to their GNPs in 1968 we obtain the results shown in table 2. If Stevenson is right we should expect to find that at least where exports are low the relationship predicted by Baran and Sweezy should hold—since according

TABLE 1

PERCENTAGE RELATIONSHIP BETWEEN MILITARY SPENDING AND ECONOMIC STAGNATION
CONTROLLING FOR LEVEL OF WORLD WAR II DESTRUCTION

	MILITARY EXPENDITURE GNP (1968)	UNEMPLOYMENT (1964-68)	GNP/CAPITA PER YEAR	
			1950-68	1960-68
Low Level of World War II Destruction				
Countries with a high level of military spending . . .	5.3	2.1	2.8	3.5
Countries with a low level of military spending . . .	2.2	3.3	2.9	3.1
All countries with a low level of destruction	3.8	2.6	2.8	3.3
High Level of World War II Destruction				
Countries with a high level of military spending . . .	6.0	1.2	4.4	3.9
Countries with a low level of military spending . . .	1.8	2.6	5.4	5.3
All countries with a high level of destruction	3.9	2.1	4.9	4.6

NOTE.—In obtaining the results shown in this table as well as in tables 2 and 3 the third variable was controlled for by dividing the countries into high and low parts by the third variable then examining the relations between military spending and economic stagnation in each part. See my original paper for a list of the countries compared.

SOURCES.—United Nations 1968a, 1968b.

to the Baran-Sweezy argument a high level of exports could be a functional substitute for military spending (providing exports exceed imports).

Once more the original results are confirmed. When controlling for the level of exports to the United States, military spending still reduces unemployment but does not increase the rate of growth. The level of exports to the United States thus cannot be considered to cause a spurious relationship between military spending and economic stagnation.

Finally, when we control for the level of U.S. direct investment in 1968 the results are seen in table 3. If our initial relationship is spurious we should expect to find that, in all the countries, but especially in those with a high level of U.S. investment, there should be a positive relationship between military spending and economic growth. The relationship should be stronger in countries with a high level of U.S. investments since the inflow of these funds should aggravate rather than alleviate the surplus absorption problem, thereby increasing the need for military spending to counterbalance it.

Again, although the results are somewhat contradictory, we see that the Baran-Sweezy hypothesis can only be consistently upheld when stagnation is measured by the rate of unemployment, not when measured by the rate of growth. In summary, no matter what the third variable that

TABLE 2

PERCENTAGE RELATIONSHIP BETWEEN MILITARY SPENDING AND ECONOMIC STAGNATION
CONTROLLING FOR LEVEL OF EXPORTS TO THE UNITED STATES

	MILITARY EXPENDITURE GNP (1968)	UNEMPLOYMENT (1964-68)	GNP/CAPITA PER YEAR	
			1950-68	1960-68
High Ratio of Exports/GNP 1968				
Countries with a high level of military spending	6.3	2.1	3.9	3.4
Countries with a low level of military spending	2.0	2.6	3.9	4.3
All countries with a high level of exports to the U.S.	4.1	2.3	3.9	3.9
Low Ratio of Exports/GNP 1968				
Countries with a high level of military spending	3.8	1.5	3.3	4.0
Countries with a low level of military spending	2.2	2.4	4.3	3.8
All countries with a low level of exports to the U.S.	3.0	2.0	3.8	3.9

SOURCES.—See table 1.

TABLE 3

PERCENTAGE RELATIONSHIP BETWEEN MILITARY SPENDING AND ECONOMIC STAGNATION
CONTROLLING FOR LEVEL OF U.S. DIRECT INVESTMENTS

	MILITARY EXPENDITURE GNP (1968)	UNEMPLOYMENT (1964-68)	GNP/CAPITA PER YEAR	
			1950-68	1960-68
High Ratio of U.S. Direct Investments/GNP 1968				
Countries with a high level of military spending	4.3	1.6	2.8	3.1
Countries with a low level of military spending	2.3	4.2	2.3	2.8
All countries with a high level of U.S. direct investments	3.3	2.2	2.6	2.9
Low Ratio of U.S. Direct Investments/GNP 1968				
Countries with a high level of military spending	3.8	1.5	3.6	4.2
Countries with a low level of military spending	1.8	2.6	5.7	5.8
All countries with a low level of U.S. direct investments	2.8	2.1	4.6	5.0

SOURCES.—See table 1; also *Statistical Abstract of the United States*, 1970.

we control for, our results come out the same. The Baran-Sweezy hypothesis *cannot* be confirmed.

It is not surprising that we found that controlling for many of the factors suggested by Friedman, Stevenson, and Zeitlin made no difference in our results. My critics sometimes seem to forget the Baran-Sweezy argument when they raise their objections. Baran and Sweezy argue that the cause of economic prosperity is the ability of military spending to absorb the economic surplus. It is irrelevant to this argument what the actual causes of military spending are. The only factors which logically could effect the argument are factors which affect the relationship between the level of surplus that needs absorption and the ability of the economy and the state to absorb that surplus. Consequently, such factors as the deliberate prohibition against military spending, the real hostility against a country, the strength of labor parties, and foreign domination per se are irrelevant, since they affect the level of military spending, not the relationship between military spending and economic stagnation.

One factor raised by Stevenson, the level of U.S. investment in a country (logically, if one is working within the Baran-Sweezy framework), has exactly the opposite effect from that suggested by him. United States investment in Europe, while alleviating the problem of surplus absorption within the United States, can only aggravate it in Europe (assuming that new investment is greater than repatriated profits). Thus, within the framework of *Monopoly Capital*, postwar investment in Europe by the United States should have produced a *greater* need in Europe for military spending to absorb the surplus than would have been the case without it.¹

Stevenson is guilty of switching his argument in midstream. He begins by challenging the lack of correlation I found between military spending and economic growth, but switches to arguments about the actual causes of the differential economic growth rates among the advanced capitalist countries. Stevenson insinuates that factors other than military spending have determined the superior growth rates of most European societies. I could hardly agree more. My paper was only trying to show that the postulated relationship between military spending and economic stagnation could not be empirically supported. I do not deal with the real causes of rapid economic growth. Stevenson goes beyond suggesting that I must control for U.S. imports of the goods of other advanced capitalist countries, the amount of U.S. investments, and the level of World War II destruction, arguing that these factors may themselves be primarily re-

¹ In fact, in the countries with the highest level of U.S. direct investments the ratio of military expenditure to GNP was 3.3% compared with 2.8% in countries with the lowest level of U.S. investments. This finding is fully compatible with the reasoning of Baran and Sweezy (see table 2).

sponsible for the differential growth rates. Such findings would of course be quite consistent with my argument.²

Likewise the factors suggested by Friedman might very well be important factors affecting the rate of growth and economic stagnation in capitalist countries. The presence of strong labor parties, the investment ratio of earned profits, the industrial base, economic concentration, the level of foreign investments in the Third World, and the trade surplus are all quite possibly important factors determining the rate of growth of capitalist economies. The acceptance of this in no way runs counter to my argument that there is no evidence that there is a negative relationship between military spending and economic stagnation.

Also promising as an adequate explanation of differential growth rates is the relatively cheap labor available in Europe compared with the United States. This, and not the level of military spending, could very probably be among the principal causes of the more rapid growth rates in Europe. Unfortunately, it is impossible in the space allocated, especially considering the task at hand, to bring further empirical evidence to bear on these questions here.

Friedman concludes his critique with an alternative proposal to the Baran-Sweezy theory of stagnation. He quite rightly argues that there is a tendency for long-term economic stagnation in the world capitalist system. This tendency might, he argues, be temporarily held off in a few countries through the mechanism of trade or foreign investment (but only at the expense of the acceleration of the trend in others). He uses this argument to explain the behavior of the Japanese economy since the end of the war. He maintains that the economic stability of the world system

² Let us briefly try to bring data to bear on Stevenson's suggestions concerning the causes of differential growth rates among the advanced capitalist countries. Among the advanced capitalist countries those that suffered the greatest destruction during World War II (plus Israel) had a growth rate of 4.9% in the period 1950-68 compared with a rate of only 2.8% for the other countries—clearly a very significant difference. The differential decreased significantly with the distance from the end of the war. In the period 1960-68 the first group of countries had a growth rate of 4.6% while the second group grew at a rate of 3.3%. This is just as we would expect if the major cause of differential growth is the level of destruction (see table 1). No relationship can be found however between the level of exports to the United States and the rates of growth. The countries with the highest ratio of exports to the United States to their GNP grew at almost exactly the same rate as those with the lowest level of exports to her—3.9% for the latter and 3.8% for the former in the period 1950-68 and 3.9% for both in the period 1960-68 (see table 2). The relationship between U.S. direct investments as a percentage of GNP and economic growth is exactly the opposite of that suggested by Stevenson. The countries with the greatest amount of U.S. direct investment grew significantly slower than those with a low level of U.S. direct investments—2.6% vs. 4.6% for 1950-68, and 2.9% vs. 5.0% for 1960-68. This means that either U.S. investment hinders growth or that the United States invests in those countries that are relatively stagnant, and that such investment does not reverse the situation (see table 3).

was maintained by the heavy military spending of the United States, thus allowing the Japanese economy to grow rapidly without the necessity of wasteful spending on the military. It is not clear in Friedman's analysis exactly how military spending in other parts of the world system in fact alleviated the tendency toward economic stagnation in Japan. Although Friedman himself does not suggest a concrete mechanism it is possible that a Japanese trade surplus with the United States could have produced the result he suggests. A trade surplus may have generated investment opportunities in Japan, thereby alleviating its surplus absorption problem. Whether or not a Japanese trade surplus with the United States is large enough to account for the phenomenal rate of growth of the Japanese economy is an empirical question which cannot be resolved here.

Stevenson ends his critique with a projection that the current economic crisis of American society will spread to all the advanced capitalist countries, necessitating increased military spending everywhere. This is supposed to occur in order to bolster their economies and protect their foreign economic interests. But the trend could just as easily be in the opposite direction, that is, away from reliance on military spending and direct intervention overseas. The U.S. defeat in Indochina, the detente with the USSR and very possibly with China, an increasing willingness to let Third World countries own their own raw materials providing that the international marketing mechanisms remain in the hands of the transnational corporations, the prohibitive cost of military intervention especially with the disintegration of army morale and disorder at home, all point in the direction of less, not more military spending, providing only that other mechanisms can be found to absorb the surplus and prevent the endemic tendency to stagnation from exerting itself.

Friedman correctly argues in agreement with Melman (1965) that long-term economic growth is hindered by military spending. He goes on to argue that this puts pressure on the competitive nation-states of the world capitalist system to reduce their military expenditures. Thus Friedman makes the opposite prediction from Stevenson. This international competition to reduce armament spending will, according to Friedman, end in the collapse of economic stability in the world capitalist system, that is, in worldwide economic stagnation. Although the first part of this argument appears to be sound, his prediction of inevitable long-term stagnation actually being realized is on shaky grounds. Contrary to what Friedman suggests, although governments would like to increase their rates of growth, they can only do this within the parameters of preserving and advancing the short- and intermediate-range interests of the corporate economy. The primary economic problems facing the capitalist state are the preservation of the rate of profit in the private sector together with keeping the rate of unemployment and political discontent within reason-

able bounds. This implies a continuing high level of military and welfare expenditures. Another way of looking at this is that the first priority of government economic policy is to fight stagnation within the parameters of the capitalist system, *not* to maximize the long-term rate of growth. In the long run these two economic policies have very different consequences. In fact this is a contradiction in the capitalist economy—capitalism cannot both alleviate stagnation and maximize the long-term rate of growth.

Let us further develop the first part of Friedman's argument. In order to preserve the U.S. dominance of the Third World for the economic benefit of U.S. corporations, a very high proportion of America's economic resources have had to be spent on the military. This has resulted in a tremendous drain on long-term economic growth. Consequently the other advanced capitalist countries have been able to overtake the United States. The lesson is plain. The cost of maintaining world hegemony is losing the economic basis for maintaining it—that is, world hegemony is inherently self-contradictory. The strain on the U.S. economy becomes greater as the United States loses its superior international trade position to more efficient and more rapidly growing competitors. Domestic economic crises such as devaluation, inflation, the food and energy crisis, the failure of real wages to rise significantly since 1965, are the natural consequences. Probably the most likely response of the U.S. ruling class is to reduce, not increase, the level of military spending. Only by reducing military spending to make it compatible with that spent by its international competitors can the United States be expected to have a long-term chance of staying competitive internationally and reversing the long-term trend of slow economic growth with its recently manifested consequences of domestic economic and political crises.

The implications of the Baran-Sweezy analysis as well as that of Zeitlin and Stevenson are that a primary contradiction of capitalism is the irrationality of military spending (which might lead to a form of fascism in the United States), its inability to spend on social welfare, and, to the extent that military spending is unable to continue to keep the economy out of a great depression, economic stagnation. This argument, as well as that of Friedman, leads us to expect growing fascistic rule; endemic poverty; lack of decent housing, medicine, and education; growing environmental destruction; continuing imperial wars with the domestic consequence of growing alienation of the young inside and outside the army; and a gradually encroaching depression (when military spending proves inadequate). This analysis implies not only a prediction about the course of American society but also a strategy for a socialist movement which is interested in transforming that society into a rational and truly democratic social order.

The evidence in my paper suggests that such implications do not have

an empirical basis. If military spending is not necessary for capitalist economic prosperity, growing fascism, chronic underspending on social welfare, and gradual economic stagnation do not necessarily follow. An alternative course is just as likely. The development of U.S. society toward a social democracy of the kind existing in the Scandinavian countries is a very likely alternative. The McGovern movement of 1972 could thus be considered an augur of the future since it represents what is probably the most rational solution for the current crisis of U.S. capitalism. The McGovern program bolstered by significantly more "progressive" measures may very well be implemented in the United States during the next generation.

The path which all of the Western parliamentary democracies have consistently followed for some time has been toward a welfare capitalism of the Scandinavian type. The program of McGovern, and now perhaps the Kennedys, is precisely to take the United States along this road. The fact that almost all the other industrial capitalist societies are much further along this path than the United States (even though most of them still are not as economically developed) indicates the real possibility of this solution as opposed to that indicated by Baran and Sweezy. Military spending can be reduced significantly without in any way making qualitative changes in the capitalist system. Social welfare spending on housing, education, welfare, public power, medicine, job retraining, etc., can be very greatly expanded without threatening the stability or profitability of the capitalist system (as can be seen by the example of just about every other advanced capitalist country). Stagnation need not be a necessary consequence of monopoly capitalism. The social democratic welfare state solution shows every sign of being economically and politically viable. There is no comparative evidence to support the thesis that stagnation is endemic—providing only that the state is sufficiently alert to the management of demand and the ruling class sufficiently sophisticated to allow it to operate, to in fact manage demand.

The implications of the Baran-Sweezy argument is that a return to the conditions of the Great Depression and the antagonism between the Third World and the United States are the two factors around which political opposition to monopoly capitalism is most likely to develop. These notions of course have been very common on the Left for some time. The justification for both are found in the economic arguments of Baran and Sweezy as defended by Friedman, Stevenson, and Zeitlin. The implications of my argument are not a defense of liberalism or reformism as insinuated by Stevenson, or as perhaps entered into the positive evaluation of my original manuscript by the editors of the *AJS*, but rather that the most probable course of political opposition to monopoly capitalism is other than what Baran and Sweezy suggest. We should not count on economic stagnation,

nor on the growing irrationality of the system as revealed in increasing militarism/fascism, nor even in the continual primacy of the Third World issue (either internationally or domestically).

Given that monopoly capitalism is very probably a more flexible system than Baran and Sweezy suggest, that a social democratic solution for the United States is very likely, and that military spending, imperialism, and stagnation may not be the principal contradictions of the society, what then would seem to be in fact the primary contradiction of American society? And what then is the most likely course of the society, and consequently the most productive strategy for the Left to follow? Because these issues are just below the surface in all three critiques as well as quite explicit in the Baran-Sweezy book, it is necessary for me to outline another possibility here. Unfortunately, because of space limitations I will be unable to bring much in the way of detailed analysis and statistics to bear on my argument. I will nevertheless outline a very probable alternative.

The classical Marxist analysis of the contradictions of capitalism, that it is the irreconcilable antagonism between capital and labor that will bring the system down, has lost much of its popularity with the rise of New Left theory. The journal *Monthly Review*, of which Paul Sweezy has been the principal editor since its founding 25 years ago, has been highly influential among Left intellectuals in the United States. The consistent theme of the *Monthly Review* has been the rejection of the classical Marxist analysis of the revolutionary role of the working class and its replacement by the notion that the principal contradiction is not the exploitation and alienation of labor but rather underconsumption and imperialism in both its domestic and international ramifications. The book *Monopoly Capital* is the most systematic and extensive presentation of the theory behind this argument. It is my position that Baran and Sweezy generalize from the special conditions of the postwar era to general laws of monopoly capitalism prematurely and without adequate evidence, thereby mistakenly rejecting the fundamental tenets of the Marxist theory of contradiction and revolution—the role of the working class as the system's special victim and its role as the revolutionary agent of transformation.

It has been very fashionable of late to glibly quote statistics demonstrating how the industrial working class is a diminishing proportion of the population and that consequently this class cannot be expected to be a serious contender for power in advanced capitalist society. Yet a careful look at the trends shows that there is no factual basis for this claim. The proletariat in the Marxist sense of the term has grown from approximately 70% of the U.S. population in 1940 to approximately 85% in 1970 while the industrial working class (skilled, semiskilled, and unskilled manual

workers) has stayed more or less constant at 47% of the population since 1940. The working class remains as socially significant as it ever was.³

The working class is as oppressed as ever. Neither the degree of its exploitation nor the level of its alienation (both as defined by classical Marxism) has been significantly mitigated by the long-term rise in its living standards.⁴ Just as high a portion of the worker's product goes to capital as profit today as in the past. The worker has no more control over his work, the product of his labor, or over his life in general than he ever had. His work is for the most part as meaningless as ever. His personal relations both on and off the job have not undergone fundamental transformations since the Industrial Revolution. The rise in material living standards has not qualitatively altered the worker's life. Still he remains an object of the economic laws of capitalism. Still his life is governed by the requirements of maximizing profits. He remains a wage slave whose life energies are suppressed for the sake of capital. In Marx's words, "An enforced increase in wages . . . would be nothing more than a better remuneration of slaves, and would not restore, either to the worker or to the work, their significance and worth" (1964, p. 132). The oppression of the working class continues to provide the motive force or energy for that class's resistance to the system and its stake in a qualitative transformation of society.

Clearly, however, the U.S. working class has not realized the role expected of it by the classical Marxist tradition. Although most of the working classes of the other advanced capitalist countries have developed class consciousness and adopted socialist-communist politics, this has never been the case for the majority of the U.S. working class. The reasons for its exceptionalism, and especially for the conservatism of the last generation, is to be found in a complex of factors, probably the most important of which is the use of racial and national antagonisms to create and perpetuate mutual antagonisms within that class. The various segments of the American working class have normally thought of themselves as white (or black), Polish, Italian, Irish, Jewish, Anglo-Saxon, before they thought of themselves as workers. This consciousness has historically been developed and manipulated by the U.S. ruling class, who of course have had a strong economic interest in preventing the development of such a class consciousness.⁵ Recent years, however, have seen the relatively rapid undermining of the social and economic basis of racial and national

³ See my 1972 article, "Trends in the American Class Structure with Special Reference to the New Working Class and Blue Cellar Workers."

⁴ See Karl Marx (1964), especially the manuscript entitled "Alienated Labor," and Andre Gorz (1967) for full discussions of the alienation of labor under conditions of advanced capitalism.

⁵ See, e.g., Pope 1942; Bracey, Meier, and Rudwick 1970; Spero and Harris 1966.

antagonisms within the United States. Economic discrimination against blacks is being undercut by the needs of the corporate system for a more homogeneous labor force. Blacks and other minorities are being brought into the trades from which they were previously excluded by the action of employers interested in reducing the wage levels and increasing employee discipline.⁶ This contradiction in the system should be expected to lead to the long-run undermining of the economic basis of racism and hence to the removal of what has probably been the single most important barrier to the development of class consciousness in the U.S. working class.

Another major prop of the false consciousness of the U.S. working class has been its virulent patriotism and general support of American values. Compulsory universal education and a ruling-class monopoly of the media have been very powerful sources of a one-dimensional antisocialist consciousness. The trends during the last decade however, almost all point to a weakening of the traditional values among the young of all classes. Youth culture has spread to the factories; absenteeism, quit rates, disrespect for authority, and wildcat strikes against both management and unions are all growing in significance. Military morale has deteriorated so badly that radical changes have had to be made in discipline and life conditions to keep the army functioning. The breakdown in military authority which has manifested itself in "fragging" of officers in Vietnam, high rates of desertion, race riots, high rates of heavy drug use, refusal to obey orders and a general lack of motivation to fight was one of the major causes of the U.S. withdrawal from Indochina.⁷ Church attendance has been on a steady decline since the midfifties, traditional marital relations are being weakened with the rise in divorce and illegitimacy together with increasing sexual freedom. All kinds of crime rates have consistently risen in recent decades, as have the rates of incidence of mental disease and drug use and addiction.⁸ Almost all signs point to the decay of traditional morality and respect for the American way of life, to general alienation and anomie—a condition which could very likely be the prelude to the development of a new class morality and consciousness in the U.S. working class. The development of a socialist class consciousness of the type exhibited by much of the European working class is thus a distinct possibility within the next generation.

⁶ See U.S., Bureau of the Census 1972.

⁷ For example, in 1971 one out of 14 soldiers in the United States, according to official statistics, deserted (i.e., was AWOL for more than 30 days). This figure was the highest in modern American military history. It should also be noted that in recent times volunteers have a much higher rate of desertion than draftees (see *Statistical Abstract of the U.S.* [1972], p. 261).

⁸ The *Statistical Abstract of the U.S.* (various) is a chronicle of the decline of traditional American morality.

A further reason for the support of the American system on the part of the U.S. working class has been the claim drummed into its head over and over again that the American workers have the highest standard of living in the world and that the United States is unquestionably the greatest country. While once true, the exceptional living standard of the U.S. working class is no more. The result of the particularly slow rate of growth of the U.S. economy (in good part a consequence of the incredible waste of resources on the military, a situation required by the need to maintain world hegemony) has been the relative decline of the living standards of its working class. The standard of living in a number of Western European countries is now on more or less the same level as that in the United States, and, together with the living standard of the Japanese, is in almost all cases rising much more rapidly than the living standards of American workers, which have remained relatively constant since 1965.⁹

In summary, we can see strong tendencies which are likely to lead to the radicalization of the U.S. working class. A careful analysis of the factors operating on this class should lead the sociologically sensitive radical to dust off the old Marxist claims of the role of this class as revolutionary agent and to critically reevaluate the popular New Left notions of recent years concerning the primacy of the contradictions of imperialism and militarism. As important as Baran and Sweezy's work, *Monopoly Capitalism*, is and as significant a role as the journal *Monthly Review* has played in the last generation, the analysis incorporated therein does not and can not replace the analysis found in *Capital*. The primary contradiction of capitalism remains the capital-labor antagonism. It is to an analysis of the capital-labor relationship, not to a study of the role of military spending, that we must turn if we want to comprehend the movement of monopoly capitalist society.

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⁹ The trends are reported in the United Nations (1968a); the International Monetary Fund (1973); U.S., Department of Commerce (various).

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ON "ACCEPTING SIGNIFICANT OTHERS"

In choosing between alternative explanations for the same set of data, sociologists are guided by several criteria. The list of such criteria includes at least the following: parsimony, substantive utility, and the degree of agreement between observed and predicted results. Several techniques are available to assess the last of these, but unfortunately no rules of thumb exist for the first two, other than some vague notions of substantive usefulness as opposed to substantive unusefulness, and parsimonious as opposed to overblown explanations. Webster, Roberts, and Sobieszek (1972) report on their research involving significant others, and choose among seven models which, they suggest, could account for the results which they observe. The method of choice which they employ is to inspect the predicted and observed results and accept or reject one explanation at a time by considering the algebraic difference between the two values. This procedure leads them to the conclusion that the model which describes the data best is one which views the subjects as flipping a biased coin. The bias in the coin is assumed to originate in manipulations of the subjects' perceptions of the environment carried out before the observation reported in the data. I do not quarrel with their conclusions; rather, I wish to suggest that by looking at the problem of choice of models in another way, all of the explanations are reasonable, and we must invoke the criteria of utility and parsimony in our choices.

The data which are examined by Webster et al. are derived from a set of observations of a Bernoulli trials process. The subjects in the experiment make a series of choices which are independent of one another, and one event is observed with more frequency than another in a two-alter-

native situation. By constructing confidence intervals about the mean for each of the conditions of the experiment, we can assess the extent of agreement between the models and the data. For our purposes, the technique suggested by Larson (1969, p. 249) is employed to construct the intervals.

Because of the nature of the experiment conducted by Webster et al., two such sets of confidence intervals may be constructed. One of these is conservative; that is, it is likely to lead to rejection of the models. The other is liberal; that is, it is likely to lead to retention of the models. The conservative set of intervals assumes that observing a limited number of acts by a large number of actors is the same as observing a large number of acts by a small number of actors. For our purposes observing 15 or 20 reference choices by 20+ actors is assumed to be the same as observing 300 or 400 choices of one actor.¹ The liberal set of intervals assumes that each actor has a unique propensity to act in a particular way and that what we observe is this probability. Hence, we assume that we observe a distribution of probabilities, rather than a distribution of specific acts. See table 1 for the confidence intervals.²

TABLE 1
CONFIDENCE INTERVALS

CONDITION	LIBERAL ASSUMPTION		CONSERVATIVE ASSUMPTION	
	Upper	Lower	Upper	Lower
H(+) L(+)90	.63	.84	.76
H(+) L(-)87	.57	.79	.71
H(+) H(+)89	.42	.71	.63
H(-) L(+)73	.39	.62	.52
H(-) L(-)60	.26	.47	.37

In the case of the liberal assumption, the models produce only one prediction outside the confidence intervals. Since the dispersion of a Bernoulli distribution is inversely related to the size of a confidence interval, the choices made by Webster et al. under this assumption are in the range of a confidence interval of 35%.

¹ This is the assumption made by Webster et al. (1972). See their n. 6, pp. 581-82.

² An interval of 10% was chosen about the observed values as a mean for two reasons. The first of these is that the nature of the data reported is such that no inferential statistics clearly "fit" the problem at hand. Since we wish to choose an interval that is close to conventional usage, the 5% or 10% levels of confidence appear reasonable. The second reason is that since the size of the interval around a Bernoulli-type mean is inversely related to the level of confidence, we wish to choose a value which gives a small interval and still allows us to make reasonably good estimates of fit. Hence, the 10% confidence interval was chosen for the problem confronting us.

TABLE 2
PREDICTIONS OF MODELS AND OBSERVED RESULTS

Condition	Observed $P(s)$	Single Source	Single Source Disagree	Simple Additive	Simple Averaging	Averaging Given Disagree	Operator Model	Self- Max. Model
H(+)	.80	.79	.81	.81	.72*	.81	.82	.81
L(+)79	.79	.73	.68*	.79	.71	.79
H(-)	.75	.63	.63	.63	.63	.63	.58*	.79*
L(-)	.67	.46*	.46*	.48*	.55	.55	.47*	.64*
H(+)	.57	.46	.40	.40	.51*	.40	.41	.62*†
L(+)	.42
H(-)
L(-)

* Outside 10% confidence intervals for conservative assumption.
† Outside 10% confidence intervals for liberal assumption.

The confidence intervals generated under the conservative assumption produce a much larger number of predictions which fall outside the confidence intervals for the data. None of the models is wholly acceptable under this assumption, and the simple averaging, operator, and self-maximization models have more than one prediction falling outside the confidence intervals. We are again confronted with a situation where there is no "best explanation," although our problem is somewhat different here than under the liberal assumption. See table 2 for the fits of the model under the confidence intervals.

If we cannot choose between the models on the grounds of differences between the predicted and observed values of the aggregate proportions, perhaps we can do so on the grounds that one model accounts for the shapes of the observed events over time. Unfortunately, the models are not closely enough specified to be able to generate such predictions at this time. An alternative does exist in assessing the models in terms of their utility and parsimony.

The simple additive model would clearly be preferred in terms of parsimony in that it requires the least difficult set of ideas to generate predictions, while the operator model would probably be the least preferred because of its relative complexity. However, the additive model is the least desirable in terms of substantive utility because it informs us of relatively little that is of value in terms of the kind of explanations we would like to create; the variables employed are not esthetically pleasing. There is no easy solution to this dilemma, and I submit that there will be none until experiments can be developed which take advantage of differential predictions of the various models with respect to values other than their end states, or at the very least experiments which test predictions of the models in situations where the predicted outcomes are radically different from one another.

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REPLY TO SHELLY

Professor Shelly's comment addresses important problems. The major issue is how to choose among alternative models, and there is no universally accepted, satisfactory method to make this choice.

On accepting evidence, we believe confidence intervals based on an N of 300–400 (what Shelly calls “conservative”) are quite clearly correct; the “liberal” N would assume that our data are equivalent to having each subject make a single decision. A Bernoulli process assumes multiple, independent trials, and this assumption is met reasonably well by our data.¹ But despite a superficial appearance of objectivity, statistical tests have meaning only as they are interpreted by “arbitrary” standards. Why choose a .90 confidence limit instead of .95 or .99 (or .75)? Are statistically significant differences the socially and theoretically significant differences? Most important, do confidence intervals provide the most relevant means to assess utility of these models? This introduces our main point.

The purpose of research must be considered in assessing evidence, and Shelly seems to misinterpret our intent. We were not primarily concerned with comparing our models’ predictions to a “null hypothesis” baseline (that is, no difference between experimental conditions), the only situation in which sole reliance on statistical criteria is appropriate. Because of previous work by symbolic interactionists and experimentalists, we had some knowledge of how individuals process self-evaluative information; so it was likely that several models would be statistically *adequate*.² What we wished to do was to provide a basis for improving existing verbal theories of the processes. Before investing effort to construct a complete, precise, rigorous theory, it was worthwhile to see which of six plausible hypothetical processes seemed most likely to reflect individuals’ information processing. Every model had some precedent in the literature, and at least an intuitive rationale; it is possible that our least successful model would in fact be the one we eventually want to pursue. But for heuristic purposes—to decide where to place bets on payoff for future work—our results are helpful.

Criteria for evaluating theories and models are also arbitrarily chosen, but there is some consensus as to which are most significant. Parsimony

¹ Using the χ^2 test, there is no evidence for one-step dependence among trials in any condition. Four conditions show evidence of two-step dependence. (For example, three “change” responses in a row are less likely to occur than we would expect on a chance basis, given the $P[s]$.) Strictly speaking, lack of perfect independence makes confidence intervals based on Bernoulli trials inappropriate, but this does not make the data equivalent to one trial per subject. More reasonably, the effect is to widen intervals to somewhere between the “liberal” and “conservative” estimates.

² Several alternative models usually are constructed for situations in which the investigator already has a rough idea what the outcome will be; thus, statistical tests are not particularly helpful for comparing models. We implicitly decided to emphasize fit of predicted to observed $P(s)$ in choosing among alternatives. Other statistics such as variance could also be used (see Webster, Roberts, and Sobieszek 1972, n.9, p. 586) when models are adequately specified to predict them and experiments are designed to produce relevant data. There was no point in proposing models which account for changes over time, for example, because in the experiments all information had been given to subjects before any data were generated.

is not usually recognized as important until all other criteria—including logical rigor, substantive utility, integration with other theories, simplicity, elegance, adequacy of explaining observations defined as important—are met equally well by two or more models. With Shelly, we deny parsimony as a major basis for choice. However, we do not conclude from this that a parsimonious model is inferior to a more complex model. Shelly seems to prefer some version of the "typical operator model" (Webster et al. 1972, p. 590) to any of the simpler "typical additive models" (p. 583). But the operator model we tested was less satisfactory than any additive model except simple averaging on *all* criteria.

Substantive utility usually means "cognitive meaning," or "explanatory power"; Shelly equates the term with being "esthetically pleasing"—a far less satisfactory usage—when he rejects the simple additive model on this ground. Tastes vary; we have several colleagues who find the simple additive model quite pleasing. Esthetic taste here is probably related to the long-standing debate in survey research on mobility: whether "additive effects" or "interaction effects" models (comparable to our additive and operator models, respectively) are more satisfactory. There is no reason to believe that self-evaluation models will parallel mobility models, but additive models seem adequate for describing results of many mobility studies. (See, for example, several articles in this *Journal* [July 1971].) As we read the conclusion of Shelly's comment, he rejects the model which did the best job of accounting for data on grounds that it does not fit his intuitions of how people process information. If we adopt that criterion, there is no reason to perform empirical tests.

As we noted, our models are interpretations of ideas frequent in the literature, but they are not tied to any formal theory. We hope they contribute to knowledge, since they are more precise than most symbolic interactionist literature and since they test explicit versions of the related ideas of Berger and Fisek (1970). Since our work, Kervin has produced a theoretically derived information-processing model which improved on our predictions (described in Kervin and Webster 1973), thus supplying what we consider the next step in this line of work.

We welcome the developing interest in examining unstated assumptions behind theoretical formulations which Shelly's comment exemplifies, and we are pleased that he and some others chose our work for their analyses. The extents to which arbitrary criteria and intellectual taste enter scientific work have only recently begun to receive the systematic attention which their importance deserves. Our thinking has been stimulated and informed by Shelly's comments. In addition, we acknowledge benefits from dis-

Commentary and Debate

cussions with Nancy Tuma and Anne McMahon of Stanford University, and Richard Robinson and Doris Entwisle of Johns Hopkins University.

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Review Essay

The Moral Basis of a Backward Sociologist: Edward Banfield, the Italians, and the Italian-Americans

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This essay is a review of Edward Banfield's *The Moral Basis of a Backward Society* (1958), a major sociological work which after a decade of general acceptance has become even more influential as a result of the renewed interest among academics in white ethnicity. In an attempt to understand the characteristics of American ethnic groups, many scholars have felt it mandatory to familiarize themselves with the life-styles of the immigrants' forebears in Europe. Observers have hoped that much of the white ethnics' adjustment to America could be understood by the continuities and discontinuities they show with their past. One of the most attractive groups for researchers has been the Italian-American, and it is in the analysis of it that the most creative investigations have been done. In the study of Italian-Americans, the single most influential work has been Banfield's. While the book was written in the 1950s, it purports to describe a social system that existed with little change at least as far back as the turn of the century, the time when most Italians immigrated to America. As a result, *Moral Basis* . . . has been generally accepted as a historical as well as a sociological investigation; and it has been used by others as such.

Banfield's influence can be seen in many of the most successful works on the Italian-American community. It was accepted not only by Herbert Gans (1962) in the early 1960s, but by Humbert Nelli (1970) in his recent study of Chicago, and with slight modifications by Gerald Suttles (1968). While some scholars employ Banfield's argument to demonstrate continuity with the past, and others use him for contrasts between American Italians and southern Italians, there is widespread agreement on its usefulness. As a result of this reliance on *Moral Basis* . . . , Banfield has come to play an unusually large role in shaping our image of the Italian-American experience.

While Banfield's reception in American sociological and historical circles has been warm, he has fared less well among other scholars. A number of anthropologists specializing in Italian studies have been unconvinced of

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the representativeness and accuracy of his analysis. They have based their criticism of him on their fieldwork in other Italian villages. Most of these empirical attacks on Banfield have been part of an in-house debate and are unknown outside of anthropology. This is not the place to evaluate in detail their methodology and conclusions.¹ Rather, it is our desire to show that Banfield is not only vulnerable to attack by "specialists" in southern Italian anthropology (whose critiques we must wait upon), but is open to criticism by any thoughtful reader simply on the basis of the evidence Banfield himself presents. Since Banfield's book has been and is used by most students of Italian-American life, it is important to bring the weaknesses of his work to the attention of American sociologists and historians.

The introduction states the purpose of *Moral Basis* . . . quite succinctly: "The book is about a simple village in southern Italy, the extreme poverty and backwardness of which is to be explained largely (but not entirely) by the inability of the villagers to act together for their common good or, indeed, for any end transcending the immediate, material interest of the nuclear family" (pp. 9-10).

¹ The most important direct attack on Banfield is a recent article by Sydel Silverman (1968). Silverman compares and contrasts her own fieldwork in Central Italy with Banfield's research. She accepts Banfield's description of southern Italian behavior as accurate and accepts his contention that an amoral familistic ethos exists. She disagrees with Banfield as to the cause, and, more important, the significance of that ethos. Silverman stresses that economic and social conditions create and maintain value systems. Values are not the primary element in fostering or retarding change, or in explaining behavioral characteristics. Rather, they are more a dependent than an independent force. Her position is quite supportive of my thesis. However, Silverman's attack on Banfield involves the denigration of values as an independent variable to a much greater extent than is either necessary or convincing. The reason she so strongly challenges the power of values to determine behavior is that she does accept Banfield's thesis that the southern Italians do hold an amoral familistic ethos. Since she does not challenge him on this vital point, she is forced to wage her attack against him by emphasizing the unimportance of that ethos. It will be my contention in this article that Banfield does not prove his basic claim that such an ethos exists at all. Thus, it is not necessary to get into the more speculative and philosophical problem of whether values have an independent power or simply "validate," as Silverman puts it, ex post facto, a system created by economic and social forces. The Silverman article also contains an excellent bibliography of earlier articles and reviews by anthropologists that deal with the Banfield thesis. The most important of these is an article by Frank Cancian (1961). This article is an interesting discussion of Banfield but is explicitly only a "modification," not a refutation, of his thesis. Cancian takes the word "ethos" and changes it to "world view," which he then interprets in a much broader fashion than simply the concern for the "interests" of the nuclear family that Banfield posits. He makes some interesting and telling points about non-family cultural concepts that affect the southern Italian's world view and behavior. Nevertheless, his vision of a "world view," unlike Silverman's or mine, is as an independent force affecting behavior. He does a lot more justice to economic conditions than Banfield, but only for purposes of modifying, not refuting, Banfield's emphasis on cultural values. (It is interesting to note that Cancian misinterprets Banfield and attributes to him a much greater emphasis on economic causation than he actually provides.)

The explanation for this inability is the existence of an "ethos"² which Banfield calls "amoral familism" (p. 10).

By "amoral familism" the author means that the people act as if they were following the rule "maximize the material, short-run advantage of the nuclear family; assume that all others will do likewise" (p. 83). This "rule" has a number of major "logical implications" (16, to be exact) which later in the book he discusses in great detail, and which together make visible the underlying "ethos" which determines the group's behavior.

The first four chapters of the book give an overview of the small town of Montegrano, which is characterized by extreme poverty, lack of organized cooperative effort, and general disorganization and distrust. It is in chapter 5 that the formal thesis or "predictive hypothesis" of "amoral familism" is carefully presented.

Banfield introduces his concept with a traditional social scientific hedge, admitting that his hypothesis may be wrong, that "the coincidence of facts and theory does not 'prove' the theory," though he admits to confidence that his analysis is correct. Banfield then develops 16 points to support his contention that an "ethos" of "amoral familism" actually exists. We shall review the most important of these points to see if the evidence Banfield presents supports his own thesis.

The difficulty, as we shall see, is that the illustrations for many of the key points in the book, when examined carefully, are contradictory to his hypothesis or lend themselves more easily to a less complex interpretation than that of amoral familism. On first reading, the concise, almost epigrammatic generalizations are so well phrased, so evocative of how people could feel, that one can easily accept them without recognizing the weaknesses of the examples upon which the generalizations rest.

One of Banfield's most important points concerns the failure of private citizens to involve themselves in public affairs: "In a society of amoral familists only officials will concern themselves with public affairs, for only they are paid to do so. For a private citizen to take a serious interest in a public problem will be regarded as abnormal and even improper" (point 2, p. 85). As an illustration of this underlying "rule" which explains the actions of the average citizen, Banfield relates the story of Cavalier Rossi, one of the largest landowners in the area, and the mayor of a nearby town. Rossi told Banfield that if he, as mayor of Capa, went to the provincial capital of Potenza and asked to be heard concerning his views on the need for public improvements, he would be listened to. But if he went as a private citizen of Montegrano they would say, "Who are

² Banfield defines "ethos" in the way Sumner does, as "the sum of the characteristics, usages, ideas, standards and codes by which a group is differentiated and individualized in character from other groups" (p. 10, n. 3, quoting from Sumner 1906).

you? It is none of your business!" Thus, "a private citizen can do nothing" (p. 85).

The story is actually diametrically opposed to the general hypothesis, since it shows not what the body of "amoral familists" think, but rather what state officials believe. Given the reception he could expect, for a private citizen to leave public improvements to the state seems quite realistic and has no bearing on the desires or hopes of the average man.

Banfield seems to notice the nonsupportiveness of his own evidence and tries to discredit his own witness. He admits that official hostility has a bearing on the public's attitude but insists that, more important, the populace "refuses" to take any responsibility. To support this contention, Banfield gives from Rossi a long quote of only tangential importance which lacks the directness of his earlier testimony. He also discusses a Communist mayor, Farmuso, from another province, in an abbreviated and unfocused discussion that seems more irrelevant to this argument than supportive. Neither example provides compelling, or even reasonable, proof of amoral familism.

Banfield's third point states: "In a society of amoral familists there will be few checks on officials, for checking on officials will be the business of other officials only" (p. 86). In this instance Banfield's supportive material is even weaker than in the previous one. He tells us of the reaction of a young school teacher to the question, what would he do if he knew a public official was taking bribes? The man replied he would do nothing: "You are likely to be made a martyr. . . . It takes courage to do it. There are so many more dishonest people than honest ones that they can gang up on you . . . twist the facts so that you appear to be the guilty one" (p. 86). The reluctance to oppose bribery offers no support for the existence of an "ethos" of public irresponsibility. This fact is made even stronger by the testimony of a leading merchant that Banfield interviewed, who says he would not report an official's misconduct because "sooner or later someone would come to me and tell me it would be good if I didn't" (p. 86). One can almost see the hand on the merchant's shoulder, and the smile on the face of the official who "warned" him that it was in his interest to ignore dishonesty.

In point 9 Banfield hypothesizes that "in a society of amoral familists, the claim of any person or institution to be inspired by zeal for public rather than private advantage will be regarded as fraud" (p. 95). But again the supporting material is weak. While demonstrating the widespread distrust of apparent altruism, it gives no support at all to the theory that the people are motivated by an amoral familistic "ethos." Rather, people seem to be reacting to an unpleasant reality. For example, the author says the people believe priests are money grubbers and hypocrites and

that Socialists and Communists are simply "pious frauds." To Banfield this "extraordinary bitterness" and "unfairness" is the result of "projection"—the selfish amoral familistic villagers know they should be altruistic, but are not, and thus project their own malice onto others. Unfortunately, Banfield gives us a great deal of evidence that the priests and left-wing "radicals" are indeed pious frauds, or at least that one does not have to resort to psychological mechanisms to explain their position. As for the hostility to the priests, Banfield himself points out that the town is anti-clerical on general grounds. So even if the hostility is "unjustified," there is nothing psychological about it. Equally significant, the author tells us "the priests seem to be no more concerned with gain than are other professionals, and their level of living is no higher than that of the others" (p. 95). This is indeed a further indictment of the priests, since we are told throughout the book that "other professionals" are relentless in their money hunger and disregard for the people. The contrast between priestly attitudes and the church's official profession of selflessness also makes public hostility seem neither the result of "guilt projection" nor an "ethos" of amoral familism.

As for the Socialists and Communists, Banfield uses them to illustrate the untrustfulness of altruistic ideology. Point 10 states: "In the society of amoral familists there will be no connection between abstract political principle (i.e., ideology) and concrete behavior in the ordinary relationships of everyday life" (p. 95). He points out that both the doctor and pharmacist (the leading town "radicals") care nothing for their responsibility to the people. Indeed, "The pharmacist, a government-licensed monopolist, gives an absolute minimum of service at extremely high prices . . . and is wholly unconcerned with local affairs" (p. 96). The doctor, on the other hand, is very "class conscious" and looks down upon the peasants as his inferiors. In this case, the evidence supports neither point 9 (because the people's hostility to altruistic claims is a reflection of reality) nor point 10, which demonstrates not a general public "ethos" but the unreliability of the upper class to which the average person is forced to respond.

One of the strangest hypotheses that Banfield presents is his sixth: "In a society of amoral familists, the law will be disregarded when there is no reason to fear punishment. Therefore individuals will not enter into agreements which depend upon legal processes for their enforcement unless it is likely that the law will be enforced and unless the cost of securing enforcement will not be so great as to make the undertaking unprofitable" (p. 90). He tells us that this attitude is especially harmful to organization and economic development—a reasonable conclusion. For the generalization to be true and supportive of his belief in the "ethos" of amoral familism, however, the evidence must show that the average person acts

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as he does because he wants to, because he values his "irresponsible" amoral vision of the way social life should be conducted, or because he has a psychological "need" to act this way. Instead, the author's evidence once again shows that the average man is forced to adopt this mode of operation because the people with power act in that manner. Banfield admits: "An employer who can get away with it is almost sure to cheat his employees" (p. 90). While the cheated worker could appeal the problem to the marshal or to a labor commission in Potenza, he will not, because: "Frequently the worker is prevented by self-interest from taking his case to the Marshal. He cannot afford to be on bad terms with the employer: it is better to be cheated than to be deprived of employment altogether" (p. 90). Certainly the worker does not value being cheated, or wish to allow the cheater to get away with it. The law is "ignored" out of necessity, not out of choice. Banfield goes on to point out that economic cooperation between tenant and landlord which would be profitable for both is made impossible by peasant "anxiety, suspicion, and hate . . ." One man goes so far as to say: "I would prefer to be the owner of eight hectares rather than have the rental of 40 because if you are an owner no one commands you and . . . you are not always worried that tomorrow your half may not be yours . . ." (p. 90). Another says: "I would prefer a little land of my own to renting 40 hectares because . . . I hate the rich who sit in the breeze all year and come around only when it is time to divide the produce which I have worked hard with so many sacrifices to grow" (p. 92). Banfield seems to think this "unreasonable" attitude supports his hypothesis that all the people are amoral familists who do not want to cooperate, do not value cooperation, or psychologically cannot cooperate. In fact, like so many of his illustrations, it demonstrates that the peasant, weak and powerless, must react to the will of the powerful. Insofar as the peasant's action (anticooperation) and belief (without fear of punishment the law is worthless) represents an adjustment to reality as opposed to the carrying out of a cultural value, the concept of an amoral familistic "ethos" as the cause of behavior is untenable.

The basic problem with Banfield's analysis is that he ignores "objective reality," the external social and economic forces that impinge upon people. The concept of amoral familism presupposes that the inhabitants of southern Italy behave as they do because of processes going on within the confines of their own minds. Banfield's concept is ambiguous as to the exact nature of the mental process that determines social behavior, but that it is an internal, not an external, one he leaves no doubt.

One of the problems that must be faced in trying to discuss Banfield's theory of amoral familism is that the hypothesis implies different things at different points in the discussion. Sometimes the "ethos" seems to be a matter of "values": people do what they want to do, and what they want

is determined by their values. People have free will and can choose to act out their values. Thus, the values, codes, and standards are an independent causative agent. At other times, consciously held values have nothing to do with action; instead, a cultural ethos is deeply ingrained psychologically in people, and this mental framework forces them to act it out, regardless of their conscious ideals or aspirations. A culturally determined psychological attitude, socialized into young children at the earliest ages, is an independent force which determines action; it does not imply free will.

That Banfield is contradictory on this important point is not surprising. He, like many commentators on lower-class groups (e.g., Oscar Lewis in *La Vida* [1966]), analyzes their behavior in terms of independent, deeply ingrained psychological attitudes, and then goes on to talk as if the lower-class group values, approves, and desires to continue what it is doing. The reason for this illogical shift is that it is almost impossible for observers not to make moral judgments about the lower-class groups that they are trying to "help," and moral judgments of necessity require the assumption of choice and free will. It is very hard for social scientists using psychological concepts to allow their subjects to escape the responsibility for their acts that faithfulness to the psychological theory implies. If a man's behavior is determined by his toilet training or by Pavlovian reflex action, then he is no more morally responsible than an animal—a view academics have found very hard to consistently carry out in their analyses.³

However, this confusion in Banfield's work is not too important for our evaluation of the thesis. We do not have to decide which concept is fundamental for his theory and which one must be discarded for purposes of logical consistency. The evidence we have presented and will present disproves both processes (i.e., the power of values, the power of psychology) at the same time. To be convincingly proven, both of these "mental" mechanisms that determine behavior require that Banfield show that the people he studies possessed more than one behavioral option. To maintain the first case, which depends on free will, the author must demonstrate that people had the freedom to change their behavior and that their refusal to do so sprang from their "ethos" or value orientation. For Banfield to do this would require him to demonstrate that the reality of southern Italian life did not force the behavior he observed. To prove the second case also requires that the author show that more than one type of behavior was possible, since the lack of a realistic alternative does

³ Marxists have the same problem with upper-class groups that men like Banfield have with lower-class ones. Their theory excludes moral judgments against capitalists as individuals, since capitalists are simply acting out the necessities of their class position. Unfortunately, most Marxists have found it impossible not to attribute moral culpability, and therefore free will, to the "malefactors of wealth."

not allow proof that the psychological process (as opposed to social and economic conditions) is actually the independent force.

Without evidence that alternative behavioral patterns were a realistic possibility, Banfield's theory is simply based upon a belief (an unproven one) that an observer can study behavior and from behavior *in vitro* discover motivation. Which is to say that if a man sits on a garbage pile, he must do so because he wants to, because he values what he is doing, or because he has an internal need to do so. Unfortunately, people sometimes do what they despise. They live as best they can despite their ideals, values, or psychological needs. That is why it is necessary to find how much, if any, choice people have before we can even start speculating on their internal mental processes.

Banfield provides no proof that alternatives were possible, no evidence that the southern Italian environment does not force the behavior he observes. In fact, Banfield does just the opposite. All of the examples we have dealt with are much more persuasive as proof that the peasants of Montegrano are "realistically" adapting to the sociopolitical-economic environment of their world than that they have a value system or psychological framework that determines their behavior.

Banfield's disregard for the importance of external reality and its power to coerce behavior is well illustrated by point 7: "The amoral familist who is an office-holder will take bribes when he can get away with it. But whether he takes bribes or not, it will be assumed by the society of amoral familists that he does" (p. 92). In order to prove this part of the "ethos," he would have to present material demonstrating "unrealistic" and "unfair" cynicism among Montegrano's people. If their perspective were simply a correct observation, it would lend no support to his thesis. What does Banfield say? He tells us that everyone—upper class, middle class, and lower class—believes bribery exists. For example, the teacher says: "Today one gets ahead only by bribes and recommendations. All of the examinations are infected with this disease and those who get ahead are the ones with the most drag. To me this is odious. I would do anything not to have to see it" (p. 93). Is the teacher's perception correct? Banfield says: "There is no way of knowing to what extent bribery actually exists in Montegrano." But the author obviously believes corruption is not widespread, because it is only if bribery is not ubiquitous that we must resort to psychological and cultural explanations to explain people's distorted perspectives. If the people's view is true (especially with the examples he gives), it only shows that the powerful are dishonest, and the powerless know it, and react to it. It does not show the powerless like corruption, value it, would engage in it themselves if they had a choice,⁴

⁴ The fact that a powerless man who needs a favor in a corrupt social system will resort to bribery too, out of a sense of realism, is not a "choice."

or act compulsively because of some tenaciously held and uncontrollable psychological attitude.

Moral Basis . . . , as we have demonstrated, is packed with evidence that the people of Montegrano have little or no choice at all. Banfield's "predictive hypothesis" is contradicted at almost every point. For example, we are told that, unlike the people of Saint George, Utah (his American "control" town), those of Montegrano demonstrate their anti-cooperative "ethos," their lack of public spirit, by not having a newspaper, pressing the local government to build a "Median" school, donating their labor to the Catholic orphanage, or initiating an emergency ambulance service. This lack of constructive action is presented in chapter 1 as the thesis problem requiring explanation; the hypothesis of amoral familism is the answer.

However, the last chapter of the book provides the reader with far better reasons for the villager's inactivity than his elaborate cultural and psychological explanatory system. Banfield tells the reader that no local newspaper exists because the local village government has no power, and a newspaper is intimately linked to power. Indeed, he says, "If the administration of local affairs were truly decentralized [they are now totally centralized] this in itself might bring an independent local press spontaneously into existence" (p. 164). And why do the people (whom he severely criticizes for their inaction in chap. 1) not demand an ambulance service be set up? "At present it would be futile for a delegation from Montegrano to go to the prefect with a plan for, say, an ambulance service. The prefect would have no authority in the matter or, if he had, he would be unwilling to share it" (p. 163).

What in the conclusion of the work are revealed to be realistic restraints on action throughout the book are explained as the result of some mysterious, ingrained cultural "ethos"! By the end, Banfield allows us to see that the government of the province of "Potenza" is a highly centralized, bureaucratic, and undemocratic structure. The mayor and his council (the local government) are figureheads. The reality that the people of Montegrano must live with is "objective" political powerlessness. That they respond by apathy and cynicism seems, unfortunately, a realistic response which does not require Banfield's imaginative schema to explain.

Even more striking, the final chapter allows the reader to at last see how grievously the economic situation in Montegrano has been previously distorted. Banfield allows that factors other than amoral familism are important in explaining the villagers' actions, "especially poverty, ignorance and . . . [the] status system . . ." (p. 155). Of course, the "strategic, or limiting factor" is amoral familism, and the situation can best be seen and altered by focusing on it. He denies, for example, the possibility that

poverty might more usefully be seen as the "strategic factor." Nevertheless, when discussing the possibility of change in southern Italian society, Banfield admits: "The sad fact is that Montegrano's isolation and relative lack of resources give it a comparative disadvantage which no amount of cooperation can overcome" (p. 157).

This remark highlights what Banfield's treatment of objective economic conditions up to now has obscured.⁵ For example, he compares an "extended" family of northern Italy with his amoral familistic "nuclear" family of Potenza. The prosperous extended families of the north are for Banfield an Italian model of the cooperative spirit that Montegranians lack. The extended or "stem" family has "trained the peasant to act organizationally"; "the father . . . organizes the labor force of the family . . . [and] because they have learned in the family to subject themselves to the discipline of a group . . . [they] unlike those [peasants] of Montegrano, are able to work together. Some participate in farmers' associations, and there are cheese-making and other cooperative undertakings in the district" (pp. 144-45).

The contrast between the normal, nonamoral familistic group in the north, and the deviants of the south, is explained on the basis of historic land tenure arrangements in the two areas. The accident of southern conditions made the extended family impossible, and thus deprived the Montegranians of the ability to be prosperous, cooperative, and moral like their northern brothers. The reality of infertile, barren soil in the south and rich fertility in the north (the Po Valley is where the northerners he discusses live) is ignored. The fact that the northerners cooperated because cooperation promised prosperity—it paid off—while the barren land of the south could deliver nothing, escapes mention.

Taken together, the contradictory or ambiguous examples Banfield uses to illustrate the details of his amoral familism hypothesis, and the out-and-out refutation that his last chapter provides makes Banfield's full explanation for conditions in southern Italy unconvincing. Determined to maintain a cultural-psychological explanation of human conduct as opposed to a socioeconomic one, he is required to force his observations into a highly questionable pattern.

Banfield's failure to prove his thesis suggests the need for a more convincing one. The evidence, as opposed to interpretation, in Banfield's book is consistent with the hypothesis that the people of Montegrano live in a harsh and brutal geographic, economic, political, and social environment. Their land lacks resources or fertility, their government is distant and uncontrollable, their peasant status is despised, and they are discriminated

⁵ Of course he makes periodic bows to "poverty," but in keeping with his basic position, he sees it as a secondary, not a primary, problem.

against by an upper class that exploits them and degrades them.⁶ Under these conditions the people have on the whole realistically adjusted their expectations and actions.

The people do not value or have a psychological "need" to act out factionalism, uncooperativeness, or powerlessness, but they see no possible alternatives. While, in all probability, they "ideally" subscribe to the desirability of charity, cooperation, trust, altruism, and dynamic action, they push these desires aside in their daily behavior in favor of a "practical" morality. In this respect the Montegransians resemble the ghetto dwellers of America that observers such as Ulf Hannerz (1969) and Hyman Rodman (1963) have so well portrayed—people who have ideals but are forced to live by a code of practical compromises. Like the American observers who see an ingrained culture of poverty among the poor, the existence of which is hypothesized from behavior, Banfield has falsely assumed that values, ideals, and needs can be discovered from how people act without reference to the external constraints that confine them.

One of the major issues with which a discussion of Banfield's thesis must be concerned is the problem of social change. According to Banfield, and scholars of like mind who study blacks in America, change will be terrifyingly slow: "Under the most favorable conditions it might take two or three or four generations for nature to restore and re-invigorate the social bonds . . ." (p. 166). This view follows logically from both "interpretations" of his "ethos" thesis: (1) to the extent that there is a cultural-psychological mechanism created by socialization among southern Italians that has an autonomous existence it will take a long time to change; and (2) to the extent that southern Italians have a value system that they approve of, they will be slow to exchange those values for new ones. The same logic has been applied to black Americans with the same pessimistic conclusions. Since we severely question the existence of an autonomous cultural-psychological mental set, or a special amoral value system, we maintain that the adjustment to new economic and social conditions will be relatively short.

In presenting a hypothesis that stresses structural (socioeconomic) causes for southern Italian behavior, we do not deny that forced, traditional adjustments can cause trouble when conditions change rapidly for the better; it would be foolhardy to claim, as some extreme situationalists do, that no "cultural lag" is to be expected; that people can discard their "practical morality" as quickly and easily as old clothes. However, we do not believe that the obstacles to adjustment will retard it for long.

⁶ Banfield is very emphatic that the upper class lacks cohesion, and thus the claim that "class" oppression exists is absurd. He refuses to recognize that class oppression can exist even if the individuals in the upper class lack class consciousness. All that matters is their individual relationship to those below them.

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Nevertheless, we must admit that there is no empirical proof for either conclusion on this vital prediction, only conjecture. Almost all the material available about the probability of rapid or slow social change is highly speculative and philosophical. Though opinions on this question are often found in detailed empirical studies of lower-class groups, such as Liebow's *Tally's Corner* (1967), Ulf Hannerz's *Soulside* (1969), or Lewis's *La Vida* (1966), their actual research provides little proof one way or the other since they have studied stable, not changing, situations. The vast "empirical" literature on the war on poverty and job-training experiments, which has impressed upon some conservative social scientists that the poor cannot easily adjust to new opportunities, is too poorly constructed and thought out to be useful (e.g., many such studies emphasize the high drop-out rate for blacks in job-training programs, even when jobs are guaranteed at the end of the training period. The fact that these jobs are "dead end" or are not highly valued in American society generally is not mentioned as a possible explanation). As a result; for the time being we can only give a prediction based on our analysis that external factors, not values or psychology, are the key to behavior, and little more.

Before concluding, it might be useful to briefly mention one aspect of the Banfield book passed over so far. Banfield engages in a bit of amateur psychoanalysis—painting a gruesome image of the psychological makeup of southern Italians. Their selfishness and immoralism is carried to such an extent that the people grow up without benefit of an internalized moral sense—a conscience. In Banfield's view, the Montegransians exhibit what he calls a "Pinocchio complex"—they do what they like without recourse to guilt; the only check on them is external restraint. The social, amoral, familistic ethos, combined with, and at least partly based on, the conscienceless individual's psychology, leads to a society that in Banfield's view is totally incapable of preventing the Hobbesian "war of all against all" without outside intervention from the Church and the government in Rome.⁷

This is a disturbing picture, especially since in a normal society the lack of an internalized conscience is one of the key characteristics of the psychotic personality. At the very least I am sure no one would want one of Banfield's Montegransians on the loose near himself or his family without a gun handy. What Banfield describes both on the individual and social levels (i.e., Pinocchio and the potential Hobbesian jungle) is so abnormal a state of affairs that one could only expect such conditions to

⁷ This view of southern Italians has directly or indirectly had an effect on American sociologists observing the Italian-American community. For example, Gerald Suttles in his brilliant study of the Addams area of Chicago tells us that Italian-American youth grow up with no internalized moral restraints, only with an awe and fear of external (adult) authority.

exist in a time of war, famine, or plague, and certainly not as part of a "normal," long-term state of affairs. For us to "prove" that Montegransians could maintain civilized life without the benefit of outside influences is beyond the scope of this article—assuming the reader really believes proof is necessary. It is interesting to note, however, that the Pinocchio complex with its picture of moral retardedness that Banfield paints is based almost exclusively on the responses of one informant, a man called Vitello. That so sweeping an indictment (and that is what it is) of a whole people can be based upon such limited evidence is as frightening as the image he creates.

American sociologists and historians who wish to know something about the background and characteristics Italians brought with them to America should use Banfield's work with great care. It has a usefulness as a source of material on how some southern Italians may have acted and lived, but not why they did so, or what values, ideals, "ethos," or psychological needs they may have possessed. The Banfield book should also alert scholars studying the Italian-American community to the danger of assuming, rather than investigating, the relationship between ingrained cultural traits and socioeconomic conditions. It has been too common for American sociologists and historians to take for granted that Italian-American attitudes to education, government, or voluntary organizations have been the result of deeply held cultural traits rather than the structural conditions they have lived under in Europe and America.

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Review Essay

Socioeconomic Background and Achievement. By Otis Dudley Duncan, David L. Featherman, and Beverly Duncan. New York: Seminar Press, 1972. Pp. xxiii+284. \$14.50.

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This book is impressive, almost overpowering, in the scope of its subject matter, methodological erudition, technical virtuosity, and substantive empirical findings. In general terms the book deals with the construction of models which identify factors determining a man's income and his educational and occupational attainments. Clearly, these outcomes are largely responsible for one's social achievements, status, even well-being; for this reason the models should create intense interest among scholars of several disciplines in the social sciences. The following list of variables which are examined as determinants of these achievement outcomes should further whet appetites across the disciplines: parental background, composition of family of origin, national origin, race, intelligence, aspirations, motivations, peer group influences, schools, the influence of one's wife, first job, age at first job, migration, fertility, and child spacing—some of which variables are of course interesting outcomes in their own right.

The models developed consist of systems of equations. The outcome variables are dependent variables which are systematically determined by three types of independent variables: background variables, such as race and father's education; intervening variables, such as intelligence and peer group influences; and career contingencies, such as age at first job and migration. The system of equations represents a temporal sequence of outcomes, which means that a "first" outcome variable, like educational attainment, can also be an independent variable in the second equation, in which occupational attainment is the outcome. This recursive structure allows estimating the parameters or relationships among the variables by ordinary, least-squares regression methods.

The basic data source is a large survey of males aged 25–64 in 1962. The variables are scaled in standard units, that is, each variable is subtracted from its sample mean and divided by its sample deviation. The parameter estimates are called path coefficients (or β -weights).

In the first three chapters, the authors present the form of the model, explain the statistical estimation methods, and analyze a simple, "basic" model that previously had been the main focus of this book's predecessor (Blau and Duncan 1967). The succeeding six chapters expand upon the basic model with variations in independent variables selected, different data sources, and more elaborate models (discussed below).

The mathematical and statistical techniques displayed are considerable and, unfortunately, are likely to repel many readers who could profit from

* The author is grateful to David L. Featherman for helpful comments on an earlier draft and to Arthur S. Goldberger for helpful discussions at several stages, but neither colleague necessarily agrees with any part of the current version.

the book. I will argue below that some of these barriers to a wider readership could have been avoided; initially, however, it should be emphasized that even the nontechnical reader can learn much from the methodological discussions, empirical findings, and accompanying interpretative discussions.

The authors' style of presentation is a mixture of modesty and boldness. They explicitly invite the reader to question their work and to offer alternatives. I have accepted this invitation and will criticize, I hope constructively, more than I will praise or summarize.

I present the criticisms under the headings of what I perceive to be four related shortcomings of the book: (*a*) a vagueness of the specific purposes of the authors' analyses; (*b*) a selection of a mode of presenting their findings which hinders translating them into an understandable language, and thereby impedes useful and practical applications of their results; (*c*) excessive complexity of the models and sometimes confusing exposition; (*d*) a number of dubious rationalizations of the structures of their models, and a number of dubious interpretations of some of their empirical results.

Purposes

Because the models and estimating techniques are often complex, a clear and explicit statement of what specific purposes the empirical results might serve would help to motivate and guide the reader. Specifically, consider the purpose of informing community or governmental decision makers about policy actions. Assume that the process or "experiment" under consideration is upgrading the occupational attainment and/or affecting the family size of a cohort of young married people, wherein one outcome of interest is the effect of these changes on the educational attainment of the children. In this case it would be necessary to ask two preliminary questions about the models in this book, which include these variables: (*a*) is the process which generated the reported empirical results sufficiently similar to the contemplated policy action, and, (*b*) if so, what are the measured effects of the variables on the outcomes in terms that the policymaker can understand?

By comparison with policy formulation, the purpose of prediction in a context without deliberate intervention is somewhat less demanding. We might, for example, be interested in whether the observed variation in father's occupational attainment and family size yields accurate predictions of the educational outcomes of the children. (Let us set aside the important question of what we mean by "accurate," since this would require that we specify what the predictions are to be used for, the costs of making mistakes, and so on.) Speaking loosely, we can say that accurate predictions may be obtained if the process of the social and economic system which generated the existing data will continue to operate in the same way. More specifically, the question is, how stable is the covariance structure of the relevant set of variables over time?

The relevance of the stability of the covariance structure may be

illustrated with the basic model demonstrated in chapter 3, in which father's occupation, father's education, and the number of siblings are determinants of the son's educational attainment. We can assume further that the wealth—or, in economists' terms, the permanent or normal income—of the parents is also a determinant of the educational attainment of the children. (Current income as measured for a single year is not designated, because it is often a weak proxy for wealth or normal income.) The income (or wealth) variable is omitted from the authors' model. Does this lead to biased estimates of how educational attainment will respond to changes in the three background variables over time? The answer is, no, if the covariance structure among the three variables and income remains the same. It is reasonable to believe that the effect of wealth previously was partially "captured" in its relations—the "old" covariance structure—to the three background variables. The variation in wealth that was independent of these three background variables was "captured" in the error term of the model. But the ability of the three background variables to yield unbiased predictions depends on that covariance structure remaining the same in the new context to which it is applied.

The authors do discuss the general issue of omitted variables. However, this issue cannot be evaluated or even become interesting unless the purposes of the model are made explicit. What is the process or experiment about which inferences will be made? If the purpose is to know the effect of a policy action, such as changing ultimate family size (perhaps by the introduction of a family planning program), then we explicitly see that the intervention will introduce variation in one background variable and not in the other three (i.e., father's occupation, education, and wealth). Here the demands for unbiased measurement of the effect of a change in family size are very strict because we must know its "net" or "partial" effect, holding constant all three variables, including wealth.

Clearly, many other variables besides wealth could become relevant omitted variables; but, to repeat, the purpose of the models and their domain of application must be known before evaluating the question. The authors do not address this question explicitly, although my impression is that historical prediction is the purpose which their models best serve.

Translating, Interpreting, and Applying the Empirical Results

Path coefficients.—The use of path coefficients limits the usability of the empirical results for many purposes. To illustrate this, we can assume that the reader wants to know the estimated quantitative relation between years of schooling of the father and the occupational achievement of the son. The reader must first transform a standard-deviation unit of the two variables into their raw units—that is, actual years of schooling and actual occupational scores. If the regression coefficients of the raw units were presented, it would not be necessary to make special calculations to determine that, for example, an extra year of the father's education (having 9 [or 13] years of schooling completed instead of 8 [or 12])

implies a prediction that the son will advance in occupational attainment by two points. (A linear and additive specification of the independent variables is the functional form for the models throughout the book.)

After applying the path coefficients to the transformed units, the user will still have to translate the change in occupational scores—two points in the example being discussed—into another unit which permits understanding and communication of the result. One can determine that a two-point gain means advancing from brakeman to switchman, bartender to waterworks man, or miner to shoemaker, to take three examples of pairs of occupations separated by two points.¹

The user will need, however, to persevere further. The two-point change reflected only the direct effect of father's education on son's occupation. In addition, one must calculate the indirect effect—via the direct effect of the father's education on the son's education, and then via the direct effect of the son's education on the son's occupational achievement. Transformations of the path coefficients into raw units and then of the raw units of occupational scores into "representative" changes in occupations are again required.

One alternative to using the occupational categories as a way of sensing the meaning of the results is to convert the occupational scores into units of annual earnings. This conversion could be accomplished in several ways but should serve as a complement, not a substitute indicator of occupational change. There is a need for some indicator that would be commonly understood. Indeed, if the authors would provide such translations for the reader, they would no doubt feel compelled to comment on the practical significance of the quantitative relations. This would be beneficial, since they are eminently qualified to do so. As it now stands, much of the significance of the book consists of piecemeal empirical findings, which are difficult to translate into predictions or evaluations of the social processes, and of findings which are mostly of methodological interest.

Measuring the dependent variable.—It is unfortunate that the authors provide no resumé of the operational definition of the occupational score variable or of its strengths and weaknesses. The reader can pursue the cited literature on occupational measurements, but ploughing through this is neither convenient nor adequate. On page 7 the authors simply state that the respondent's occupation is "the ultimate outcome of the whole process" and that "the letter *Y* stands for the variable, occupational socioeconomic status, as measured on the scale developed by Duncan." The reference (Duncan 1961) reveals that a predicting equation was constructed in which an occupational prestige score (*Y'*) was regressed on age-adjusted measures of income (*I*) and educational attainment (*E*) in

¹ The numerical example above is hypothetical. Actually, in the first set of empirical results the direct effect of the father's education on the son's occupation is only about one-sixth this size and is, in fact, labeled a zero effect in the diagram (p. 39) and text discussion. Clearly, larger changes in occupational scores would reveal sharper and more meaningful contrasts in the occupations, and it is not my point to dwell on the difficulty of evaluating "small" changes.

the occupation. (I is the percentage in the occupation which earned \$3,500 or more in 1949, and E is the percentage in the occupation with 12 or more years of schooling completed, with both variables scaled in standard units.) Forty-five occupations with prestige scores based on a 1947 survey were used, and the 1950 census provided the income and education variables. The equation is:

$$Y = \hat{Y} = a_0 + a_1I + a_2E.$$

The estimated relationship in the Duncan article cited is:

$$Y = -6.0 + .59I + .55E.$$

Therefore, the SES value, Y , is determined for any occupation for which the requisite information about I and E exists.

We see that Y could be defined, with probably little change in results, as the simple sum of the percentage earning more than \$3,500 and the percentage with 12 or more years of schooling in the occupation, since $.59I + .55E$ is approximately equal to $.57(I + E)$, and the constant $.57$, as well as the constant -6.0 , is irrelevant to the performance of Y in the regression models.

It should also be pointed out that because Y is defined in standard units, a one-unit change in Y will mean different amounts of changes in the SES scores whenever the variance of SES differs from one sample to another. Thus, the user should be aware that a dependent variable, for example, son's educational attainment, may be more responsive to, say, SES of the father in Study A than in Study B, even though the coefficient of father's Y (equal to SES in standard units) is smaller in Study A than in Study B. Clearly, the issue is interesting only if the units of the occupational scores have some meaning to readers—if only in the limited way that IQ scores or temperatures have meaning. If there is no cardinal meaning to the units, then, of course, any statistic which measures the sign of the relationship—regression coefficient, correlation coefficient, or β -weight—would serve equally well. I assume that there is some interest in the actual SES scores, and I would insist that there is interest in the “natural” units which measure schooling, income, age, and other variables used in the book.

Several other questions may be raised about the key variable, Y :

1. Would the equation for Y , which was based on the 1947 and 1950 data, change substantially if it were reestimated with more recent and more abundant occupational prestige scores and the 1960 Census data? Are there advantages in using the more recent measure in a study of occupational attainment in 1962? What differences would this have made? What criteria could determine whether the differences are important or unimportant?

2. A conceptual statistical problem with the estimating equation is the apparent inconsistency of the estimate a_1 , the coefficient of I . If the economic theories of a trade-off between income and prestige are correct (as the authors themselves imply on p. 243), then changes in prestige will, all things being equal, change income. The mechanism is that an

increase in prestige will attract new entrants to the occupation, and the augmented supply will depress wages and earnings. The reverse process would occur if the prestige of the occupation underwent an autonomous decline. A dependence between the error term of the equation and the income variable results; this will produce an inconsistent estimator of a_1 .

3. A comparison of the SES score and the actual prestige score on pages 48–49 creates in my mind more uncertainty about the properties of the score. The occupations listed for fathers and sons in the Detroit Area Survey are assigned the prestige score, Y' , and the SES score, Y .² The same scores are, of course, assigned to any given occupation regardless of whether it is the son's or father's. Thus, the regression between Y and Y' , for example, $Y = a + bY' + u$, will yield approximately the same intercept and slope coefficients, whether fathers or sons are the source for the sample of occupations. In particular, the error term, u , in the regression will tend to be positive or negative for any given occupation, irrespective of whether the error term is computed from the sons' or fathers' regression. Given a correlation between father's and son's occupations, it therefore seems untenable to assume uncorrelated errors between Y and Y' for fathers and sons. Yet the authors make this unwarranted assumption to justify a series of calculations examining the issue of whether Y or Y' is the "truer" measure of occupational status and prestige.

4. The definition of the SES score suggests that a "mechanical" correlation exists between it and the educational attainment of the respondent; the more educationally homogeneous the respondent's occupation is, the higher the correlation will be. Mitigating this correlation is the fact that the educational component of the SES score is the percentage with 12 or more years of schooling completed, whereas the respondent's education is measured in years of schooling completed.

5. Finally, the operational definition of the SES variable may explain why the relation between father's education and son's education is less when the SES score is used than when the actual prestige score is used. The reason is that the father's SES score has a built-in educational component, so the net effect of the father's education on the son's education is diluted when the father's SES score is included—given the positive correlation between a person's education and the educational measure of his occupation.

These remarks are not made contentiously. Indeed, they are motivated by the technical detail about scales of measurement which the authors have scrupulously provided—in a scant five pages! The remarks are intended to reopen the issues of the meaning and interpretation of occupational scores.

Excessive Complexity of the Models

Chapter 6 involves an investigation of aspirations and motivations as intervening variables. It is the longest, the most difficult, and to me the

² The prestige score, Y' , was actually based on scores derived from a 1964 survey

most confusing chapter in the book. Complexities arise when latent (or unobserved) variables and simultaneously determined (or "feedback") variables are introduced. Although these formulations are sometimes essential for a correct modeling of the process under investigation, I believe they produce in this chapter a number of redundant and uninterpretable empirical results, and they serve to illustrate my general criticism of excessive complexity.

1. An early model in the chapter provides an example of how a much simpler presentation can achieve the same conclusions as a more complex model. Consider Model I:

$$x_3 = p_{31}x_1 + p_{32}x_2$$

$$x_6 = p_{63}x_3 + p_{61}x_1 + p_{62}x_2,$$

where x_3 = educational plans of the student, x_1 = father's SES, x_2 = student's IQ, x_6 = actual educational attainment, and the p 's are path coefficients. The error terms are omitted from the equations.

The authors offer a new model which I claim is in effect Model II:

$$x_3 = p_{31}x_1 + p_{32}x_2$$

$$x_6 = q_{63}x_3 + q_{62}x_2,$$

which incorporates the authors' assumption that $p_{61} = 0$. The authors conclude, by a route discussed below, that $q_{63} > p_{63}$. But isn't it self-evident that the effect of the educational plans of the student on his educational outcomes will be larger in an equation in which father's SES (x_1) has been omitted?³ The authors claim, furthermore, that their alternative model "requires a 'sleeper effect' of intelligence to account for the correlation of attainment with background." Again, the required sleeper effect is self-evident from the imposed assumption that background (x_1) has a zero direct effect.

The obviousness of these and some other conclusions is, however, obscured by a confusing substitution of symbols; namely, $x_a = p_{3a}x_3 + e$, where x_a is called the latent decision, and e may be considered an error term that is uncorrelated with other variables. The authors' model can, in fact, be derived by simply substituting in Model II the three unknown quantities, $(x_a - e)/p_{3a}$, for the observed variable, x_3 . A number of manipulations and calculations will then produce only a display of p_{ai} and p_{ja} coefficients instead of the p_{3i} and q_{j3} coefficients available in Model II. While there may be methodological interest in an estimate of a reliability coefficient for p_{3a} , the limited practical information available from Models I and II is obscured by the presentation of results in terms of a latent variable.

2. Single-equation and recursive models are not strictly appropriate

of the National Opinion Research Center, rather than from the original 1947 survey by NORC. However, this is irrelevant to the points made above.

³ Clearly, x_1 will be positively correlated with both x_3 and x_6 , and x_3 will be positively

when there is mutual determination, or simultaneity, among variables in a model. As the authors point out, contemporaneous measures of "motivation" and "occupational achievement" are likely to reflect mutual causation. For me this fact undercuts any substantive interest in the authors' ingenious, but involved, reanalysis (pp. 116-30) of a model by Harry J. Crockett, which ignores this feedback relation. However, readers who are willing to accept the assumptions that the measure of motivation (Thematic Apperception Tests) is both a predetermined and an accurate measure of the motivation that applied when decisions were made years earlier will find the authors' reanalyses of more than methodological interest.

In a subsequent model in this chapter the authors confront a similar problem in examining the relationships among five motivational variables: (a) "subjective achievement" (SA); (b) "importance of getting ahead" (IGA); (c) a latent variable, "ambition" (A), which is a substitute for IGA; (d) "commitment to work" (CW); (e) a latent variable, "work orientation" (WO), which is substituted for CW; and, finally, (f) a series of conventional variables: father's occupation, siblings, education, occupation at marriage, income at marriage, current occupation, and current income. I confess that I could not fully follow the development and estimation of some of the models with these variables; therefore the following comments are tentative.

Two recursive models are offered. The first is called a "naive" recursive model, in which father's occupation, siblings, and IGA determine education; all four in turn determine CW, and all five in turn determine the remaining conventional variables. In a second model the unobservables, A and WO, replace the observables, IGA and CW. The introduction of A and WO make the model underidentified, but empirical estimation goes forward by assuming various values for path coefficients linking A to IGA and WO to CW.⁴

After an extraordinarily elaborate (even for this book) set of calculations, the latent-variable model yields empirical results: many are similar to the naive model, others change signs as different assumed reliabilities are used, and others are simply anomalous. (For example, both A and IGA are negatively related to education; the only significant coefficient of father's occupation on son's occupation is negative; and so on.)

To be fair, the authors label this section exploratory, and they admit to a certain arbitrariness in their causal-recursive ordering of IGA, CW, and SA. (Let me note that a component of CW and, indirectly, A, is "the work I do is one of the most satisfying parts of my life," and yet

correlated with x_6 . These results guarantee that $q_{63} > p_{63}$ except in the implausible case—especially implausible as an empirical matter—in which x_1 affects x_6 only through its effect on the empirical measure of educational aspirations (x_3).

⁴ The source for obtaining assumed values of the path coefficients is a set of correlations based on a model in which SA is determined by the conventional variables, IGA is determined by A and SA, and CW is determined by WO and SA (see p. 132). Different assumptions about the reliability of the correlations involving A and WO provide several values of the path coefficients.

CW and A are specified as causally prior to "current occupation" and "current income"! The most important questions I would raise about this section, however, do not concern the empirical results; rather, they are related to the question of purpose, mentioned above. The authors state their central concern to be testing whether motivations "are 'relevant and important' to social mobility" (p. 134). What is the context in which this issue is posed? "Important" by what criterion and in the context of what process?

3. Educational and occupational aspirations are also examined in chapter 6 as intervening variables in a system in which father's SES and IQ are background variables. To these variables the authors add a latent motivation variable, M . I examined in detail the first (WISC-I) of two models, and I will merely assert here that I believe this addition of the latent variable yields no information that could not directly, more easily, and more understandably be derived from the observed variables and the same restrictions on the model that the authors must make to convert the underidentified system (with M) into an estimatable one. (I will be glad to send any interested reader my documentation of this assertion.)

Incidentally, the necessary restrictions are not altogether palatable, because they require that (a) the respondent's educational aspirations depend on his occupational aspirations and on his father's SES but not on his own IQ; and (b) that his occupational aspirations depend on his father's SES and his IQ but not on his educational aspirations.⁵ Regarding point a, it seems unreasonable to assume that a person's IQ, which partially determines his success in school, won't directly affect his aspirations for further schooling. With respect to point b, it also seems unreasonable to assume that occupational aspirations are not influenced by educational aspirations. There are probably many young people who do well in school, who aspire to college, and who will, as a consequence of their educational aspirations, aspire to an occupation that befits a college graduate. Thus, the system of equations determining the two aspiration variables appears hopelessly underidentified. Perhaps it is a matter of taste, but I believe it is more instructive to readers simply to point this out, rather than to expend so much effort—some pedagogical rewards notwithstanding—in attempting to square the circle.

Interpretations of the Models

The variety of models and abundance of empirical findings expressing quantitative relationships among variables guarantee a rich learning experience for the attentive reader. No doubt many readers will engage in

⁵ These restrictive assumptions are not explicit in the authors' model using the unobservable M , but they are explicit in the model which uses the observable variables. The authors' model and the reformulated model are the same except for the definitional substitution, $X_M = (X_4/p_{4M}) + e$. X_4 is observable while the other terms are not.

silent disputations with the authors about their models and the interpretations made of the empirical results. I will confine my long list of potential comments and criticisms to a few cases which illustrate some general points.

One source of challenge to some of the authors' interpretations stems from the economist's view of the household as a decision-making unit. This view leads to a distinction between endogenous variables, which represent the outcomes of the decisions, and exogenous or predetermined variables, which impinge on these decisions. These categories of variables do not necessarily conflict with the categories the authors use, but the framework may point to some different interpretations. For example, in contrast to the authors' model, in which educational attainment, age at first job, status of first job, and, in places, age at marriage are ordered in a particular temporal sequence, the economist's view might be that these contingency variables comprise a common set of mutually interdependent choices. Thus, the decision to pursue a college career is simultaneously made along with the decisions to enter a first job at approximately age 22 (to enter, for example, the engineering occupation) and to marry at approximately age 23 or 24. The term "simultaneous" should be thought of in this context as referring to an inextricable set of decisions which have been meshed over a period of time prior to the outcomes being analyzed. Viewed in this perspective, the authors' temporal ordering, in which some of these variables appear as exogenous right-hand-side variables presumed to be uncorrelated with the error term in the model, is not correct. The ordinary least-squares regression estimates of the recursive system would produce inconsistent and biased estimates of the parameters in the simultaneous-equations system; and if the latter system is the correct modeling of the process, only these estimates are relevant.⁶ I should add that in my opinion economists have not been very productive in empirically estimating these processes, so that there is little evidence to demonstrate which model—recursive or simultaneous—is "better." Recursive models do have the virtue of being less complicated.

Economists have more successfully employed the household decision-making model to analyze behavior of wives, an aspect of behavior which is not given much attention in this book. The shortcoming may be serious if, as I believe, the wife's decisions about labor force activity and child-bearing are in part "causes" and in part "effects" of the various outcomes of husbands which the authors examine. A fuller treatment of the wife's behavior would challenge, to cite just one example, the author's estimate of the effect of fertility on the husband's income and occupational status (see section 8.5). Although the authors indicate one reason why causation runs from fertility to income (and occupation), a common alternative view in economic models is that the husband's income bears a net negative relation to the wife's labor force activity and that wives who work less

⁶ A useful discussion of this point and, indeed, of several issues that appear in this review is provided by Goldberger (1973).

are likely to have more children.⁷ Incidentally, the wife's labor force and fertility decisions are usually considered to be "simultaneous."

Another useful distinction in applied econometric work is that between variables that represent potential policy instruments and those that are not amenable to policy manipulation. This perspective leads to a meaningful basis for asking the question of whether the effect (coefficient) of the variable is "large" or "small," "important" or "unimportant." As argued elsewhere (Cain and Watts 1970), regression coefficients provide a necessary ingredient to answering these questions.⁸ Note that this claim for the policy relevance of the regression coefficients is separate from the claim that they have greater stability than correlation coefficients or β -weights (see Tukey 1954). The book's focus on path coefficients, simple and partial correlations, and "contribution" to the multiple R^2 provides statistics which are inherently ambiguous for assessing and ranking the importance of variables in multiple regression models.

Many examples could be cited to illustrate this ambiguity. In chapter 7 the authors reveal that the effect of the wife's education on her husband's occupational status is positive and statistically significant. They say, however, that "wives apparently contribute little directly to their husband's career—at least by way of their socioeconomic characteristics and personality traits" (p. 179). Setting to one side the questionable causal nature of the wife's education with respect to husband's occupation, we have a right to ask, what is a "little" effect here? The authors also say that the wife's education relates "slightly more importantly" to husband's occupational status than does his father's occupational status. What justifies this claim? Does marrying a wife with a college degree instead of a high school degree have a large effect on husband's occupation compared to his having a father with a professional, as opposed to a craftsman, occupation? Apparently the authors' answers depend on a ranking of the variables according to the sizes of their path coefficients. But they do not explain how the sizes of path coefficients, which embody a mixture of the quantitative relations (regression coefficients) and the sample variances, are useful measures of a variable's importance.

In another example, school inputs are downgraded as a determinant of educational aspirations on the basis of an analysis devoted to partitioning variances into "within"- and "between"-school components (pp. 191–98).

⁷ If the hypothesis (not necessarily economic) that higher income permits the parents to afford more children is true, then this would be another source of a causal relation running from income to fertility. A test of this hypothesis would, as the authors point out, require that current income and current occupation be good proxies for the theoretical variables, the "permanent" income and occupation which the parents expect to have (or attain).

⁸ The standard errors of regression coefficients are also necessary to assess the statistical significance of the effect and to permit confidence limits to be established. The authors seldom report the standard errors of the path coefficients and sometimes do not report whether the coefficients (or correlations) are statistically significant.

But this lengthy and involved exercise says nothing about the policy-relevant issues of the quantitative effects on educational outcomes of changes in school inputs. It is not clear whether the authors address this question, given their fixation on accounting for explained variation (i.e., for the R^2). I am surprised to see this fixation, since one of the authors has written an excellent article on the limitations of variance partitioning (Duncan 1970). My guess is that if the purposes for which the model was estimated were made explicit, many of these issues would be clarified.

This section of the review has been monopolized by an economist's perspective. Let me close with one example of a disputed interpretation that more directly involves sociologists and cultural anthropologists. In analyzing black-white differences in income in chapter 4, the authors in effect attribute the gap to two sources which are defined by the equations determining income: one source is the different values of the variables for the two racial groups, and the other is the differences in parameters (effects) of those variables. When they equate the values of the variables, specifically the values for the person's father's socioeconomic level, they find that only 22% of the income gap is accounted for. They assert that this estimate "should give pause to those who cite the 'vicious circle of poverty' or the 'culture' of poverty as the fundamental cause of the black's lower income" (p. 60). One can agree with the authors' skepticism about these explanations and yet disagree that the values of the variables rather than the values of the parameters are what the exponents of the "culture of poverty" have in mind.

Conclusion

This outstanding book represents a milestone in the grand research tradition of the development of quantitative models of the processes of achievement and social stratification. As promised, it provides "systematization and synthesis," "interpretation and generalization"; in so doing it transcends descriptive fact presentation or, at the other extreme, armchair theorizing. There is hardly a section that does not stimulate and challenge the reader with some interesting empirical relation; some skillful use of statistical technique; some enlightening comment on the methodology of measurement, model construction, and estimation in the social sciences; some insightful comment leading to, in their words, "a clearer vision of the entire process of social stratification."

The criticisms and questions raised in this review are intended to suggest ways in which this type of research could reach a wider audience and contribute more effectively to the application of social science to coping with social problems.

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Book Reviews¹

Émile Durkheim: His Life and Work. By Steven Lukes. New York: Harper & Row, 1972. Pp. xi + 676. \$17.50.

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This is a very important book on the modern founder of sociology, one which no serious student of our discipline should miss. Within months of its publication it was given front page reviews in both the *New York Times* and the *Times Literary Supplement*, which should attest to its unusual impact. What makes this impressive work of scholarship so worthwhile is the thoroughness and objectivity with which a youthful author treats the subject matter, presented originally as a doctoral thesis at Oxford.

The genus (not genius) of this book is intellectual portrait. There are few studies of this kind in sociology, which is unfortunate, since the intellectual portrait ideally can provide contemporaries with a distinct reflexive learning experience: it gives meaning and life to the sociological tradition by relating sociological issues and problems formulated by an important historical figure to the social milieu in which they arose. Reinhard Bendix's *Max Weber*, John R. Staude's *Max Scheler*, and J. D. Y. Peel's *Herbert Spencer* are rare companion pieces to Lukes's *Émile Durkheim*. If we think of a model intellectual portrait as offering the reader (1) a lucid in-depth exposition of its subject's ideas and concerns, (2) an interpretation of how these ideas relate to the times in which the subject lived (the existential nexus between a subject's writings and his *Lebenswelt*), and (3) the relevance of his ideas and concerns to a deeper understanding of contemporary sociology, then Peel's study is the better approximation of this model.

In 559 pages of text and 55 pages of bibliography Lukes covers almost everything written by Durkheim, on Durkheim, and publicly known about Durkheim. Lukes has left hardly any pebble unturned in exploring dossiers, archives, course catalogs, and personal sources, so that his book is a must "data bank," one which considerably updates the anterior studies of Durkheim by Gehlke, Alpert, and Benoît-Smullyan, among others.

The book has three main sections, covering three major stages of Durkheim's life and evolving thoughts. Lukes devotes the first section to Durkheim's formative years (1858–87), marking him and his generation with the disaster of France's crushing humiliation at the hands of Germany in 1870 and the consequent search for national regeneration. Unfortunately, Lukes fails to consider that the bloodbath of the brief but intense civil war of the Paris commune was equally disastrous for the social fabric and

¹EDITOR'S NOTE: Harvard University Library has recently published its sociology shelflist. Volume I is a classified subject listing by call number of all its volumes in sociology, and Volume II is an author and title listing for sociology.

that this event may have been an important background consideration in Durkheim's pronounced aversion for social conflict, leading him subsequently to stress a sociology of reconciliation. In any case, we see the young intellect leaving the quiet of the province (but it should be noted, which Lukes does not, that Durkheim's Lorraine was a center of pro-republican sentiment, which might have some bearing on Durkheim's lifelong attachment to, espousal of, and participation in the principles of the Third Republic) for the more challenging environment of Paris. He arrived during a period of great intellectual and political ferment, and at the Ecole normale supérieure, he—like so many others of various denominations—broke formal ties with the religion of his parents. By the end of his studies, culminating in a study visit to Germany, sociology had started to become for Durkheim a religion, one which inspired a burning faith and whose main proselytizer and charismatic leader he was to become in later years.

The second part of the book spans the years 1887–1902, representing that half of Durkheim's academic career spent at the University of Bordeaux. Lukes provides valuable information about these important years. Although he does not touch upon the social milieu of Bordeaux or upon Durkheim's interpersonal ties with town and gown, Lukes does cover all Durkheim's writings, courses, and professional activities; most of these writings and academic projects are probably unfamiliar to American sociologists, since they embrace a much greater range than the well-known works of Durkheim's Bordeaux period: the *Division of Labor*, *Suicide*, and the *Rules of the Sociological Method*. Thus, Lukes provides valuable materials on Durkheim's interests in pedagogy, kinship and marriage, law and politics, and religion. In a separate chapter Lukes discusses receptivity to Durkheim's ideas and the controversies they generated, including a lengthy discussion of his debate with Tarde. Finally, the author devotes a chapter to Durkheim's participation in the Dreyfus affair, which not only culminated the multiple political schisms and tensions of French society but also resulted in the definitive displacement of conservatives in French higher education by the liberal, secular, progressive elements, with which Durkheim identified. Had the conservatives won, Durkheim might have spent the rest of his life at Bordeaux.

Part 3 covers the last stage of Durkheim's career. His appointment to a chair at the "new" Sorbonne (a position enabling him to exert great influence and power as to the direction sociology would take, including appointments in sociology at various centers) symbolized his triumph in establishing sociology as part of the French curriculum. Again, Lukes thoroughly covers Durkheim's activities, his courses and writings on the sociology of religion, knowledge, morality, philosophy; in addition, Lukes discusses Durkheim's practical concerns and activities until his untimely death in the midst of the Great War.

What aspects of Durkheim emerge from this intellectual portrait? Certainly, Lukes successfully conveys his prodigious activism. His organizing and participating in a great many voluntary associations and public lobby

groups, mainly those involved in secular causes and civil liberties (in the context of his time), indicate that Durkheim (like Weber, Schelen, and Spencer) was not a simple "ivory tower" type. Thus at Bordeaux he organized chapters of the French League of the Rights of Man, and the "Jeunesse Laïque" (in addition, of course, to organizing and directing the *Année sociologique*, which served as a journal, through which Durkheim propagandized his conceptions of scientific sociology, and as a laboratory of sociology). During World War I, even though his son and student André was missing in action, a tragedy compounded by the death of other close colleagues (such as Hertz), Durkheim maintained a feverishly active pace. He helped organize a committee of distinguished intellectuals, who prepared documents to boost the morale of the country (one of his own monographs, a study of German mentality, prefigures later personality-and-culture studies). He was president of the Commission for Russian refugees, providing indirect assistance to Trotsky and his co-workers (p. 558); while his health deteriorated from fatigue and depression, he also found time to concern and inform himself about the Jewish question in Russia (p. 557).

Active in writing, organization of intellectuals, and polemical fights over sociology and a variety of causes, Durkheim emerges as an intellectual *engagé*. His militancy always gave priority to sociology, however, since sociology was *his* cause above all else, certainly above party politics. Seldom oblivious of the political aspects of social phenomena, he never let politics seduce his rational approach. The materials Lukes has uncovered make us fully aware of Durkheim as an intellectual activist of uncompromising standards, fully experiencing social issues, always exercising intellectual integrity and social responsibility to the fullest extent.

Lukes has provided a major study, which is an insufficient treatment of Durkheim only in the respect that it is anatomical rather than physiological. The study eschews interpretations and is bare of reflections. It lacks drama and a feel for tensions. The inner man, his genius, the ambiguities of Durkheim the sociologist and the person are left undiscussed. For example, Lukes does not wrestle with the puzzle that Durkheim, very much a liberal and moving in left-wing circles (most of his ablest students were Socialists, and Durkheim had very close ties with Jaurès), developed a sociological approach and perspective that has a much closer affinity with a conservative model of society. Lukes does not probe the similarities between Durkheim's and Maurras's models of society (he dismisses this possibility summarily in a note on p. 541). The fact that Maurras was a monarchist and more Catholic than the Pope should not deter a careful scrutiny of his and Durkheim's proposals for reconstructing the social order. Nor does Lukes reflect on the influence of Saint-Simon's writings on Durkheim's conception of a new social order. For example, the ambiguity of fascism and socialism coexisting in the same model of society can be traced to Saint-Simon.

Finally, Lukes does not explore Durkheim's relationships with others, which would both humanize and intellectualize him more fully. At the

biographical level, the relationships between Durkheim and certain key figures such as Bergson, Espinas, Jaurès, and Liard (the latter having played a crucial part in Durkheim's rise on the academic ladder) are insufficiently treated. I would also like to know more about the formation of the *Année sociologique* group as a collectivity: how Durkheim selected those who joined the inner sanctum, and who broke with Durkheim and for what reasons. These considerations would give life and flesh to the study of the master's school (as Jones did for psychoanalysis).

At the intellectual level, Lukes does a thorough analysis of the various critiques sociologists and anthropologists leveled at Durkheim's theories during his lifetime: in discussing the *Elementary Forms* Lukes demonstrates his familiarity with the influence of Durkheim on contemporary social anthropology. But he is tantalizingly silent about the influence of Durkheim on contemporary sociology and the significance Durkheim has for today's sociology. For example, Lukes ignores Parsons, Nisbet, Gurvitch, Duvignaud, and Bellah, among the important sociologists of our time who have benefited from Durkheim's legacy; nor does Lukes discuss the recent critics of the structural-functional approach, which is Durkheimian in inspiration.

There is something to be said for these avoidances. Lukes has been faithful to his purpose of studying Durkheim objectively. Although the objectivity is in my opinion too strenuous, I do not hesitate to recommend this book highly. In one sense, it is an exhaustive study of Durkheim. Yet, the last word on Durkheim remains to be spoken. There is need for two other studies as follow-ups on *Émile Durkheim: His Life and Work*. One might be called *Durkheim's Paris* (using as a model Janik and Toulmin's brilliant recent study, *Wittgenstein's Vienna*), and the other, *Durkheim Today*. These potential studies and the present volume would create a composite and exciting intellectual portrait of the beacon light of our discipline.

The Social Philosophers: Community and Conflict in Western Thought.
By Robert Nisbet. New York: Thomas Y. Crowell Co., 1973. Pp. xii+466.
\$10.00 (cloth); \$5.95 (paper).

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Cross-fertilization among disciplines has long been an "in" idea among academic policy makers, but the offspring, alas, is most often sterile. What a delight it is, therefore, to be able to report that Robert Nisbet has been eminently successful in combining social and intellectual history to produce a work that is a lucid and enlightening introduction to both.

Eschewing the commonly used chronological and narrative approach to the history of ideas, where it would seem as if ideas beget ideas in some kind of social vacuum, Nisbet has chosen to root firmly the philosophies and systems of ideas he discusses within the social and historical contexts

in which they emerged and found their reception. His book is both a capsule social history of Western Europe since the time of the Greeks and a sophisticated introduction to the most eminent thinkers who reflected on the perennial predicaments of men within the specific historical circumstances in which they found themselves.

Each of the first three chapters is devoted to the discussion of a major institution or, as Nisbet prefers to call it, community. I was at first surprised to find that there is no chapter on kinship, but soon realized that Nisbet chose to deal with the Military Community, the Political Community, and the Religious Community as three types of institutions which, at least in the West, can be seen as basically antagonistic to the principles of kinship-based society. The military and political order of the polis came into being on the ruins of Athenian kinship. Imperial Rome destroyed *patria potestas* to enhance the power of the state. Christianity endeavored to transcend ethnic and kin ties by basing its appeals on the new universalistic community of Christian believers. The major thinkers in the realm of military, political, and religious thought, from Cleisthenes to Grotius, Clausewitz, and Mao; from Plato to Bodin, Hobbes, and Rousseau; from Saint Augustine to Erasmus, Montaigne, Luther and Calvin, were all concerned, so Nisbet contends and illustrates with fine excerpts from their writings, with a quest for new community to substitute for the lost world of locality, ancestral worship, and the kinship order.

The other three chapters deal with what the author calls the revolutionary, the ecological, and the pluralistic community. Here the institutional emphasis is less pronounced and the discussion of ideas takes pride of place. Revolutionary thought from the English Puritans and French Jacobins to Marx, Sorel, Lenin, and Fanon is passed in review. This is a tradition less congenial to Nisbet than most of the others, and though he tries to be fair-minded, one still senses that his heart is not quite in it. (In the pages on Marx, in particular, a number of errors have crept in: Marx studied in Berlin, not in Jena, the *Anti-Duehring* was authored not by Marx but by Engels; *The German Ideology* was written before, not after, *The Communist Manifesto*.)

By contrast, Nisbet's discussion of communitarian thought from Saint Benedict to Thomas More, Proudhon, and Kropotkin is infused by the author's broad sympathy with attempts to overcome the atomization of *Gesellschaft* and "otherhood" through communal "brotherhood." The chapter on the pluralistic community, with its discussion of such thinkers as Aristotle, Althusius, Burke, and Tocqueville, men in whose footsteps Nisbet has attempted to walk since his first writings, is a fitting climax to the book.

Occasionally Nisbet succumbs to the characteristic vice of intellectual historians: that is, to the overestimation of the power of ideas. He asserts, for example, that "unfortunately, the ideas of Althusius had little effect in the seventeenth and eighteenth centuries. If they had had a significant effect, Western Europe might have been spared the age of the absolute, centralized, and omniscient state, built upon atomized individuals" (p.

406). This gives the impression that had people but read the right books, instead of Hobbes and Rousseau, the world we live in would have been fundamentally different. But such idealistic excesses are fortunately quite rare.

As a whole, this is a balanced and intellectually exciting treatment of ideas in context. If one wishes to introduce students of sociology to the adventure, and the clash, of ideas in the Western tradition, here is the book that is most likely to stimulate their imaginations.

Marxism and the Origins of British Socialism: The Struggle for a New Consciousness. By Stanley Pierson. Ithaca, N. Y.: Cornell University Press, 1973. Pp. xiv+290. \$10.75.

Tom Bottomore

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The subject of Pierson's study is one that has often been alluded to but rarely examined in any depth. Why did Marxism have so little influence upon the British working-class movement in the later 19th century? After all, Marx took Britain as his model of a capitalist society in which the class conflict between bourgeoisie and proletariat would develop; there had already been a significant working-class movement—Chartism—which Marx and Engels referred to as the "first workingmen's party of modern times"; and the International Working Men's Association, established in 1864, in which Marx had a predominant influence, was closely connected with some sections of the trade union movement in Britain.

There are two different ways of accounting for the fact that the British labor movement acquired only much later, and even then far from completely, a socialist outlook. One is to refer to the economic and political conditions which encouraged the working class to accept the general framework of established society while pressing for reforms within it—for example, the benefits which a large and organized section of the working class could gain from Britain's industrial monopoly; the Reform Act of 1867, which gave workers a share of political power; the favorable report of the Royal Commission on trade unions in 1869. The other approach emphasizes the strength of the existing intellectual traditions, which constituted a barrier to the diffusion of Marxist or, more generally, socialist ideas.

While recognizing the importance of economic and political circumstances, Pierson concentrates on the intellectual environment. He distinguishes, analyzing briefly and clearly, three strands of thought which formed an obstacle to the introduction of Marxism: first, the continued effort by religious thinkers (Coleridge, Arnold, Maurice) to formulate the ideal of a Christian commonwealth; second, the romantic social vision of Carlyle and Ruskin; and third, utilitarianism. It was against this background that attempts were made, in the 1880s, to introduce Marxist ideas, notably by H. M. Hyndman in the Social Democratic Federation and by William Morris in the Socialist League. (Rather oddly, Pierson does not

mention the earlier attempt by Marx himself, through the IWMA, which was not wholly without effect; see the study by Henry Collins and Chimen Abramsky, *Karl Marx and the British Labour Movement* [London: Macmillan, 1965], esp. pp. 301-3.) These attempts were not notably successful. Neither Hyndman nor Morris was a very profound or systematic thinker, and both of them produced a strange mélange of Marxist ideas and notions drawn from native intellectual traditions, so that, as Pierson observes: "For Marxism the outcome was a rapid loss of theoretical coherence" (p. 59).

Nevertheless, even this kind of propagation of Marxist ideas had *some* influence, which Pierson traces in the development of later, more or less socialist groups—the Fabians, the Ethical Socialists (Glasier, Blatchford, and others), and the Independent Labour party. It would have been useful here to compare, even briefly, the course of events in Britain and Germany. Not only were the economic and political conditions quite different in Germany (the Anti-Socialist Law, in particular, had an important effect in creating a more revolutionary working-class outlook), but the intellectual traditions were much more favorable to the reception of Marxism, with the result that by the end of the century Marx's theory had become the center of serious discussion among social thinkers, while in Britain it was largely ignored.

Far from trying to establish such distinctions, however, Pierson is more inclined to treat the British case as typical of the whole European working-class movement. Thus he writes in his concluding chapter: "The fate of Marxism in late Victorian Britain was not an isolated and idiosyncratic phenomenon. It anticipated the wider development of Marxism in the twentieth century. Later European Marxism has followed much the same pattern of breakdown and reassimilation which took place in Britain" (p. 278). I doubt if this could be supported as a general judgment on the development of the working-class movement and of Marxist thought in Europe during the 20th century, and it detracts somewhat from the author's own effort to establish the specific features of the development of socialism in Britain. It also raises another doubt about the usefulness of his general framework of analysis. Pierson seems to conceive the introduction, diffusion, acceptance, or rejection of Marxism as a once-for-all affair, the outcome of which now can be plainly seen. What is obvious, however, is that the Marxist theory of society, for all the criticisms that have been leveled against it, has enjoyed notable revivals throughout the 20th century—in the revolutionary period of 1913-20, in the 1930s, and again in the 1960s—and has continued to be one of the most powerful animating ideas in European social thought (and now in the world at large). The influence that it will eventually have in creating new forms of society is still quite undecided.

These are serious deficiencies in the general orientation of Pierson's study. What is most valuable in it is the series of intellectual portraits which show, though not always in as much detail as one would wish, the diverse outcomes of the confrontation between Marxism and other, indig-

enous social doctrines, and the problems that arose in the relation between theoretical ideas and the practical needs of a political movement.

Bonds of Pluralism: The Form and Substance of Urban Social Networks. By Edward O. Laumann. New York: Wiley Interscience, 1973. Pp. xi+342. \$14.95.

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Working Papers on a Sociometric Approach to Urban Social Structure would have been a more clearly descriptive title for this collection of related empirical essays. The chapters are organically related in that they employ the same body of data, the same collection of analytic techniques, and the same body of overarching theory. Moreover, the conclusions of the various chapters are mutually relevant, furthering the development of the larger theory. Still, in spirit the work is a series of connected progress reports about a continuing program of research rather than an expository essay on intergroup relations.

Bonds of Pluralism is addressed to a question of fundamental importance in the study of urban social structure: how can the "macrostructure" of the metropolitan community be described in terms of the "microstructure" of interpersonal relationships? This issue is theoretically crucial if social structure is conceived of as an outcome of a social process and is thought to be related to distributions of behaviors, attitudes, values, and so on *because* these are also affected by the same social process. Informal systems of sanctions, that is, along with social psychological processes, are part of the explanation for the personal correlates of social position. The *form* of the macrostructural relationship between two categories of the population (e.g., German Methodist or Eastern Catholic) consists of the microstructure of interpersonal relationships—including friendships, neighboring relations, kinship, formal and informal interactions, and the like—among individuals in those categories. The microstructure itself contains formal properties, such as the extent of homogeneity or connectedness, which are relevant to the description of macrostructure because they presumably have casual significance in the operation of urban social structure. As Laumann's title implies, the form of urban structure is studied in relation to its substance: the attitudes, values, interests, and behaviors of the incumbents of social positions.

In such broad outline this theory is not new to sociologists. But describing urban social structure in rigorous detail is easier said than done, and Laumann proposes to do it: his contribution lies in making the theory specific, even to the level of empirical reality. This goal entails problems not merely of epistemic correlation but, more significantly, of specifying general theory to the point where it can be applied unambiguously to the connection between microstructure and macrostructure under urban con-

ditions. Such empirical applications of sociological and social psychological theory have generally been hard to pin down: (1) Microstructures can be observed in the laboratory, but do they work the same way in the urban community? (2) Macrostructural differences can be observed in urban populations, but exactly how are they connected to the actual structure of interpersonal relationships? (3) The entire set of bonds among the individuals or positions comprising an urban population or structure must be thought about but is much too vast to be observed directly.

Laumann attacks such thorny problems with some basic ideas from social network analysis, some clever applications of survey methodology, and one version of social distance theory formulated as smallest-space analysis (as in his previous *Prestige and Association in an Urban Community* [1966]). The latter formulation is greatly clarified in an excellent appendix to *Bonds of Pluralism* written by David McFarland and Daniel Brown: "Social Distance as a Metric: A Systematic Introduction to Smallest Space Analysis." The combination of these elements results in one of the clearest statements of empirically grounded theory on social process (in terms of which urban social structure can be described) yet available to sociologists.

Specifically, Laumann approaches the form of urban social structure in empirical studies of the "friends of urban men: accuracy of description, mutual choice and attitude agreement"; of friendship choices as patterned by ethnic and religious group membership as well as by occupational roles; and of the homogeneity and connectedness of friendship networks. The substance of urban social structure is considered in an empirical study by Stephen Cutler on "Voluntary Association Membership and the Theory of Mass Society," one by Laumann and David Segal on "A Re-Examination of the Status Inconsistency Hypothesis," and one on "The Persistence of Ethnoreligious Differences in the Worldly Success of Third- and Later-Generation Americans." All are analyses of the same body of data, namely, a survey of white native-born males, age 21-64, in the Detroit area, carried out by the Detroit Area Study in 1966.

This is by no means to say that the theoretical problems of urban social structure have now been solved. On the contrary, *Bonds of Pluralism* raises 10 questions for every answer it provides. For precisely this reason, it has special value as a set of working papers: it is not the culmination of a line of inquiry but rather a promising beginning. It is the kind of work which can be built upon by others and which invites the cumulation of further knowledge in several ways.

For one thing, the empirical studies do not begin to exhaust the theory. Friendship choices, for example, represent only a beginning to the study of networks of intimacy, and even networks of intimacy may represent only a beginning to the study of the social networks of which urban social structure consist. Yet Laumann has analyzed friendship choices thoroughly enough that one has some idea where to go next in extending his work.

For another, many of the results reported in this book are most unsatisfying for the urban sociologist. These friendship choices, for example, which we are regarding as the sinew of intimate social structure, often appear to

be the most superficial of social relationships. Laumann finds that less than half the friendship choices are reciprocated and that an urban man agrees with his friends' attitudes on social issues approximately as often as he agrees with those of a random member of the community. Is this situation the basis for informal adult socialization and social control in the metropolitan community? Perhaps the urban structure of intimacy is to be found in different relationships. Or perhaps this is all there is! What has intimacy to do with the substance of urban social structure? In short, Laumann's findings, being unsatisfying, pose theoretically significant problems.

Georg Simmel on Individuality and Social Forms. Edited and with an introduction by Donald N. Levine. Chicago: University of Chicago Press, 1971. Pp. lxx+395. \$12.50 (cloth); \$4.25 (paper).

Everett K. Wilson

University of North Carolina

The beauty of Simmel lies in the eye of the beholder—which is itself good Simmel doctrine. That is, reading Simmel is like reading Mead, Dewey, and Bentley, where the forms of human interchange are shaped by both parties: reader as well as writer creates the intellectual outcome. Levine offers this recreation to old Simmel hands, and to the novice he offers the best single volume for introducing a seminal source of social theory.

We've had Simmel before, from Albion Small through Spykman to Wolff. Why another? The answer hinges on the quality of an editor's introduction, on access to writings hitherto unavailable, and on the disclosure of quintessential Simmel in a framework that articulates sensibly with common sociological categories. In this case Levine's introduction is lucid and illuminating. Newly accessible material is sparse. And the selection and organization of Simmel's work is first rate.

The introduction is useful. The novice assuredly needs a helping hand with Simmel, whose language is difficult, sometimes even in translation. There are two other difficulties: the wealth of allusions from various fields, figure-woven into the argument; and the problem of seeing Simmel as a whole—or sufficiently so that the wealth of illustration and the play of contraries are joined to the central themes that unify his work.

Levine's introduction sketches Simmel's contributions: for example, developmental parallels between realms of culture, personality, and society (and the interplay between the social and the cultural). His analysis of Simmel's mode of inquiry and its underlying assumptions is helpful. He naturally deals with Simmel's research directive: identify and explain the forms of interaction, for these constitute the proper preoccupation of sociology. Levine explains the dualisms, Simmel's love of paradox and polarities in dissecting patterns of interaction. In no other theoretician does one find such a good example of Ferguson's sagacious person, who perceives similarities in the unlike and discrepancies in what the vulgar see as similar.

In this process we find specification, an aspect of Simmel's methodology not noted in Levine's comments. Simmel's propensity to look at the other side of the coin compels him to state the conditions under which a contrary form of interaction will emerge. Thus "the common submission to a ruling power by no means always leads to unification but, if the submission occurs under certain conditions, to the very opposite of it" (p. 105). Simmel also establishes a specification of conditions-under-which that leads him to propose a curvilinear relationship between oppressive domination and the response of group members: up to a point, resistance to the leader increases; beyond that point, internal dissensions attenuate group-leader antipathies.

There is another buried methodological presupposition in Simmel's stress on the reciprocity of human relations. We think of him as relying on empathetic understanding of human impulse, regularly invoking psychic states for understanding. But reciprocity requires a category of others who share, sometimes determinatively, in defining the actor's role. Thus there is an intimation of the behavioral persuasion in Simmel's discussion of the poor. While poverty may entail psychic states such as ambition or resignation, as a social fact it depends on others' reactions to deprivation: "Only when society . . . reacts toward him with assistance, only then does he [the poor person] play his specific role" (p. 176). The reaction gives meaning to the act.

The final pages of the introduction explore Simmel's complex image of man and his impact on fellow sociologists: Durkheim, Weber, and von Wiese in Europe; Small and Park in the United States; and, more recently, his legacy extended in the empirical work of Mills, Caplow, Bales, Merton, Goffman, Coser, and Homans.

Concerning the organization of the book: a novice approaching Simmel needs selection and synthesis. It is superficial to characterize him as an intellectual window-shopper. Elements of a lifetime's thought are not arranged by a table of random numbers. Peter Etzkorn, Murray Davis in recent studies, and Levine in this work tease out unifying themes.

Levine's synthesis and arrangement of Simmel's ideas is sensible, which can be seen by the fact that the table of contents writes itself into a coherent statement. The task of sociology, it says, is to identify and analyze forms of social interaction which, in their recurrence, define social types such as the stranger, the miser and spendthrift, the adventurer, the poor. But typified social interaction does not exhaust social forms. Permutations of human experience yield deviant patterns: forms of individuality. Such forms are always at war with the preexisting culture and the social structure that embodies that culture. Through this dialectic (life processes generating individuality that confronts the formal typification of human roles) social change emerges. The statement makes sense, and its elaboration makes the book.

I conclude by recommending a cursory reading of the "Philosophy of the Social Sciences," and several rereadings of the essays on conflict and exchange, especially the latter, since Coser has so fully presented and

explained the former. This essay (on which Blau draws extensively in *Exchange and Power*) should be compared with Homans's statements for depth and grasp of microprocesses. ("Value arises in . . . the interval which obstacles, renunciations, and sacrifices interpose between desire and its satisfaction") (p. 57). Desire, operationally defined in the exchange process by sacrifice, conditions both scarcity and exchange value.

Examples of crystallized forms of interaction (social types) deserve scanning, while the social psychology of part 5, "Individuality and Social Structure," warrants close attention. Here the important discussions are those of "Group Expansion and the Development of Individuality," the familiar essay on the metropolis, and the telling statement on "Subordination and Personal Fulfillment." The final section exploits person-group tensions in the service of understanding social change.

Simmel merits repeated reading. Levine's book provides a good context for doing so.

Ferdinand Toennies on Sociology: Pure, Applied, and Empirical. Edited by Werner J. Cahnman and Rudolf Heberle. Chicago: University of Chicago Press, 1971. Pp. vii+351. \$13.50.

J. Allan Beegle

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The sociology of Toennies, presented in the writings selected for this book, is a most welcome addition to the Heritage of Sociology series under the general editorship of Morris Janowitz. The book contains some 26 selections from the 600 or so items in the Toennies bibliography (p. xviii); all except three have never before appeared in English. Giving the collection the character of a "reader," the editors appraise Toennies as a scholar and human being and provide invaluable connective commentary. They in fact plan a second Toennies volume, because of "the wide range of Toennies work and the sheer extension of it, in combination with the difficulties of interpretation and the fact that very little thus far has been available to English-speaking readers" (p. vii).

The tedious job of translating the selections was a cooperative (shall we say *Gemeinschaft*?) effort on the part of seven scholars, including the editors. Carola Toennies Atkinson, with the assistance of Rudolf Heberle, translated chapters from the *Introduction to Sociology*; and Franziska Toennies Heberle "provided the personal touch in our on-going conversations as well as the index" (p. xxii). While the editors usually have not translated the words *Gemeinschaft* and *Gesellschaft*, they have occasionally rendered *Gesellschaft* as "association" rather than "society." They have also chosen to translate the psychic correlates of *Gemeinschaft* and *Gesellschaft* as "essential will" and "arbitrary will" rather than as the familiar "natural will" and "rational will" used by Charles P. Loomis (*Fundamental Concepts of Sociology: Gemeinschaft und Gesellschaft* [New York:

American Book Co., 1940]; this book is now available as a Harper Torchbook titled *Community and Society*).

The selections in *Ferdinand Toennies on Sociology* are arranged in five major sections: "Formation of Concepts," "Elaboration of Concepts," "Pure Sociology," "Empirical Sociology," and "Applied Sociology." The first section includes the first and second prefaces to *Gemeinschaft und Gesellschaft*, written in 1887 and 1912, respectively, when Toennies was relatively young, as well as an intellectual-autobiographical piece published in 1932, when he was an aged man. In addition, this section contains two passages from Toennies's book on Hobbes as well as two selections from *Gemeinschaft und Gesellschaft*.

The second major section contains selections of Toennies's writings that elaborate and clarify the theoretical position and concepts found in *Gemeinschaft und Gesellschaft*. This section includes the so-called Saint Louis paper of 1904, delivered at the Congress of Arts and Science, entitled "The Present Concept of Social Structure." On the same program were Gustav Ratzenhofer and Lester F. Ward. The paper presented by Toennies later appeared in the *American Journal of Sociology* (1905) and hence is among the selections previously appearing in English. The second section also includes the Naples paper, a succinct statement dealing with the taxonomy of general and special sociology.

Section 3 contains some seven excerpts from Toennies's *Einführung in die Soziologie*, first published in 1931. In these selections we find Toennies dealing with such subjects as social relations, collectives, social values, norms, and social structures.

The section on "Empirical Sociology" is made up of two selections—a very brief statement on statistics as a method or as a special social science, and an example of Toennies's empirical work. The latter deals with convicted criminals in Schleswig-Holstein. After Toennies differentiates among types of crime, incidence rates are derived for natives and nonnatives, for urban-born by size of city, and for rural-born by size of village.

The final section contains selections on public opinion, the emergence of industrial society, and the course of modern history. Included here is Toennies's last publication, "The Active Forces of Social Development in the Modern Age," published in 1935. Those interested in social change, particularly the consequences for rural areas, will find this paper filled with insights.

This book should greatly enhance the exposure of sociologists, particularly American sociologists, to the sociology of Toennies, not only because of the newly translated selections but also because of the effective organization and notes which the editors supply. Those familiar only with the Loomis translation will find the selections detailing the intellectual roots of Toennies's thought and the elaboration concepts especially enlightening. While this book is rich in selections explicating the derivation and meaning of typologies associated with Toennies, lingering doubts that Toennies thought of *Gemeinschaft* as "good" and *Gesellschaft* as "bad" may well remain. Finally, the large question of the relationship between Toennies's

work and that of Durkheim, Marx, and Weber, in particular, apparently has been relegated to another volume. It is not evident why the editors have omitted such a crucial essay from this volume, despite plans for a second volume. Nonetheless, the book stands as a major contribution in providing new material on Toennies's sociology in English, in helping to appraise his scholarly contributions in the development of sociology, and in clarifying the meaning of Toennies's system and concepts.

The Urban Wilderness: A History of the American City. By Sam Bass Warner, Jr. New York: Harper & Row, 1972. Pp. xvii+303. \$12.50.

Morris J. Vogel

University of Chicago

Recent years have witnessed an outpouring of historical writing about cities, since Americans have rediscovered the city as a significant organizing concept for the national experience. Sam Bass Warner has participated in this activity with *Streetcar Suburbs: The Process of Urban Growth in Boston, 1870-1900* (Boston: Harvard University Press, 1962), a monograph on the process of urbanization. With a mass of information now available and an increasing number of courses in American urban history in universities, scholars are building on monographs and creating a larger organizational scheme in which to deal with the history of the city. Warner has participated here too, with "If All the World Were Philadelphia: A Scaffolding for Urban History, 1774-1930" (*American Historical Review*, vol. 74 [October 1968]) and *The Private City: Philadelphia in Three Periods of Its Growth* (Philadelphia: University of Pennsylvania Press, 1968), tentative essays constructed about a single city. Now Warner has attempted a major synthesis of American urban history.

The book is a polemic, part complaint and part program. Warner details the forces that have shaped the urban environment: private property, the market economy's subservience to the profit motive, and inequality. The land tenure system developed in the colonial and early national period, which once expanded and assured civil liberties, assured in later years that cities would not have the flexibility to cope with change. In an example, Warner deals perceptively with the deterioration of neighborhoods once meant for the middle classes. To maximize profits, developers utilized "pinched and inflexible land divisions" (p. 206), crowding all lots with houses. Such areas could be comfortable as long as their residents remained middle class. Neighbors had to have incomes and interests that united them in the upkeep of their small front yards and the area's other limited enhancing features. The land use pattern could not cope with the automobile, however, and with its introduction streets and yards became crowded and the neighborhood degenerated esthetically. But the auto did offer original residents the opportunity to move elsewhere. They were replaced by poorer residents, whose multiple occupancy severely cramped

the area; this, together with their limited resources and lack of political influence, speeded the decline of the neighborhood's amenities and services. A fated ugliness resulted, and the poor were left to assume the social costs that never entered into the developer's calculations. Warner argues that part of the problem was that builders did not provide for the possibility of change; with change as a constant reality in urban America, that oversight proved ruinous.

The Urban Wilderness presents a history of urban change, adopting a generally accepted periodization and employing different cities to epitomize historic periods. It parallels the development of the physical and social fabric of the city with the predominant form of social organization, evaluating the quality of life in each period.

New York from 1820 to 1870 was the living city of mixed land uses and generally mixed neighborhoods. Geography only minimally divided rich from poor, and primitive mechanization and small-scale industry did the same with laborer and capitalist. Chicago from 1870 to 1920 typified the city of mass transit and "The Segregated City." Mass transit permitted highly differentiated land use and led to strict social and racial segregation. The corporate organization of large-scale industry separated employer from employee as suburbanization divided those with resources from those without. As rigid as its subway and elevated lines, the city imprisoned its poor in crowded central areas from which they were unable to escape because they could not afford the high cost of land along transit routes or of the transportation itself. Los Angeles from 1920 on represents the decentralized or automobile city. With automobiles, individuals can freely choose their residence and employment. Spatial freedom results in social and economic freedom, to which a decentralized corporate and service economy also contributes.

Many urban historians favor the mass transit city, perhaps out of nostalgia for the period of their researches. But for Warner the city of 1870-1920 is the segregated city, in which the bottom third of the population was exploited and severely restricted in the range of its occupational and residential choices. Warner has been influenced strongly by Los Angeles. He finds the automobile city attractive because its flexibility broadens the potential range of personal choices and freedoms. Its basic structure presents all urban dwellers with the possibility of participating equally in modern society. Warner believes that there is no need to revitalize or extend mass transit. Efforts in this direction constitute a raid on the public treasury by the middle classes for their own benefit. While he does not write off the central city as an idea whose time has passed, Warner sees much of its revitalization as designed to serve the well-being of the fortunate, an attempt to protect downtown investments and businesses. Given the existing commitment to highways, he feels public policy should assure each individual an automobile, thus providing all with the key to freedoms and rights in our society.

Warner's regard for the automobile is remarkably like that of earlier liberal theorists for private land ownership; one guarantees the freedom

to participate fully in society, the other insures civil liberties. Perhaps the latter policy will be as constricting and debilitating to our cities as it is argued the former has been. Warner's answer is in part based on an expanding technology; he expects a city based on automobiles to be viable because the autos will be nonpolluting. But available technology can contract as well as expand. An energy crisis might force suburbanites to limit or give up their auto commuting habits. Neighborhoods built around the automobile might be no better able to withstand the restriction of automobile use than the streetcar suburbs (whose deterioration Warner so elegantly describes) were able to cope with the automobile's introduction.

One of the major points that *The Urban Wilderness* seeks to make is that "to plan without regard for the processes of change is inevitably to fail" (p. 14). Cities have long been left to cope with yesterday's values and errors. A strength of this book is that, though at times overstating, Warner describes and analyzes this process historically. As a program, however, it is less successful.

Social Conflict and Social Movements. By Anthony Oberschall. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1973. Pp. xi+371. \$9.95.

S. Frank Miyamoto

University of Washington

There is currently an understandable surge of interest in studies of conflict and social movements. Anthony Oberschall's book is one of the first efforts, arising from this renewed interest, to present a systematic account of social movements. I feel the effort is not altogether successful, but the book also has its points of merit.

The study is restricted to those forms of social conflict which most commonly lead to protest movements: class, racial, and communal conflicts, rebellions, riots, social disturbances, and the like. The theoretical discussion opens with a chapter on the sources of social conflict. I found this discussion poorly integrated and weak; the author contends that much has already been written on the causes of discontent, so that he wishes to place his emphases elsewhere. He is especially concerned with developing a sociological theory of mobilization, and for this purpose he draws upon Mancur Olson's theory of collective action.

The connection between the present work and Olson's theory seems loose and vague, but Oberschall's underlying theme, that rational self-interest is a basic determinant of behavior in social movements, clearly squares with the Olson model. This theme is reinforced by occasional references to a game theory mode of analysis, and is highlighted by an explicit rejection of William Kornhauser's mass society theory of political behavior and of contagion theory notions of collective behavior.

Oberschall's theory of mobilization can be reduced to the following propositions: mobilization for conflict requires two conditions within the

collectivity, a minimum consensus on hostility objects and basic grievances, and a preexisting basis of organization. With respect to the latter, societies are typed by their horizontal and vertical dimensions of organization. On the horizontal dimension, societies are predominantly communally organized, associationally organized (an abundance of voluntary associations), or lacking either organizational base. On the vertical dimension, integrated societies in which elites have many ties with nonelites are distinguished from segmented societies in which such ties are relatively weak. The two-times-three cross-classification of these two dimensions yields six possible conflict settings. Oberschall further assumes that the mobilization of conflict groups is facilitated in segmented societies and inhibited in integrated types. The form of conflict group organization, on the other hand, is determined by the horizontal condition. From these assumptions, a series of hypotheses are drawn regarding the varying potential for conflict mobilization of different forms, which occurs in different situations. A separate chapter on leader-follower mobilization develops the view that participation is determined by calculations of risks and rewards, conditioned by the available resources and by the nature of the society within which mobilization occurs.

The three remaining theoretical chapters are concerned with social control and conflict regulation, confrontation, and group violence. The general principle that the weakening of social controls foreshadows the emergence of dissident movements is recognized, and the reactive consequences which follow the employment of different control measures are also analyzed. Confrontation, which occurs when polarization defines the contending situation between two parties, refers to both the within-group and between-group processes which follow as the opposing parties seek to negotiate differences. Group violence is interpreted as the product of conflict escalation that results usually from the failure of those in power to take heed of the grievances of the oppressed, and as most often initiated by the power group in attempts to suppress the dissenters through coercive tactics. Since generalizations bearing on each topic are drawn from a variety of sources, the discussion in these chapters tends to be eclectic; however, it does include the underlying theme, that behavior is guided by cost-reward principles. A chapter on the Nigerian Civil War and another on the civil rights movement in the United States are used to illustrate the theory.

This has proved a most difficult book to review fairly. The author has obviously devoted a great amount of work and thought to assessing the evidence bearing on the complex phenomena involved in social conflict and social movements. His theorizing yields some interesting suggestions. And he has attempted a systematic treatise in a difficult field where documentary material is abundant but previous systematization has been scarce. Having read the book, however, I have no clear sense of understanding social movements better than I did before. The difficulty is in part organizational—subjects often are not treated in an orderly sequence, and secondary issues often are given as much space as primary ones. Further-

more, the work lacks an overarching orientation; at least, the parts are not well integrated. The author uses an exchange theory orientation but gives evidence of not having worked out its implications systematically. And I personally disagree with his slighting of collective behavior theories. Still, this is a book which deserves study by students of social movements.

On Purposeful Systems: An Interdisciplinary Analysis of Individual and Social Behavior as a System of Purposeful Events. By Russell L. Ackoff and Fred E. Emery. Chicago: Aldine-Atherton, Inc., 1972. Pp. ix+288. \$12.95.

Robert Boguslaw

Washington University

There is only one way to begin a review of a book entitled *On Purposeful Systems*. One asks: what is the purpose of the authors? where do they stand?

This raises a problem of teleology and mechanism which is at the heart of the work itself. The authors make it very clear that they are not presenting a theory of behavior or a set of generalizations about why people behave as they do. They are not trying to describe behavior or to mathematize or formalize the study of human behavior. Their stated purpose is to provide "a way of looking at human behavior as systems of purposeful (teleological) events."

One might well raise a question about the adequacy of this as a complete statement of purpose. In the preface we learn that the idea arose in 1941 when "then Associate Professor Thomas A. Cowan of the Department of Philosophy of the University of Pennsylvania suggested to Instructor C. West Churchman and Assistant Instructor Russell L. Ackoff that they try to extend the work of E. S. Singer, Jr., the grand old man of the department who had befriended and taught all three. Singer had developed a conceptual system designed to show the compatibility of teleology and mechanism and carried his work to the edges of psychology. . . . Cowan urged Churchman and Ackoff to carry this work deeply into the behavioral sciences."

Late in 1967 Ackoff began collaborating with Fred E. Emery, then at the Tavistock Institute, London. They applied some of the conceptual system that had been developed to explain patterns of drinking behavior Emery had discovered in his studies in Europe. Emery also brought to the collaboration, we are told, his long concern with open systems in general and sociotechnical systems in particular, as well as a deep appreciation of the work of G. Somerhoff.

We are not given answers to such questions as the following. What was the original purpose of the proposal made by Associate Professor Thomas A. Cowan? What were the original purposes of Instructor Churchman and Assistant Instructor Ackoff in agreeing to the proposal? Have these pur-

poses been modified in the "over thirty years this book has been in the making"? What about Emery? How did drinking behavior become involved? More generally, do we require answers to questions such as these to deal effectively with the problem of purpose as related to the design of this book?

The questions are not meant to be frivolous; this is an important book and deserves serious attention.

The authors address themselves to the task of developing an "objective teleology." Aristotle, they recall, used teleological concepts to explain why inanimate as well as animate things behaved as they did. Contemporary social scientists who try to explain human behavior by invoking such concepts as beliefs, attitudes, traits, and drives are carrying on in his spirit. They employ a "subjective" teleology. In an "objective" teleology, beliefs, attitudes, and traits are attributed to an individual because of what he does. The properties are derived from perceived regularities of behavior under varied but specified conditions. The concepts do not lie behind behavior; they lie *in* behavior. They are not treated as intervening variables but are objectively derived from observations. "An objective teleology is in no way constrained to the study of subjective purposiveness. . . . Introspection is not required."

In order to open the study of the mind's inner workings to public examination, it is necessary to provide *idealized operational definitions* and measures of functional concepts (e.g., length, density, and energy in physics). The authors feel that an objective teleology expressed in the form of a conceptual system can serve as a foundation for significant research into such phenomena as choice, communication, conflict, and other types of social interaction. Their idealized operational definitions are intended to demonstrate general and rich hypotheses about human behavior. These definitions are seen as providing a basis for designing adequate tests for the hypotheses and for relating test results to each other, since the definitions form a network of concepts.

The hope is expressed that this effort will facilitate consideration of behavioral variables in the evaluation and design of social systems, including those involving machines as well as men. The authors very properly point out that the models of such systems currently used in systems science, management science, operations research, and systems engineering almost invariably treat even their behavioral variables ateleologically. They insist it is not enough to predict behavior; we must explain it.

Much of the book is devoted to the development of idealized operational definitions and related measures. In the course of this effort the authors display a keen and versatile analytic sense. The volume is sprinkled with little bits of wisdom. Clearly two first-rate minds are at work.

However, there seems to be much more mechanism here than teleology. Compared to prevailing thrusts in the literature of operations research and systems analysis—even those addressed to "social systems"—this represents a giant step forward. But the authors seem to have enormous difficulties in escaping the gravitational pull of ateleological thinking and

notation. If the manifestations of this problem were confined to the use of a superficially impressive but intellectually shallow symbology, and a format reminiscent of engineering or physics texts, one might dismiss it all simply as idiosyncracies of innovative behavior—as a form of poetic license. But one suspects a more basic infirmity is involved.

For Ackoff and Emery, the *goal* of a system is a preferred outcome that can be obtained within a specified period of time. An *objective* of a system is a preferred outcome that can be obtained over a longer time period. A *purposeful* system is one that can change its goal even under constant environmental conditions; it selects goals as well as the means to pursue them. Human beings are the most familiar examples of such systems. An *ideal-seeking* system is a purposeful system of a special kind. "The capability of seeking ideals may well be a characteristic that distinguishes man from anything he can make including computers."

The ideal which, if achieved, would make it possible to achieve all other conceivable ideals is omnipotence. This, then, is the ideal which the authors wish ultimately to understand and toward which they presumably aspire on behalf of social systems.

I, for one, have little difficulty in conceiving of a computer which has been fed the ideal of omnipotence. Every computer I have ever known seems to suffer from delusions of omnipotence as part of a deep-seated character disorder. And I am not at all prepared to settle for an ideal which is fundamentally little more than a better mousetrap.

Perhaps this ideal of ideals is all too ambitious for some of us. Perhaps we have become somewhat surfeited with historical and contemporary paranoia and with dreams of omnipotence. Perhaps we would be eternally content with ideals falling considerably short of the one that makes it possible to achieve all other ideals. Must we be gods before we can become human beings? When the search for purpose is embedded in a matrix of "objectivity," is it inevitable that even ideals must assume an instrumental quality? To what extent is the latent, perhaps unconscious, purpose of nominally objective teleologies to develop more effective instrumentalities for repressing rather than freeing the human spirit? Perhaps we must be more direct and more complete about the matter of purpose. Perhaps teleology can become objective only when we are willing and able to answer the questions: whose purposes are you fulfilling? where do you stand?

Techniques and Problems of Theory Construction in Sociology. By Jerald Hage. New York: John Wiley & Sons, 1972. Pp. xiii+239. \$11.50.

Jack P. Gibbs

University of Arizona

Hage's book is the seventh on theory construction published by an American sociologist since 1965. Each sets forth a distinctive scheme for the formulation of theories, but each differs as to the intended audience.

Hage evidently wrote primarily for students, and that focus dominates his strategy.

A mode of theory construction is essentially a logic of formalization, and hence one cannot use it without substantive ideas (e.g., concepts, empirical propositions). Since a logic of formalization provides no directions for arriving at substantive ideas, students are likely to be frustrated by it, as though they have been given a road map but not a car. Hage attempts to arm students with both.

Writing in a "how to do it" style, Hage speaks of techniques and strategies in describing such endeavors as (1) searching (Hage's favorite word) for new theoretical concepts, (2) searching for "general variables," (3) reducing variables to a basic dimension, (4) converting ideal-type constructs into "continuous statements," (5) combining continuous statements to create new ones, (6) searching for theoretical definitions, (7) searching for operational indices, and (8) cross-classifying primitive terms. This list reflects Hage's terminology and only indicates the prescribed pursuits.

Given such a variety of techniques in a book of this length, the treatment is bound to be skimpy at various points. Certain techniques (e.g., comparing diverse case studies) are described in some detail, but many students are not likely to understand the others. The frequent but brief references to the classical theorists (Marx, Durkheim, Simmel, and Weber) have limited illustrative utility for students, and even professionals will be startled by the brevity of Hage's directions (less than two pages) for synthesizing theories.

Some sociologists are likely to view Hage's work as a gross oversimplification for reasons other than its skimpiness. There is a more general issue, best articulated by questions. Is there or can there be a logic of discovery? Is it realistic to attempt the inculcation of imagination? Is it wise even to suggest that theories can be somehow manufactured? (If I knew how, I wouldn't tell a soul!) Hage would balk at the idea that theories can be manufactured; nonetheless, his style of writing, complete with sentences which would stun Norman Vincent Peale, creates that impression (p. 19): "Variables can almost leap from the page once we have trained ourselves to look for them."

If such sentences and the overall "how to do it" tone lead professionals to hoot at Hage's enterprise, I would not join in the derision. The issue is not simply that students want something more than a logic of formalization. Early in their training they should come to recognize that "doing theory" need not be an esoteric art restricted to an anointed few, which is precisely the impression created by the sociological literature and even by a conventional curriculum. Contemplate the absurdity of restricting courses on theory construction to graduate students, or even having a course on "theory." What are we teaching in other courses? Both practices reflect the tradition of equating "theory" with the works of stuffed Europeans. The activities prescribed by Hage could lead students to recognize that a 19-century birth date is not essential for theorizing.

All would be well if the book could be described as possibly beneficial to students and, at worst, harmless. But it is not quite harmless. Hage joins several other previous writers on theory construction in displaying an astonishing indifference to issues. Thus, he uses the terms "causation," "explanation," and "validity" uncritically; and students will not realize that such notions are most controversial.

Returning to my analogy of motion and direction, Hage supplies the student with a crude and confusing road map. He rightly identifies terms as the atoms of a theory but then plunges into an invidious comparison of "nonvariables" and "general variables." The tone suggests that nonvariables are to be banished from sociology and to that end Hage gives directions for "converting nonvariables into general variables." Using one of his examples, perhaps Durkheim's mechanical-organic distinction can be converted into 13 general variables (e.g., population size). It is pointless, however, to suggest that so-called nonvariables have no place in theories. Hage's examples indicate that units (e.g., communities, societies) are instances, and he later acknowledges such terms. So the scheme blurs the distinction between terms that denote units and terms that denote unit properties (e.g., density).

Hage's typology of terms is incomplete because he evidently presumes that the word "concept" is sufficient to identify all constituent terms of a theory. But, again, what of the distinction between units and properties of units? Then consider identifying both "population density" and "instrumental integration" as concepts, with no further analytical distinction, even though the latter term virtually defies a complete definition and the former can be (1) defined completely and (2) linked to a conventional formula. Hage's determination to make do with one type of term is further manifested by his dismissal of "constructs" (p. 44) as "a set of concepts, most of which are usually nonvariables."

The scheme also falls short regarding less abstract types of terms. Hage stresses that a theory is not testable without operational definitions and formulas, but neither one can be construed as a *term* in a theoretical statement. Finally, the expressions "greater . . . greater" and "greater . . . less" are used to denote relations, but those expressions are surely distinct from concepts.

What has been said of Hage's typology of terms applies also to his implicit typology of theoretical statements. He acknowledges axioms, postulates, theorems, and corollaries (p. 35), but only to turn away from them: "there are so many names, it seems best to avoid using any of them and therefore the preference for the neutral term theoretical statement." However, "theoretical statement" is a generic designation, and Hage subsequently does use terms that denote types of statements (e.g., hypothesis) but without specifying the position of each in a theory. In his examples of theories some component statements are identified as premises, but it is difficult to see how a premise differs from a statement that stipulates a "theoretical linkage or rationale," especially since the latter type of statement does not appear in all examples of a theory.

Given the shortcomings of Hage's typology of terms and statements, the logical structure of the scheme is obscure. Moreover, Hage stipulates no rules for derivation or deduction, even though his examples suggest that a theorist moves from premises to equations. One must surely wonder how equations can be derived from verbal premises without explicit rules, and if they cannot be so derived, one must wonder even more how tests of the equations have a bearing on the premises. In that connection, one of Hage's statements (p. 145) is baffling: "The equations are not logically derived as much as they are implied in the premises."

Turning to the organization of the book: delaying a definition of a theory until the next-to-last chapter is scarcely defensible. In the last chapter Hage succumbs to the grand tradition and offers a brief survey of theoretical perspectives (e.g., structural functionalism). That subject is treated more extensively in numerous books on sociological theory, and the chapter could have been used better to clarify the scheme.

It hardly needs to be said that much of the foregoing carping reflects my predilections. Moreover, despite the criticisms, the book can be used effectively in some sociology courses, provided that instructors are willing to elaborate on Hage's illustrations and direct the students' attention to issues in theory construction.

Elementary Survey Analysis. By James A. Davis. 3d ed. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971. Pp. viii+195. \$3.95 (paper).

Donald R. Ploch

National Science Foundation and Yale University

This is a frustrating book to review. On the whole it is a valuable addition to our teaching tools, but it is flawed in part by minor typos (e.g., S for X, p. 70; A*B for A*D, p. 46) and in part by its overall scheme. Therefore one should evaluate it with care before adopting it for a text. (The author has indicated that most of the typos were corrected in the fourth edition.)

In a breezy but powerful style, Davis invites the student to try the analysis of variables. He is introduced to Yule's *Q* to analyze two-by-two tables and is asked to stay with this measure, both for parsimony (too many statistics can spoil the mind) and because the logic of analysis doesn't change with the statistic. Having mastered two variables, we are introduced to a third, which sets up the Lazardsfeldian glossary—suppressors, specification, etc.—as well as causal sequences. Having mastered three variables, we encounter a fourth; and then, in the final chapter, we are warned about "(Too) Many Variables."

From the beginning of the book statistics are used both for description and for inference, which is tied to confidence limits rather than significance tests. Introduction of the third variable gives Davis a chance to explore partial and conditional *Q*'s and to exhaust all the connections between zero-order, partial, and multiple statistics. This is done with skill and

verve. It is a compelling and lucid treatment. The causal analysis is a little tendentious and will take longer to lead a student through.

For one minor and two major reasons, I would use the book for supplementary readings but would not use it as a text. There are minor technical inaccuracies; for example, the diagram for confidence limits for Q (p. 55) is misleading unless one substitutes asymptotic limits (based on sample values of Q) for true limits (based on population values). When population $Q = 1.0$, then all sample Q 's $= 1.0$. But if the sample $Q = 1.0$, then the population Q can range as follows: $1.0 \geq Q > -1.0$. At 95%, even with N of 1,832, confidence limits will be skewed as sample Q approaches $|1.0|$. Minor inaccuracies such as this are misleading and must be unlearned as the reader becomes more sophisticated.

The book begins and ends in data analysis, surely a plus, with very little theory, surely a minus. In order to demonstrate the utility of partials, all possible relationships are examined. One never encounters the prior question: which of these relationships should be examined? A student processed through this book will be an efficient numerical analyst. Why certain relationships rather than others should be analyzed will remain beyond his ken.

Finally, the introduction of the third and fourth variables subtly shifts the attention of the book in a way that is not made clear. The third variable is a test variable. From that point on one is trying to decompose a presumed satisfactory two-variable relationship. The logic is that of covariance rather than multiple regression, but the introduction of causal schemes tends to blur the point. If one knows the logic of the general linear model, one can interpret the text, can see how the analytical logic develops. It is more difficult to go from the text to the general linear model.

The book is a well-paced and lively introduction to the analysis of quantitative data. But its flaws, though few, may tend to make future learning difficult. It can be a valuable supplementary text in an introductory course.

Not by the Color of Their Skin: The Impact of Racial Differences on the Child's Development. By Marjorie McDonald. New York: International Universities Press, 1970. Pp. xiv+242. \$7.50.

Black and White Identity Formation: Studies in the Psychosocial Development for Lower Socioeconomic Class Adolescent Boys. By Stuart T. Hauser. New York: Wiley Interscience, 1971. Pp. xv+160. \$9.95.

James M. Jones

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When you get right down to it, it's really a matter of your methodology and your point of view. Hard-nosed psychologists say "race is not a variable." Why? Because you cannot manipulate race in an experimental sense; you can only manipulate the environment so that certain psycho-

logical experiences that are salient to members of one race can be experienced by others. After this is done (if it can be done), it is expected that the full range of phenomena attributed to race can be recreated independent of people who are phenotypically assigned to that race. Sociologists treat race in a very straightforward manner as a significant social fact that influences the social system in predictable ways. Numbers become important here, because race becomes a concatenation of digits, percentages, and probabilities. On the other side we find soft-headed psychologists (read psychoanalysts) who avoid numbers and eschew (experimental) manipulation. They prefer myth, magic, insight, and creative analysis. Thus, in sum, black is a color (not a variable, a statistic, a symbol, an object, a social grouping of humans), is beautiful, is ugly and degrading, is irrelevant and critically important. Again, it depends on your methodology and your point of view.

It is the psychoanalytic perspective that holds our attention as we contemplate Color (black) in American society (white). The two books here reviewed create a dramatic contrast in method and perspective. Both are based on psychoanalytic psychology, and both are concerned with society and the social problem of race relations. Let's go first to Cleveland to see what problems confront the Hannah Perkins School for emotionally disturbed kids.

Marjorie McDonald is a psychoanalytically trained consultant, staff member, and observer at the Hannah Perkins School. The school is a therapeutic nursery and kindergarten for children with psychological difficulties. There were 15 children in each class, which met daily from 9:00 to 2:30. Mothers had weekly meetings with analytically trained therapists. McDonald has collected her observations and analyses over a three-year period from 1965 to 1968, and records them in her book which she calls "a psychoanalytic book about skin color and personality development, and about object relationships and racial integration" (p. vii).

A few years ago the school decided to "do something to improve its racial integration" (p. 3). The result has been a maximum of 25% racial integration of children, but "our teaching staff has remained all white and our kitchen and housecleaning staff has remained mostly Negro" (p. 15). It is clear from the beginning that color is the focus of much of the activity of this school. Indeed, the perspective of this book is well reflected in the following passage: "Today it is especially his full identity as a human being for which the Negro is fighting. He does not want to be regarded . . . as a black object [but] . . . as a worthwhile human being whose skin is dark. . . . The frightened white person . . . feels the Negro to be a threat to his identity as a human being with light skin; he fears being demoted to a white object" (p. 96).

It's not just color but skin itself that is critical to this analysis. "The skin is an organ of great significance to personality development . . . [it] is strongly invested with [his] drive to live and develop [and] act as a major organizer of this driving force" (p. 97). To bring this study into sharp focus, we find that "it is especially the difference in skin color

which is at the root of racial prejudice. Other distinguishing features of racial differences, although important, are secondary" (p. 95). Thus, skin color is critical to identity formation and, because of this central position, is the mediator of interracial relations. The core problem of racial integration, then, is to handle the shock and threat posed by anxiety over skin color differences.

These passages comprise the major theoretical stance taken by McDonald, as extracted from examples from her therapeutic/observational role at the Hannah Perkins School. After an introductory chapter, she details the staff meetings at which so much of the discussion and sharing of events and episodes took place. She then provides us with extensive accounts of the color problems the children faced and provides details about how the problems were handled. These encounters make fascinating reading, but it seems clear that their significance depends on the point of view of the individual reader. For example, mothers of black children sometimes found the focus on skin color excessive. The staff interpreted this objection as reluctance to face the reality of the central importance of skin color. There is not sufficient space to analyze the numerous examples of childhood interaction and staff analysis. The accounts are clearly selected to present those "color" cases in detail, and the analytic interpretations are offered unabashedly.

The theoretical section from which I have quoted liberally above is intriguing, but, for someone with my point of view, difficult to take seriously. The literature review which closes the book is selective but representative, and highlights the essential arguments in a more familiar terminology. The message of the book is very simple. Skin color is significant in this society and in the lives of individuals. We should recognize that fact and try to deal with it head on. I can only answer—okay.

Let's move on to New Haven, Connecticut, where Stuart Hauser spent four years studying *Black and White Identity Formation*. Hauser's basic approach is psychoanalytic ego psychology, and he draws heavily on Erik Erikson's work about identity formation. The stated intention of this research is "an exploration of identity development in varied sociocultural contexts" (p. 6).

Eleven white and 11 Negro boys aged 14–16 from lower-class families were interviewed and tested over a four-year period. At the initial interview, open-ended questions elicited verbal self-descriptions which were abstracted in positive form and typed on three-by-five cards. The total set of cards became each subject's "deck," and items were added, but never deleted, on the basis of interviews in subsequent years. Q-sorts of these decks arranged according to a 10-point scale of importance, were made in the fall of each subject's sophomore year in high school, and in June of each subsequent year. On each occasion eight sorts were made according to the following instructions: "How am I now; How I would be if I were a perfect son to my mother; How I appear in the eyes of my friends; How I will be in 10 years; How I would be if I were a perfect son to my father; How I appear in the eyes of other people; How I was at the begin-

ning of junior high school; How I would be if everything worked out exactly the way I want it to; How I would be if my dreams came true." The sorts and open-ended interviews provided the basic data reported.

Two correlations were computed to assess identity development. The first (called *intra*year correlation) was based on the relationship of the items from the first to the second administration of the *Q*-sort in a given year, and was the operational definition of "structural integration." A second (called *inter*year correlation) was computed between the items of the *Q*-sort for one year and each succeeding year and became the operational definition of "temporal stability." The analysis of Eriksonian concepts of identity formation was operationalized as patterns of change over the four-year period for each of these measures. For example, the notion of "identity foreclosure" was operationalized as "little or no change in temporal stability or structural integration," while "psychosocial moratorium" was conceived as "fluctuation, swings in both directions of temporal stability and structural integration."

I have spent much time presenting the outlines of Hauser's research because by far the greatest importance of this work lies in the methodological/analytic contributions it makes to the study of identity formation. The substance of the *Q*-sorts results are presented in chapter 4 and can be summarized succinctly: "The overall black patterns [for] both identity formation processes . . . [do] represent identity foreclosure . . . the whites exemplify an overall pattern most consistent with that of progressive identity formation" (pp. 59-60). What this means in Hauser's data is that there is a tendency for all of the correlations to be higher and less variable for blacks than for whites. Since the black subject's correlations are so high, it is entirely possible that a ceiling effect is in operation which precludes the pattern Hauser would define as progressive identity formation.

When Hauser moves into a clinical assessment of the interview data, it is clear that these interpretations are based on the acceptance of the premise that the *Q*-sort data show black identity foreclosure, which is itself based on a diffused discontinuity with the past, an absence of positive role models, and a bleak outlook for the future. These interpretations seem to accept and apply the standard psychoanalytic literature on blacks (e.g., Kardiner and Ovesey; Dollard; Erikson; etc.). As with most interpretative work on the psychosocial dynamics of black people, a restricted perspective and limited observation of functional processes in the black culture make such interpretive analysis hazardous. For example, one black boy refused to take uninspiring jobs as grocery boy or magazine salesman ("I mean I want to get a job that I like to do. I just wasn't no grocery boy, that's all. It wasn't too cool" [p. 85]), and Hauser comments: "The work is somehow always degrading. Frankie likes being comfortable and cared for. Work that is physically demanding is thereby unacceptable" (p. 85). Here as in most other interpretations of his data, Hauser assumes negative significance of thematic material and correlational patterns for black respondents. What started out as a provocative new methodology

degenerates in the end into yet another ratification of the "Dark Ghetto" syndrome.

To summarize, McDonald's book chronicles the experiences of children, parents, and teachers in an interracial setting and provides numerous examples of skin color themes. Hauser's book presents a new methodological approach to the study of identity which shows promise for interesting new lines of investigation. Taken together, they share a perspective that is too familiar and badly in need of redirection and focus.

Managed Integration: Dilemmas of Doing Good in the City. By Harvey Luskin Molotch. Berkeley: University of California Press, 1972. Pp. xii+239. \$8.95.

W. Scott Ford

Florida State University

Managed Integration is another important work published by the University of California Press dealing with issues concerning race and residence. Harvey Molotch provides us with a research monograph written in the resurrected "Chicago" tradition, a tradition respected by many (the reviewer included) for its descriptiveness, conceptual insight, and human quality. *Managed Integration* should be read by any student who professes interest in race relations, urban sociology, urban ecology, community development, organizations, or city planning.

The author has written a case study of the largest and best-organized "self-help" voluntary organization in the United States—the South Shore Commission in Chicago. Molotch spent two years (1965–67) studying the commission's organization, strategies, and impact on the South Shore area of the city during the time in which it attempted to "preserve" the neighborhood by minimizing the entry of black families while attempting to increase the demand for housing in the white market. The commission was, in many ways, exceedingly well organized, highly regarded by City Hall, and successful in many of the projects it undertook. Yet it was unsuccessful in preventing the resegregation of the neighborhood. The author carefully analyzes why this massive attempt at "doing good" in an inner-city neighborhood characterized by modest to middle-income housing failed, and, more importantly, his interpretations posit a genuine challenge to liberal social scientists and planners.

The book is divided into three parts. The introductory chapter includes a description of the ambitious nature of the research, a general discussion of the author's methodology (a more detailed description is found in an appendix), and a brief discussion of community action and the invasion-succession process. Chapter 2 makes a particularly significant contribution to the sociological literature by analyzing the dual housing market which exists in urban areas having sizable nonwhite populations. (The importance of this chapter is due partially to the fact that several other chapters

of the book have appeared as articles whereas this discussion appears for the first time.) Molotch tests the thesis that housing within transition neighborhoods such as South Shore is worth more to blacks than it is to whites, and that owners and agents take advantage of this by charging blacks higher rents for comparable units than their white counterparts. Evidence for this discriminatory practice is presented through an analysis of survey data collected from Chicago-area real estate dealers in the South Shore neighborhood and in other areas of the city. Dealers themselves operated on an assumption of two markets; they made no pretense of parity, especially if they were among those who rented in or near the transition areas. The survey also reveals that the attitudes of white residents in formerly all-white rentals only partially explain why they leave and these areas become increasingly black. It was found to be common practice, for example, for management to lower building maintenance standards with the arrival of black families. Most important, however, is the fact that the decision to rent to blacks is due simply to the lack of profit that agents and owners realize from vacancies and the ready supply of black families whose alternative housing opportunities are few and who are thereby obliged to pay premium rents.

The second part of the book comprises a description of the South Shore Commission, the study area, and the chief means by which a "stable" racially integrated neighborhood was sought. The commission had been in existence for more than a decade when Molotch began his study. Its leadership has passed from the hands of those who wanted the neighborhood to organize to prevent blacks from entering (exclusionists) to those "liberals" who wished to create and maintain an integrated, balanced community (integrationists). The commission had grown to the point where it had an annual budget of \$90,000, 3,500 family memberships, and 75 board members (only six of whom were black in a neighborhood which had become one-third black!). The leadership was drawn from the ranks of the middle-class business and professional community, many of whom spent as many as 20 hours a week on commission activities. They commonly viewed themselves as having an extraordinary moral commitment, of which they were duly proud.

As the ideology of the commission changed from exclusion to managed integration, its strategy changed from an emphasis on "community improvement" to an advocacy of intervening in the processes of real estate buying and management. The reason for this strategy alteration was what Molotch refers to as "the improvement paradox": if your goal is to retard entry of blacks and keep and recruit additional whites, then programs which improve the community (e.g., better police protection) serve equally well to increase demand among blacks. Therefore, beginning in the early sixties, the commission placed increasing importance on its Tenant Referral Service (TRS), a listing service with a high volume of activity and an admitted policy of "setting up artificial restraints until . . . the community [could] achieve racial stability" (p. 116).

The third part of the book focuses on whether the TRS or other

intervention strategies were successful enough to slow the neighborhood transition process from the pace one would expect under the ordinary invasion-succession process. Using a series of innovative measures (which cannot be described here due to space limitations), Molotch found that the distribution of incoming blacks did not approximate a diffuse pattern but, rather, the racial composition (proportion black) of a given area within South Shore was clearly a product of the distance from the point of black entry into that neighborhood. Additionally, he found that, when compared with 12 other Chicago community areas in transition, the rate of racial displacement in South Shore was one of the highest in the city!

Outstanding studies often demonstrate that what we all "know" to be true is questionable, if not fallacious. In chapter 8, Molotch's findings regarding property turnover rates and instability in a transition neighborhood challenge much of the conventional wisdom of the social science literature as well as the well-entrenched view of the lay public. South Shore was compared with another Chicago area which was highly similar to it on 17 relative variables except that it was virtually all white and was not "threatened." The results indicate that, regardless of property type (apartment, multifamily, or single-family dwelling unit), the South Shore area was as stable as its counterpart, or more so. Property exchange rates are generally high in Chicago and elsewhere. The appearance of the first and subsequent black families in what was an all-white block is simply more visible. Linking his findings to an earlier discussion of the dual market system, Molotch argues that panic selling and white flight are not necessary for a fairly rapid displacement process to occur. "Normal mobility makes neighborhood racial change *possible*; when markets are structured in such a manner that blacks continuously constitute the bulk of those who move into the resulting vacancies, racial change is *inevitable*" (p. 173).

In Molotch's final chapter, "Dilemmas of Doing Good in the City," he summarizes the South Shore Commission's intervention program of managed integration as a case study in failure. Although the commission erected one of the most powerful community voluntary organizations in the country, its efforts regarding its major manifest goal were in vain. (In terms of self-perpetuation and growth, the organization was vastly successful.) And, in this neighborhood, failure could not be attributed to racial prejudice (not that it was not there), unscrupulous realty interests, or political manipulation. The commission leaders agreed to attempt a moderate-rational reconciliation with the opposition rather than to employ pressure tactics. This posture prevailed too frequently and for too long. However, Molotch suggests that the principal reasons for failure stem from the lack of realism involved in pursuing the goals of community autonomy and racial integration at a neighborhood level without fully recognizing the more pervasive metropolitan ecological forces as well as certain system-wide inequities and priorities. Independent efforts toward regeneration on the part of one urban area unavoidably bring about change in other areas. For example, in the case of South Shore, community im-

provement increased black demand for housing in a contiguous neighborhood. Regarding integration, short of a random assignment of families to housing units, Molotch argues that only if priorities are radically restructured so as to create new housing on a wide scale and to effectively eliminate discrimination will stable integrated communities be possible. In other words, no amount of community autonomy can reverse the consequences of a land allocation system in a "stratified caste-ridden society."

I recommend this book; it is a sound contribution, it deserves to be read, and I agree with its basic arguments. However, there are a few disappointments. Molotch is his own worst enemy when, in his preface and again in his introductory chapter, he refers to his own research techniques and later analysis in such a fashion as to suggest that he is more a cross between an artist and a detective than a social scientist: "It likely would only take more time and more drafts to revise and perhaps reverse many of my conclusions" (p. xi). On a few occasions, the author makes reference to the "contact hypothesis" without making it terribly clear how he interprets this perspective. Although racial head counts of blacks and whites in various community settings do indicate the rarity of interracial contact, they do not clarify the style of contact that does take place. Neither is the impact that residing in a transition neighborhood has upon the residents measured in any meaningful way. (This is more of a comment than criticism; in this case, one should not be criticized for what he did not do.) With regard to data collection and methodology, I think it would have been appropriate to include the landlord interview schedule in an appendix. Since a third of the real estate dealers refused to be interviewed (pp. 233-34), some (unobtrusive?) attempt to determine whether their operations were significantly different from those of the respondents might have proved revealing.

I have a concern (rather than a criticism) with Molotch's critique of community organization and integration as local strategies pertaining to the possible use of certain statements in future policy-related decisions: "it is probably unrealistic to expect any community organization to take responsibility for solving problems created by the general plight of the American black—problems which have proven too difficult for rather substantial government programs and for sophisticated social scientists" (p. 214). Such a statement can, and probably will, be interpreted in different ways. I would hope that no one will interpret the major thrust of Molotch's concluding argument to suggest that neighborhood stability, especially under desegregated conditions, is desirable but impossible and thereby abandon the goal in a manner similar to cases where school desegregation became a short-term impossibility and other urban "demonstration" projects failed. One might better emphasize the following statement from the author's concluding pages: "Improving the *quality and quantity* of black people's housing and achieving the spatial integration of American blacks are not separate or contradictory goals. To question which is more important is to raise a false issue. They are inseparable and, significantly,

the solution to the integration problem is through the solution to the quality-quantity problem" (p. 220).

Managed Integration is a well-integrated book; it reads easily and is well written. As I suggest above, unlike many research monographs it has something to say of genuine importance to an interdisciplinary audience. I fully expect that its findings and interpretations will be widely cited in the years to follow. There is a brief postscript included in which Molotch reports his observations of the commission and South Shore after the passage of three years. I leave that to the reader.

Discrimination, Personality, and Achievement: A Survey of Northern Blacks. By Robert L. Crain and Carol Sachs Weisman. New York: Seminar Press, 1972. Pp. xiv+225. \$9.95.

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This study is a heuristic attempt to use survey data in measuring the personality traits of northern blacks. Information was gathered by the National Opinion Research Center for the U.S. Civil Rights Commission in 1966. Although originally intended as a study of the impact of school integration upon adults, the project was expanded to cover a large number of social-psychological variables. Over 1,650 respondents between the ages of 21 and 45 in 25 nonsouthern metropolitan areas made up the sample; comparative materials were later obtained in an NORC survey of urban whites as well.

The authors' basic hypothesis (i.e., that discrimination leads to psychological disabilities which in turn prevent black achievement) has long been accepted by thoughtful scholars in the field of race relations, and various facets of the thesis have been tested over the years. As early as 1940, for example, Lloyd Warner and his associates examined the effects that subordination had upon personalities of Negroes in Chicago in their work *Color and Human Nature: Negro Personality Development in a Northern City* (Washington, D.C.: American Council on Education, 1941). Most of the research has been piecemeal, however, and the present work represents the most systematic attempt to date to understand the interactional effects of the relationship of several personality traits to success. The study is particularly welcome at a time when literature on black poverty has become embroiled in polemics. Radicals tend to blame all inequities on white racism; conservatives blame the blacks themselves. Questions about psychological disabilities among blacks are therefore not being asked by the former or answered by the latter. While neither of these two groups may be particularly happy with this book, it should serve the cause of social science well.

Much of the text is devoted to the analysis of aggression, internal control or fatalism, self-esteem, happiness, and antiwhite feelings, which

are then correlated with eight achievement variables. The final section examines those factors unique to the black experience—family structure, segregation, and racial discrimination—to study how these may account for observed black-white differences.

Findings support the general thesis that blacks do have personality traits—different in kind or degree from whites—which hinder success. They have a higher incidence of overt aggression, as measured by fights and fights, as well as a higher incidence of anger inhibition. They are more fatalistic, more often unhappy, and those who are migrants from the South generally have lower self-esteem (high self-esteem here is defined as viewing oneself as better than average on a number of items). Collapsing black personality traits creates two clusters. The first, labeled the "secure" group, consists of high internal control, a sense of happiness, low aggression, and low antiwhite feelings. This cluster is positively correlated with measures of achievement—occupational and marital stability, ownership, financial responsibility, and higher levels of education and income. The second, or "assertiveness," group includes the ability to express anger and high self-esteem. It is negatively correlated with the same items, and its relationship to the achievement factors is the reverse of the elements of the first cluster.

The authors were surprised that expression of aggression and high self-esteem proved dysfunctional for blacks, given the contemporary struggle for greater freedom and self-expression. Yet it is on the "assertiveness" cluster that the most serious questions about validity of measurement are raised. The questions used to discern anger or its inhibition are self-referential (e.g., "I have been arrested," "I have been discriminated against"). The authors' use of frustration-aggression concepts also contains many of the psychoanalytical ambiguities that Gordon Allport criticized in the 1950s. By defining high self-esteem in terms of above-average self-esteem, Crain and Weisman may have obscured other basic components. It is quite conceivable that their high-esteem group included a significant proportion of respondents who mask basic insecurities by a kind of bravado.

As expected, family background, segregation, and discrimination were found to account for many of the personality difficulties, although always in terms of the conventional wisdom of social science. The considerable variation by sex and region of birth. Coming from a rural home does depress income for men and educational success for women. Poverty was not found to account for the effects of disrupted homes on personality factors that were strongly associated with broken homes: greater aggressiveness, fatalism, unhappiness, the likelihood of changing jobs frequently, and marital instability. In other words, deprived children, without parental love, grow up with an excessive need for gratification and a sense of anger at this deprivation which in turn affects occupational and marital stability, a pattern consistent with much theory regarding the effects of deprivation of parental security.

Contact with whites at an early age (especially through school) is

tion) seems to reduce anxiety, increase a sense of security, and enhance achievement for northern-born respondents, but was less beneficial for southern migrants. Southern-born blacks are more likely to succeed if they are lighter skinned, were socialized to be assertive, and had a mother who was a high school graduate; but northern-born blacks who remain segregated in the ghetto are likely to be more unhappy, express more rage, and do less well economically than southern migrants. Segregation, often taken for granted by those raised in the pre-civil rights South, seems thus to be a greater irritant to the northerner who feels he is caught up in a life of "freedom without hope." Over all, school integration does seem to have important sociological and psychological benefits. Respondents who attended integrated schools are better educated, earn more money, say they are happier, and are less aggressive. But these effects depend on the consistency of one's experiences. Those who seem to make the best adjustment in adult life are those who experienced interaction in both elementary and high school, followed by those who experienced segregated schooling at both levels. Inconsistent integrated experiences in school, in neighborhoods, or in friendships produce negative effects, due perhaps to a sense of marginality or cultural shock. The authors admit that their conclusions are complicated by the changes that have occurred since their respondents were growing up. They do not, however, feel that what is fundamentally beneficial about true integration has changed. For them, segregation as a symbol of lack of opportunity is as harmful as discrimination, the actual restriction of opportunity.

It is perhaps accurate to say that the study raises more questions than it answers. There are so many findings that are unexplained, so many relationships that are too little explored, so many points that seem confusing or contradictory; the reader must return again and again to earlier textual material. Yet there is little doubt that this book will stimulate new and more meaningful research. While some critics may find fault with the project for its lack of in-depth analysis of the black personality, others will applaud it as a major attempt to move analysis from small and often artificial clinical or college samples to the "real world" of black urban America. Where better can we obtain the kind of information needed to assess the impact of minority status and social change?

Understanding Race Relations. By Ina Corinne Brown. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1973. Pp. xi+275. \$7.95 (cloth); \$3.95 (paper).

Clovis E. Semmes

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As an anthropologist, Brown takes the significant point of view that race relations is an aspect of culture and therefore part of acquired behavior. She begins with a brief introduction to contemporary race relations, describing the historical foundations of slavery and the changing mood among black Americans, and goes on to describe the institutional character of

racism as well as the traditional conception of "good" race relations. For example, she states, "'Good' race relations have usually meant racial harmony achieved within the framework of the subordination of the minority group" (p. 10). Brown closes her introductory chapter with a definition of race relations: "the race relations problem can be summed up as a division of values and goals growing out of the fact that whites and Negroes view their relationship from different positions in the social structure. The white people on the whole have set up for themselves a set of values and goals that are quite different from those they deem appropriate for Negroes" (p. 11).

Brown spends a great deal of time dispelling concepts of racial inferiority as well as probing the roots and function of racial mythology. For example, she discusses and dismisses assumptions that the black man is culturally inferior and that Africa is a land of primitive savages. In addition, she provides a brief history of the black man from the rise and fall of the great African civilizations to slavery and contact with Europeans. This account is followed by a brief overview of the historical experiences of blacks in America from 1619 to the 1960s.

Brown includes chapters on "Race, Class, and Poverty" and "Race, Crime, and the Law." Considering the former, she states: "Although the cards are clearly stacked against the poor in general, the black man has all the handicaps of his white counterpart plus a great many that are peculiarly his own" (p. 122). In the chapter on crime she discusses such topics as the problem of lawlessness, ordinary crime, white-collar crime, organized crime, the unequal protection of the law, violence as a form of protest, and law and the system of values.

Subsequent chapters deal with the "Black Experience" and the "Black Revolution." Unfortunately, these chapters are very superficial. Attempts to define the black revolution, the search for identity among blacks, the revolt among youth, etc., often leave the reader without a firm grasp of any particular issue. Brown presents a similar chapter on the "White Experience," which leaves the reader with little understanding of what constitutes such an experience.

In the last chapter, "The Human Use of Human Beings," Brown tries to make the point that constructive change, made possible by the commitment of good men as opposed to the actions of militants and rebels, can rectify the past. She states:

Those who see no relevance in the past are prone to condemn Western civilization as something wholly evil. They point to modern man's plundering of the earth, his materialistic values, his worship of success, his preoccupation with brutal wars, his prejudice, and his general inhumanity to man, as marking the total failure of modern civilization, particularly that part of it labeled Western. So disenchanted are many of today's revolutionaries that they are committed to tearing the whole thing down without much thought of the consequences to the human species, or any clear idea as to what they would put in its place.

But those who would destroy "the Establishment," close the universities,

and do away with representative government without regard to the resulting chaos, reveal their ignorance of the nature of man and the means of life. [P. 226]

Undoubtedly, few people would disagree with an argument that calls for human beings to work together and commit themselves to eliminating the evils of mankind. But Brown merely prescribes more of the same. That is to say, she projects an ideology already dominant in the Western world and in Western social science: Western society, exemplified by the United States, is synonymous with mankind and human progress. My intention is not to debate this issue but only to point out that this book is designed to foster such a perspective.

The text is clear and readable, although footnotes, references, and suggested readings are included at the end of the book. The author explains that she organized her book for clarity, but the reader is faced with difficulty if he or she wants to check Brown's sources. Brown states initially that she intended her book to be an overview and a guide to further research. This is an accurate description of the book at its best.

Understanding Race Relations has some merit by virtue of its range of information. However, I would not call Brown's endeavor innovative. It is a well-written addition to traditional, status quo-oriented social science literature and has some value as an introductory text. The broadness of her presentation and often superficial handling of various topics require the interested novice to seek other sources and points of view.

The Violent Society. By Stuart Palmer. New Haven, Conn.: College and University Press, 1972. Pp. 223. \$6.50 (cloth); \$2.95 (paper).

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The Violent Society attempts to integrate a number of approaches to violent behavior in order to explain both individual acts of violence and riots. The effort falls severely short of its goals in a number of areas, primarily because of inconsistent, illogical, and inaccurate uses of terms and explanations. These problems are so pervasive that they minimize the importance of the book as a contribution to the field.

The author argues throughout the book that unreciprocity leads to homicide (and riots) and reciprocity (or large drops in unreciprocity) leads to suicide. Unfortunately, the concept of reciprocity is so inadequately defined that it becomes practically meaningless. Unreciprocity is defined both as conflict and as leading to conflict. It is identified with frustration, with role conflicts, with differential access to means or blockage of such means, with discrimination, and with status threats. At times, unreciprocity and reciprocity require interaction and at other times they do not. Although reciprocity leads to low tension, it also leads to frustration, which is generally defined as involving tension.

Inadequate conceptualization emerges repeatedly in circular arguments and contradictory statements. Thus, since Icelanders have low homicide they must have mid levels of reciprocity, even though they are "fiercely independent" (p. 29). The author cites evidence for this assertion in a note which indicates it is his personal observation.

Similarly, since New England has a lower homicide rate than the South it must have greater reciprocity. Since urban areas now have higher homicide rates than rural areas, the locus of greatest unreciprocity must have changed. Palmer later argues, however, that urban areas also have higher suicide rates and therefore must also have greater reciprocity. Although explaining ghetto homicide in terms of large amounts of unreciprocity, he accounts for suicide rates in ghetto areas by describing them as highly organized, with too-great reciprocity. The author offers no independent evidence to support his contention that weekends, which witness an increase in ghetto homicide, must be periods of high unreciprocity. Since male homicide rates are greater than female rates, he argues, the male role must involve greater unreciprocity, although unreciprocity can develop from the inferior status women have. High swings in tension must account for the high suicide rates among black males.

Palmer's logic is also weak in the face of topics such as the uses of social control. While it may be true that agencies of social control are not sufficiently concerned with crime prevention, it seems stretching a point to argue that they have a vested interest in perpetuating homicide, suicide, and other violent behavior in order to stay in business. The parallels the author draws between the agencies of social control and the offender are also exaggerated. His argument that legalized killing and homicide are symptoms of similar underlying social processes runs counter to the information that legalized killing is decreasing while homicide is increasing, information which he has presented himself. The only explanation offered for the contradiction is that there may be a lag of "several decades" (p. 77).

While arguing that social-control environments (prison) are extremely brutal, the author also contends that prison provides great rewards. The great rewards, however, do not induce a released prisoner to return to prison. Nor does it appear that labeling the individual "murderer" causes him to commit homicide again. ("Homicidal offenders . . . seldom return to prison" [pp. 74-75].)

With little apparent justification, Palmer argues that homicide and race riots might best be contained by not introducing agencies of social control. He believes that the presence of such agents exacerbates riots, despite the fact that agents have adequate controlling force. Palmer presents a picture of a society allowed to run amok until such time as tensions are sufficiently lowered so that social-control agents might propitiously enter the scene.

The author's contradictions are not always so subtle. For example, he argues that poor blacks and poor whites need each other and depend on each other in their adversary relationship. Consequently "an inverted

reciprocity develops because of the patterned unreciprocity that obtains" (p. 173). The most striking example occurs late in the book. After arguing throughout that the extremes of unreciprocity and reciprocity lead to conflict, the author states that "humans tend toward extremes. [It is] the middle ground that is ambiguous and anxiety-provoking for many" (p. 147).

There are some worthwhile discussions in the book, such as the chapter "Homicides and the Individual." A rapid reading of the work provides an impression of some insightful concepts and explanations. Unfortunately, careful examination of the material indicates that the discussion is quite inconsistent and that the formulation offered has, at this point in time, little substance.

Deterrence: The Legal Threat in Crime Control. By Franklin E. Zimring and Gordon J. Hawkins. Chicago: University of Chicago Press, 1973. Pp. xiv+376. \$13.50.

Charles R. Tittle

Florida Atlantic University

It is not easy to praise a book which portrays one as stupid. But despite repeated references to a minor finding in one of my articles as a prime example of methodological incompetence among deterrence researchers, I must acknowledge the merit of this work. It is a thorough, skillfully written, and insightful book which makes an important contribution to the study of law and criminology.

After being ignored or considered resolved for decades, questions concerning the extent to which and the conditions under which sanctions deter deviance have recently evoked great interest. Even though much social policy already assumes well-founded knowledge, actual empirical information is still very meager. Operating from a strictly pragmatic legal perspective, Franklin E. Zimring and Gordon J. Hawkins have written a treatise intended to make that fact clear and to stimulate efforts to rectify the deficiency. The book, basically an expansion of Zimring's earlier NIMH monograph, demarcates issues in the study of deterrence, points up our ignorance concerning these matters, identifies methodological pitfalls in investigating the questions, and outlines strategies for possible research.

The discussion includes (1) a section that distinguishes among and clarifies the meanings of different kinds of deterrence, (2) a detailed theoretical rationale for deterrence that pinpoints direct and indirect ways in which the threat of punishment can affect the degree of conformity in a social unit or among individuals, (3) a thorough cataloging and assessment of the various conditions which might influence the extent to which a deterrent effect is possible for given individuals or in given situations,

(4) an extensive critique of various research strategies, emphasizing their strengths and weaknesses, and (5) a programmatic plan for future research. Throughout, the authors weave in the wisdom and/or empirical findings of a vast body of legal, psychological, and sociological literature. The result is a careful and sobering review of the status of knowledge about an immensely important issue.

Despite the many virtues of the book, it is not without defects. First, as would be expected, the authors are far more conversant with the legal literature than with the fields of sociology and psychology. Although they demonstrate a remarkable eclecticism, their deficiencies in the nonlegal areas are all too obvious. Yet arrogance leads them to believe that they have surveyed all the relevant material. With heavy reliance upon pre-1960 materials, the authors show no hesitation in asserting that there is little or no data about such matters as persuasive communication, risk taking, perception, or group influences on individual behavior. Moreover, they often cite speculation by legal scholars when reference to research literature of another field would have been more appropriate.

Second, while the methodological critique is probing, astute, and in most instances correct, there are clear signs of naiveté, particularly about ordinary statistics. An enchantment with experimental methodology or analogues thereto has resulted in failure to recognize the importance or the validity of possible interaction effects in research on the consequences of sanction threats. The authors' projected ideal design is one that eliminates the effects of all except one independent variable, but there is good reason to suspect that deterrent effects may be contingent on the simultaneous operation of multiple factors.

Third, many sociologists will be disappointed in the crass pragmatism of the work. The authors are primarily interested in resolving practical legal questions about whether given threats in a specific situation will produce a deterrent effect at an acceptable cost. They eschew development of general principles or efforts to build general theories without realizing that such efforts could in the long run better serve both practical and scientific goals.

Finally, even though the intent of the book is to stimulate research, and even though it encourages attacking problems with a variety of imperfect designs, its critical methodological stance may have been so salient that it actually discourages research. Many are likely to be left with the impression that nothing is known or can be discovered about deterrence, especially since Zimring and Hawkins are themselves apparently discouraged. Although they both have long been involved with the deterrence question, their empirical contributions to the literature are conspicuously absent.

But such deficiencies should not be allowed to detract from the quality of the book. I recommend it highly not only to specialists in criminology or law but to the general sociologist. After all, the role of sanctions in the development and maintenance of social order is not a specialized question, but is in the most fundamental sense crucial for all students of society.

The Ideological Imagination. By Louis J. Halle. Chicago: Quadrangle Books, 1972. Pp. xiv+174. \$6.95.

Seymour Leventman

Boston College

This is a difficult book to evaluate. Although the publisher claims it is a thoroughgoing scholarly treatise on "the rise of mass bigotry in our time," the author develops his book as a personal essay on those philosophic antecedents of current events that offend his own ideological proclivities. Accordingly, the work is a rather curious mixture of scholarly exegesis (his discussion of the *Communist Manifesto* [pp. 56 ff.]), pedestrian observations ("all we know is nothing alongside all we do not know" [p. 137]), and naive misstatements ("by 1970, free speech on the issue of the Indo-China War was still possible virtually everywhere in the United States except on university campuses" [p. 129]). The author is a professor at the Graduate Institute of International Studies in Geneva.

The most interesting and acceptable part of the book is the first half, in which Halle displays considerable philosophic erudition, ruminating on the classical origins of both democratic and centralist ideologies. Tracing its intellectual degeneration, Halle claims ideology has in fact become associated with totalitarian beliefs. He defines the term "ideology" as beliefs "so complete that whole populations may live by them alone" (p. 5), having been presented by organizational elites "that claim exclusive authority as representing something like revealed truth, and that consequently require the suppression of whatever does not conform" (p. 6). The author appears to miss Karl Mannheim's essential point that ideologies mask the deceptions and guises of all human interest groups.

Halle contends that historically ideology accompanied the rise of mass democracy. However, while democracy accommodated social diversity, ideology eventually floated into the totalitarian camp, thus reaffirming the necessity for uniformity and social constraint. Examining the writings of Jean-Jacques Rousseau, Thomas Hobbes, and Karl Marx, Halle concludes that modern man is in danger of losing his mooring in social reality by virtue of the mythic simplifications of professional ideologists and revolutionaries.

The social role of myths is a problem which is fascinating and eminently worth pursuing. Regarding Halle's discussion of man the myth maker, Ludwig Gumplowicz once wrote about the societal blending of fact and fancy until one seems incapable of existing without the other. In the last third of the book it becomes clear that he is not especially inclined to pursue these suggestive theoretical leads. He instead becomes concerned with demonstrating that the academically based radical movements of recent years represent the most serious threat to modern liberal democracy. Thus, "the vast intellectual communities of modern times are . . . more vulnerable than others to ideological thinking, with its intolerance" (p.

129). Presumably only men of practical affairs, for example, businessmen and politicians, with no ax to grind but their own special interests are qualified to run society.

Halle does raise such potentially important questions as the apparent affinity of intellectuals for left-wing rather than right-wing politics. But is it really true that ideologizing is most characteristic of mass thinking and those intellectuals who speak for the masses? Ultimately, Halle seems most concerned with condemning the producers and receivers of what he maintains are low-level appeals to mass intolerance. Rather than elevate his work to a social epistemology, he reduces it to a polemic against academics and other "wrong-headed" intellectuals who appear to him to be at the root of modern mass bigotry, that is, the suppression of diverse opinions. I prefer the clearer-headed wisdom of C. Wright Mills, who wrote the following words in regard to this issue in 1955:

It is the respected judgments of Secretaries of State, the earnest platitudes of Presidents, the fearful self-righteousness of sincere young American politicians from sunny California, that is the main danger. For these men have replaced mind by the platitude, and the dogmas by which they are legitimated are so widely accepted that no counter-balance of mind prevails against them. Such men as these are crackpot realists, who, in the name of realism have constructed a paranoid reality all their own and in the name of practicality have projected a utopian image of capitalism. They have replaced the responsible interpretation of events by the disguise of meaning in a maze of public relations, respect for public debate by unshrewd notions of psychological warfare, intellectual ability by the agility of the sound and mediocre judgment, and the capacity to elaborate alternatives and to gauge their consequences by the executive stance. ["On Knowledge and Power," *Dissent* 2 (Summer 1955): 201-12]

Lacking an appropriate sociological imagination, Halle mistakenly attributes contemporary mass intolerance to counterculturalists rather than to "pro-cultural" functionaries of modern societies, particularly the state, democratic or otherwise. These criticisms notwithstanding, the book can still be recommended for the sincere scholarship it embodies.

Work in America. Report of a Special Task Force to the Secretary of Health, Education, and Welfare. Cambridge, Mass.: M.I.T. Press, 1973. Pp. xix+262. \$2.95 (paper).

William H. Form

University of Illinois

Work in America represents the synthesized wisdom of a committee of experts who commissioned 57 other experts to summarize their thoughts on how Americans react to their work. Two subjects are examined in the report: the problems of work and job satisfaction, and of employment and wages. Presumably, sociologists and social psychologists were primarily responsible for the section on jobs and work, and economists

primarily compiled the sections on employment and wages. Those who seek a statement of what liberal social scientists think is going on in the world of work and what reforms would therefore cure the evils therein will not be disappointed. The credo can be summarized briefly.

Everybody knows that jobs are so dull and work so meaningless that absenteeism, tardiness, sabotage, and strikes are increasing. Affluent young workers are now overeducated for their jobs, and they cannot actualize themselves in work as Maslow says they should. Following Herzberg, workers may be satisfied with extrinsic aspects of their employment, but they experience little intrinsic job satisfaction. The consequences of this condition are alienation, decreased longevity, heart disease, poor mental health, drug abuse, delinquency, and related social pathologies. What workers really want is not more money but more work autonomy, more participation in management decisions which affect them, more challenging jobs, and more involvement in the work organization. Management should give workers what they want, because research clearly demonstrates that workers will then become more interested in their jobs, miss fewer days of work, be more productive, and become more involved in community affairs.

By assuming that social scientists agree that a crisis of work satisfaction exists, by accepting much soft-headed research, by ignoring many studies which contradict the position of this report, the task force has done a disservice to social science. A case can be made for conclusions opposite those presented in *Work in America*. The evidence is overwhelming that most workers are satisfied with their jobs, that wages are their most immediate concern, that job interest and autonomy are not associated with productivity, that reduced life chances result from position in the stratification system and not from job dissatisfaction, that most workers distrust management participation schemes, and that most workers do not want their jobs to be redesigned. The task force's distorted and partial reporting of social science findings places reviewers in a dilemma: if they agree with the report, they are not impartial social scientists; if they disagree, they are conservative opponents of social change in the workplace. Changes are needed in the world of work, but many of them cannot be justified on the basis of hard consensual social science findings but, rather, on the basis of affirming a particular moral stance.

The second part of *Work in America* strongly reflects recent research in economics. Analysis of the present scene begins by asserting two basic goals: total employment of the potential labor force and high wage levels. Poverty, unemployment, underemployment, and welfare loads are identified as the problem in America. The responsibility for failure to attain the twin objectives of total employment and high wages is placed in the functioning of the educational, economic, and governmental institutions. American higher education is attacked for its credentialism and inability to train people for jobs; vocational education is attacked for its costliness and failure to train skilled workers, and secondary education for its irrelevance. Hard data are brought to bear on these attacks. Moreover, soft spots in

the economy and polity are clearly identified: hidden unemployment, the existence of a stratified dual labor market (one sheltered and the other exposed to the vicissitudes of the free market), the inability of the federal government to manage the inflation-employment nexus, the failure of the federal government to spend in areas which would employ manual workers, failure of the manpower programs to train a broad range rather than only marginal workers, failure of American industry to keep up with technological changes abroad, and the economic and moral bankruptcy of the AFDC and other welfare programs. Unlike the reforms proposed to improve work and job satisfaction, the programs proposed to increase employment and wages are concrete and evaluated for their costs, benefits, feasibility, urgency, and morality. Moreover, the programs of reform are specific in describing the kinds of institutional changes and institutional exchanges which need to be made in order to achieve the stated goals. If economists can do this kind of work, who needs sociologists?

Bureaucratic Structure and Authority: Coordination and Control in 254 Government Agencies. By Marshall W. Meyer. New York: Harper & Row, 1972. Pp. ix+134. \$3.50 (paper).

James J. Noell

University of Notre Dame

This short book is a comparative, quantitative analysis of 254 state and local government finance departments considered as formal organizations. It is a product of one of a series of research projects conducted under the aegis of the Comparative Organization Research Program, founded some 10 years ago by Peter Blau and Wolf Heydebrand, then both at the University of Chicago, to plan and carry out comparative, quantitative analyses of large numbers of organizations.

Meyer focuses on a number of fundamental problems concerning the structural patterns and dimensions of formal organizations. These include the structural consequences of the "size" of the organization; the implications for the centralization or delegation of authority, of various patterns of organizational differentiation; the impact of "automation" on the formal structures—including the hierarchy of authority—of the organization; and the structural and administrative significance of varying degrees of intra-organizational interdependence (among an organization's "divisions"). Using the "organization" as the basic unit of analysis, Meyer explores a number of ideas as a part of what he calls "a conceptual inquiry into how organizations function" (p. vii).

Although there is much that is suggestive and valuable in this work, a number of problems—both conceptual and empirical—must be noted. First, Meyer rejects the concept of the organization as an "open-system." He claims that the "environments" of the organizations he studies are less salient than those of other organizations. This assertion is based on no

evidence—would that we had such evidence about the impact of the environments of different types of organizations! Later in his analysis (p. 62), he himself invokes the environment as a significant determinant of organizational structure. In addition, his conception of organizations as “closed, self-contained systems” (p. 9) restricts the relevance of his findings for developing a more general “theory of organizations,” even—or perhaps especially—if the “environment” is of “minimum” consequence in this “case.”

Second, and more important, Meyer’s analysis is characterized by lack of a unified perspective. In writing this short work, he has essentially expanded several previously published articles. Articles are not infrequently expanded into books of course, but there is always the risk of insufficiently integrating the materials so that the work as a whole lacks coherence. The problems Meyer chooses to expand from previously published articles are worth further and more detailed consideration in the form of a book. Because he does not use the same set of variables in the analysis of different problems, however, his work lacks coherence. When he analyzes the problem of the centralization (or delegation) of authority, he fails to consider the effects of automation or variations in intraorganizational interdependence; when he analyzes the consequences of automation, he ignores differences in interdependence, and so on. This failure stems from two sources. First, he does not establish a “hierarchy of variables” to be used in a systematic analysis. Second, although he frequently points out the “complexity” of the organizations he studies, he fails to employ sophisticated statistical techniques that would enable him to do multivariate analyses.

A number of more specific criticisms could also be made concerning his handling of particular issues, but I will note only one which concerns his analysis of automation, obviously a problem of wide interest for some time—at least since Marx! While there are, of course, a number of significant issues concerning the impact of automation—some of which Meyer handles in an interesting fashion—one fundamental issue concerns the effect of automation on the “skill level” of employees: will the latest form of automation result in proletarianization? One way to answer this question is to compare “automated” with “nonautomated” organizations in terms of the “expert” personnel employed to see if there are any differences. Meyer instead misleadingly compares divisions in nonautomated organizations with the “line” and “data-processing” divisions in automated organizations (table 7, p. 72). This approach allows comparison among divisions within automated organizations but does not provide an overall evaluation of the impact of automation on skill levels in all organizations.

In sum: this book deals with interesting, significant problems but leaves many unanswered, unanalyzed questions. It is to be hoped that Meyer—who is completing a longitudinal analysis of these same organizations—will deal with these problems in a more comprehensive fashion in his future reports.

American Journal of Sociology

Household and Family in Past Time. Edited by Peter Laslett with the assistance of Richard Wall. London: Cambridge University Press, 1972. Pp. xii+623. \$37.50.

Nicholas Tavuchis and Steven Kaplan

Cornell University

For years historians have disdained the systematic study of the family, either because they did not believe that it was susceptible to conventional historical inquiry or because they did not think it was the locus of a really significant social experience. Sociologists had little incentive to reproach them for this neglect, in part because there was so little serious dialogue between the disciplines but also because the social scientists, themselves uncomfortable in the time dimension, had no reason to quarrel with the conveniently packaged "invented history" (the phrase is borrowed from R. J. Smith, one of the contributors to the book under review) which happily confirmed their theoretical insights, buttressed their models, and also served certain ideological needs of society at large. This complicity of mutual indifference and complacency between historians and social scientists resulted, *inter alia*, in the perpetuation of certain myths about the structure of the family and the household which have shaped the nature of the investigation and the interpretation of social change.

The publication of this imposing volume dramatically marks a new direction in the historical study of the family. To be sure, the old stereotypes had already fallen under attack, but never so massively nor so decisively. It is revealing that Peter Laslett, the moving force behind this renewal, came to the study of the family partly in an effort to make sense out of 17th-century political theory and its relationship to English society. Instead of satisfying himself, as many historians and sociologists continue, alas, to do, by inferring knowledge of social structure and change from contemporary attitudinal evidence, he developed a strategy for studying social structure and change directly, as it were, without intermediaries. The Cambridge Group for the History of Population and Social Structure has become, along with the Institut National d'Etudes Démographiques and the Sixth Section of the Hautes Etudes in Paris, the world's leading center for research in historical demography and related areas of social history. *Household and Family in Past Time* presents the results of an international conference organized by the Cambridge Group in 1969 for the purpose of assessing the hypotheses and methods of Laslett and his associates and placing their findings on household size and composition in comparative perspective. Although the volume modestly purports to be a mere progress report, there is no doubt that it will become an indispensable point of departure for future interdisciplinary social research. "Interdisciplinary" is the methodological and hortatory theme of the book, and it merits special emphasis at a time when the chasm between the humanistic and scientific cultures still looms large. The book is striking evidence that the historian is no longer unwilling or unequipped to count and to use

relatively sophisticated statistical techniques without, however, succumbing to some sort of neopositivistic mystique. He remains acutely skeptical of elaborate sociological theories (see, for example, A. M. van der Woude's essay), but he is also alert to the utility of using models and forming hypotheses. From the other side, it is clear that social scientists themselves are increasingly allergic to "hypotheses in the absence of fact" (see E. A. Hammel's essay); that they are less inclined to deal with historical data casually or cavalierly, as window dressing or foils for other purposes; and that they are learning to subject these data to rigorous, critical scrutiny. *Household and Family in Past Time* augurs well for cooperation and exchange across traditional lines which in some ways no longer have meaning.

The major questions considered by Laslett and the other contributors may be stated as follows. What historical and comparative evidence do we have that the residential familial or domestic household in preindustrial societies was large and complex and that most members of society lived under such conditions? In addition, what do surviving historical documents reveal about the alleged determinate transition from extended to nuclear households as the predominant form of family organization?

The book is introduced by a seminal essay by Laslett, about which we will have more to say later; commentary by T. K. Burch on demographic influences on household size; and a description by the anthropologist J. Goody of the developmental cycle of agricultural domestic groups in Africa and Asia. The plan of the volume revolves about a detailed comparison between mean household size and distribution of households by size (among a host of other variables, e.g., inclusion of other kin, servants, generational depth, number of children, etc.) of a basic English sample of 100 communities and studies from Western Europe, Serbia, Japan, and the United States, including appropriate controls when the historical data were not comparable. Important areas and cultures are obviously not represented: Africa emerges only incidentally, and there are no studies of India, China, Russia, Latin America, or Islamic societies.

Concerning England, the book contains Laslett's essay on mean household size since the 16th century, R. Wall's on mean household size estimates from printed sources, W. A. Armstrong's study of 19th-century York, and M. Anderson's investigation of Preston, Lancashire, in the 19th century. Studies encompassing selected communities in Western Europe include J.-N. Biraben on the southern French village of Montplaisant in 1644, Y. Blayo's examination of household structure of the village of Grisy-Suisnes in the Parisian basin in the 19th century, C. Klapisch on Tuscany in the 15th century, J. Dupâquier and L. Jadin on household structure in late 18th-century Corsica, van der Woude on the Netherlands in the 17th and 18th centuries, and É. Hélin on Liège in 1801.

A cultural area that has received scant attention from Western scholars—Serbia—is admirably treated by Hammel's discussion of the *zadruga*, that is, complex Balkan households, as process. Laslett and M. Clarke analyze the Serbian sector of Belgrade in 1733–34; J. Halpern the 1863 census of towns and countryside in Serbia. Household structure in pre-

modern Japan is discussed by Smith, the Tokugawa period by A. Hayami and N. Uchida, and C. Nakane offers a comprehensive survey of Japanese households over three centuries.

Finally, relevant comparisons between the English base sample and the United States are made possible by the work of P. J. Greven, Jr., on the average size of families in 18th-century Massachusetts and in the colonies of the United States in 1970, and E. T. Pryor's comparison of Rhode Island family structure between 1875 and 1960. Included in this section is a provocative essay on the psychological implications and consequences of family size by J. Demos.

Inevitably the essays are uneven in quality and differ to some extent in purpose, approach, and scope. The authors do not always agree about their use of the same terms or the same kinds of documents. Given this formidable array of complex materials and analyses drawn from widely divergent cultural areas and time periods—500 pages of sometimes laborious prose and tables—Laslett, the primary editor, might understandably have succumbed to the temptation of perfunctorily "letting the facts speak for themselves" and thus relegated this book to the academic limbo of widely cited but little consulted reference books. That he chose not to do so is evident in his 89-page introduction, which gives the volume a remarkable coherence and enormously enhances its utility as a guide for further research. In the introduction he critically reviews various speculative theories of family change (e.g., evolution, the notion of survivals, Marxian, Le Play, etc.) and explicitly scrutinizes the strengths and weaknesses of various kinds of sources which were and continue to be used as "evidence" for the decline of large families in the course of industrialization. He also spends considerable time laying ground rules with regard to definitions, methods, and analytical approaches that he hopes can be systematically employed in studying the family cross-culturally and historically. In this context Laslett presents an extremely useful set of ideographic notations for various family/household structures using English data as the standard. Finally he juxtaposes the Cambridge Group's findings on England with the other cultural areas covered in the book. The essays are further linked by an elaborate series of cross-reference footnotes which suggest comparisons, discrepancies, and problems that might easily elude the reader and which give the volume an unusual continuity of dialogue. The bibliography at the end of the volume is wide ranging but unfortunately does not contain full references to all the works cited in the text, which will in some cases be extremely difficult for scholars to retrieve.

Although it is impossible within the constraints of a review to delve into the intricacies and myriad qualifications of Laslett's arguments based on his own contributions and those of others, we find that he comes to two major conclusions. First, the nuclear family, defined variously as a married couple, a married couple with children, or a widowed person with offspring (conjuality being the crucial structural principle), is and was the dominant form of the domestic group for most people in societies in the past for which any historical records remain. (Laslett underscores the

fact that he has not concerned himself with kinship relations or ties between distinctive households. To do so would raise a totally distinct set of problems and questions that simply could not be adequately answered with the available data.) Moreover, even in the apparent exceptions cited—various parts of Serbia, southern France, northern Italy, the colonial United States, and perhaps Czarist Russia—large extended households existing for numerous generations were not numerically dominant, although a large proportion of the population may have experienced such domestic arrangements at some time or other in their lives. Although mean household size in Japan is comparable with that of England over time, as a result of arrangements peculiar to Japanese society, the extended household seems to have played a greater role there, ideally and empirically, than in any of the other areas studied.

Second, the ideal and ideology of the large extended family may have been implemented by certain elite groups with the necessary resources or by groups confronted with special economic circumstances, but there is little solid evidence that any sizable proportions of past populations were affected by or desired to emulate these ideals in their routine family experiences. Both of these conclusions of course apply only to cultures for which we have admissible data.

This volume cautions at every instance against simplistic and hasty explanations of social change. It emphasizes the crucial importance of the developmental cycle which historians, despite their diachronic interests, tend to overlook. It warns against using or imputing "remnants" as a way of learning about the past. It questions what we might call the "corrosive" model of change whereby putatively "traditional" structures (e.g., a certain kind of family) suffered disintegration at the hands of some destructive impersonal force (e.g., individualism). Virtually all the essays are informed and indeed haunted by a concern with industrialization. Nowhere, however, are the actual connections between household/family and industrialization explicitly and systematically explored. Industrialization remains a portentous, all-embracing euphemism for change, at once process, state, periodization, etc. One wonders whether this concept, too, like that of the family, is not heavily marred by ideological and other extraneous factors and whether it would not be useful to subject it to the same sort of sweeping reexamination. In the same vein characterizing a social structure as "preindustrial" is really begging the question. To be sure, these matters are really beyond the purview of this book, and it would be unfair to belabor them.

The relevance of this pioneering work for social scientists, especially those concerned with social change, is of the first order. This careful, painstaking, arduous research by Laslett, his colleagues, and the contributors has to a large extent and with the explicitly cited caveats spared us from investing countless hours and energy in idle speculation and following false trails. At the same time it opens up a whole new mine of potentially fruitful materials together with techniques to exploit them that will illuminate the present by providing us with reliable benchmarks

from which to assess changes. Finally, with regard to industrialization and its correlates, these materials do not suggest the epiphenomenal nature of family structures but rather the necessity to examine more closely the simplistic formulations equating the nuclear family with industrialization and thus to direct our attention to other combinations of patterns and variables.

The Changing Family. By Betty Yorburg. New York: Columbia University Press, 1973. Pp. viii+230. \$9.00 (cloth); \$2.95 (paper).

Gaye Tuchman

Queens College, CUNY

Some of the academic publishing houses have recently "discovered" a new genre, the "minitext." A compact book that ideally attempts to integrate past research with new ideas, the minitext is designed to be accompanied by supplementary paperbacks because of its relatively low price. *The Changing Family* belongs to that genre.

Within the limits of the minitext, Yorburg succeeds quite well. In seven brief chapters, she outlines a theoretical context, discusses the biological base of family life, reviews types of human societies and varieties of families (emphasizing the significance of urbanization and industrialization to the contemporary American family), discusses ethnic and class variations in the United States (she views class as the key variable), and offers predictions about the future of the American family. To have packed this much material into 230 clearly written pages is no small accomplishment.

Yorburg's attempt to avoid some of the invidious comparisons between men and women that have characterized too many textbooks dealing with the family is equally welcome. For instance, rather than assuming that men have a stronger sex drive than women, Yorburg writes, "Individuals who are most liberal in their sexual practices are those whose ties to traditional organized religion and to conventional morality are weakest: the highly educated, men in general, blacks, political radicals, and non-churchgoers" (p. 63). Similarly, discussing the connections between biological mandates and the socialization of American adolescents, Yorburg notes some dysfunctions (although she does not use this term) of female socialization. She writes, "Feminine role prescriptions . . . ill-prepare females for the resourcefulness, impersonality, and self-direction that are often required in rapidly changing, bureaucratic and anomic American society" (p. 67).

Yorburg's discussion of biology sins by omission. She writes about hormones, chromosomes, aggressiveness, and territoriality in nonhumans, seeking parallels to humans, without introducing the work of John Money and his associates (*Sex Errors of the Body* and *Sex Research: New Developments*) or the article of Mary Jane Sherfey ("The Nature and Evolution

of Female Sexuality in Relation to Psychoanalytic Theory," *Journal of the American Psychoanalytic Association* 14 [1966]: 28-128). Money's work is crucial to an understanding of the biology of sex roles in its demonstration of the extent to which socialization may contravene biological "dictates." His work also suggests that both males and females are aggressive (the distinction lies in the modes of expression, as well as in levels of physical activity), and both Money and Sherfey discuss the ethologists whose work Yorburg draws upon.

Yorburg's use of the Parsonian distinction between instrumental and expressive roles in American families is more serious. To state that these concepts have lost their ubiquity, if they ever had any, is to lighten the matter. It is difficult to imagine the stereotypical "instrumental" president of a corporation who pays no attention to the social and emotional interaction of his vice-presidents. Similarly, it is difficult to conceive of the stereotypical "expressive" housewife who mediates the emotional needs of her family but neglects to perform such basic instrumental tasks as cooking dinner and cleaning the home. To ignore the multifaceted nature of male and female roles, the extent to which each role is both instrumental and expressive, is to perform a serious injustice to the sociological attempt to describe and analyze social reality.

Despite these flaws, Yorburg's book is a good one. Her emphasis upon the importance of social structure to family organization is frequently sensitive. For instance, rather than stressing the so-called pathology of black families, she has emphasized their strengths: "These distinctive variations in the husband and father role are in the direction of modern patterns in family life. . . . And these preexisting norms facilitate the adoption of urban values, given enabling economic opportunities for the black male. Whites, in fact, are moving in the direction of age-old black family patterns with respect to . . . declining role segregation in the marital relationship. . . . The relative absence of patriarchy . . . bodes well for the realization of two other modern, urban values—rationalism and achievement orientation. . . . Black women are, generally and from long experience, more pragmatic, more resourceful, and more flexible than women who have been protected within the patriarchal structure for centuries" (p. 143).

Equally valuable is Yorburg's analysis of the historical relevance of economic factors to family structures, although she does not mention the work of either Ester Boserup (*Woman's Role in Economic Development* [New York: St. Martin's Press, 1970]) or Valerie Kincaide Oppenheimer (*The Female Labor Force in the United States* [Berkeley, Calif.: Institute of International Studies, 1970]). Discussion of each would have added to her treatment of the family.

Yorburg's strength is her insistence upon the relevance of class to the structure of the family. She concludes the chapter on ethnic variations by stating, "Acculturation to urban values depends ultimately on the cultural baggage and, even more importantly, on the existence of real and effective economic and educational opportunity. Social mobility is the prize

and class location is the major and prevailing factor in the persistence of traditional values or the adoption of modern values in contemporary American life" (p. 152). *The Changing Family* can be called a successful minitext.

Colleges and the Urban Poor: The Role of Public Higher Education in Community Service. By Doris B. Holleb. Lexington, Mass.: Lexington Books, 1972. Pp. xvii+175. \$12.50.

James McPartland

Johns Hopkins University

Many recent books in the sociology of education strive to strike a balance between research findings or theoretical ideas and practical suggestions for changing public educational institutions. The most influential of these books organize their practical suggestions within a major theme that is derived from highlighting certain research results or theory approaches in the particular study. This book, which deals with the changing character of urban postsecondary educational institutions, places more emphasis on practical suggestions than it does on research or theory. Moreover, Holleb elicits policy suggestions from a compilation of as many pertinent facts as possible, without being highly selective about those with the most theoretical or practical importance. As a result the book is more likely to prove useful to educational planners who are looking for a variety of practical recommendations than to sociologists who are interested in new findings or ideas.

The author presents facts about two separate social phenomena: trends in the conditions of poor people in cities and the changing nature of institutions of higher education. The book uses previously unpublished statistics of Illinois educational and manpower institutions to provide a clearer and more intensive examination of these patterns than is usually available from national statistical sources.

Part 1 reviews the depth of the problems of the urban poor and the degree of recent changes in these conditions. Tabulations remind us that large concentrations of severe poverty remain in central city areas. Recent statistics again show that the disparities in employment, income, health, family stability, and education are greatest between certain central city districts and the suburban areas.

The second part of the book examines changes in enrollment practices of institutions of higher education as they have affected the minority and poor population groups. For the most part this section tells the story of the development of community colleges as elements of expanded state systems of public higher education. Among the most important facts demonstrated about Illinois are the degree of racial segregation on the separate community college campuses, the dramatic increase in the size of their enrollments during the sixties, the closing racial and economic gap

in initial enrollment rates in postsecondary schools, and the high remediation work and attrition probabilities experienced by particular students.

Although the first two parts of the book have some minor but distracting editorial shortcomings—such as the frequent use of cumbersome words or phrases and the occasional poor matching of tables with text—Holleb provides a good deal of valuable descriptive information. Because the book primarily aims to recommend specific college actions, however, it is not able to deal analytically with the existing facts and important unknowns. As the author recognizes, studies which would clearly answer a number of questions about colleges and the poor are not yet available. These questions include the relative importance of factors which inhibit student attendance at community colleges, the effects of specific programs of study on students' learning and occupational chances, and the role which segregation plays in these effects. Rather than including different possible answers to these and other questions in the analysis, Holleb, in arriving at practical suggestions, makes implicit assumptions about the unknowns. The sociologist consequently will find it difficult to draw from this book the clear causal models needed to generate future research.

The third part of *Colleges and the Urban Poor* moves beyond considering college programs for potential students from poor neighborhoods to questions of how college programs can affect the rest of the urban poor population. The author suggests that colleges should adopt and implement an expanded notion of community service in order to fulfill their responsibilities to the urban community. The book does not provide an analytic definition of expanded community service to guide this discussion. Instead, Holleb attempts to communicate ideas by drawing numerous distinctions, referring to an example of one action program described in an appendix, and listing various practical suggestions, which range from faculty consulting and extension services to instructional or research programs which give students practical experiences in urban neighborhoods. Throughout this section of the book the author cites general assumptions about community renewal programs such as the need for resident participation in the planning process, the training requirements for social service professionals, and the usefulness of many-sided approaches which somehow begin with single concrete objectives.

The general point is made that the instructional and research programs of urban colleges can be implemented in their local communities to the advantage of the neighborhood residents. Beyond this, the book does not provide a coherent theory of the relative importance of factors which will determine the success of such community institution building. Although it is not stated, the book implies that leadership from the existing personnel within the colleges is the primary determining factor and offers a variety of practical suggestions for these people.

This book will be received differently by different audiences. Those who are responsible for planning expanded systems of higher education will find numerous helpful recommendations. The book appears to be aimed primarily at this audience and succeeds best in this area. Social researchers

interested in the recent changes in educational institutions will find some interesting facts in the volume, but the book's emphasis on recommendations precludes the kind of analytic treatment of facts and remaining questions which would have been most useful for research and theory purposes.

The Political Web of American Schools. By Frederick M. Wirt and Michael W. Kirst. Boston: Little, Brown & Co., 1972. Pp. xv+285. \$7.95.

Paul E. Peterson

University of Chicago

This book "encompasses . . . many phases of the politics of education" (p. 241). It is a "textbook, research project, bibliographic essay, and statement of research directions" (p. 1). The first half covers the literature on political socialization, educational interest groups, "power structures" as they affect local schools, school referenda, and the state politics of education. It thus provides an extremely useful bibliography of a literature found only in widely diverse and often obscure sources.

But the review of the literature is somewhat less than distinguished; it does little to reconcile apparently contradictory discoveries or to draw interesting conclusions that might have been reached in the process of bringing together divergent findings. For example, the discussion of the power-structure literature fails to analyze the relative advantages and problems associated with each of the various approaches to the study of power relationships in local communities. Conclusions drawn from the discussion of this literature are so even handed as to be almost pointless: "The school system is caught between the forces of popular participation and bureaucratization. . . . Over time, the latter prevails, but also through time the influence of the former may be felt" (p. 77). Similarly, the authors conclude their rather uncritical review of the state politics literature with the not very surprising argument that "the influence of educational lobbies and parties varies [among states], as do the weight of governors on outputs, the interaction between interest group and legislature, and the force of direct democracy." In sum, these pages include good hints about recent writings but offer little in the way of interesting analysis and interpretation.

The second half consists of three studies of policymaking in education: a discussion of the implementation of the Elementary and Secondary Education Act, the implementation of the Supreme Court's school desegregation decisions, and curricular policymaking. The desegregation study is the most interesting, for it brings together a substantial body of literature which reveals the tactics and strategies followed by the South and the courts in pursuing their respective goals. Yet any study which suggests that there were no legal precedents for the *Brown* decision (p. 178) can hardly be considered definitive.

What holds the potpourri together are concepts taken from David Easton's *A System's Analysis of Political Life* (New York: John Wiley & Sons, 1965). Unlike Easton, Frederick Wirt and Michael Kirst do not think a political "system's capacity to persist is the most important aspect [of politics] to study" (p. 231); instead, they use Easton's framework only "to organize our data in some sequential process for describing the interactions of State and School in policy making" (p. 232). But Easton's theoretical framework, which focuses so explicitly on the mechanisms for reducing demand input, maintaining diffuse and specific support, and the feedback on these variables of system outputs, loses its coherence and logic if its focus on persistence is stripped away. What remains is a grocery list, a set of topics that have lost the conceptual interdependence which Easton intends to give them. Under the circumstances, systems analysis becomes a substitute for theorizing about the materials at hand, a sleight-of-hand that allows the authors to feel that they are systematically inter-relating a diverse set of materials when they never actually achieve such a goal. At times it seems as if the authors realize themselves that they are simply stating in systems language points that could be made just as easily (or more easily) in less technical language. The chapter on "Curricular Decisions in the Political System" is a somewhat revised piece published originally by Kirst and Decker F. Walker in *Review of Educational Research* (December 1971). It is remarkably free of the systems language used elsewhere, except for concluding paragraphs which summarize the preceding material in systems language. I, at least, could detect little additional insight gained by such a restatement.

Nonetheless, this study does identify a wide range of literature that touches on the politics of education, and it is sufficiently dull to become a rather widely used textbook.

Implementing Organizational Innovations: A Sociological Analysis of Planned Educational Change. By Neal Gross, Joseph B. Giacquinta, and Marilyn Bernstein. New York: Basic Books, 1971. Pp. vi+309. \$8.95.

K. George Pedersen

University of Victoria

This book, which describes in pedantic detail the case study of an educational innovation involving 11 teachers in a single elementary school, provides the results upon which to challenge the so-called power-socialization thesis of organizational innovation. While there is much to commend in this research, including its conceptualization and the meticulous care taken in its completion, it is unfortunate that over 300 pages had to be devoted to a description. Neal Gross and his associates probably should have considered a more modest forum for describing their work, possibly in the form of a monograph or even an extended periodical publication. In fact, the plethora of detail and repetitiveness distract from the genuine

research value, at times reminding one of the prose which often characterizes graduate dissertations.

Like a number of current publications which focus on the topic of social change, this study was developed as a challenge to the rather simplistic notion that the basic deterrent to organizational change is the initial resistance of the constituent members. An extension of this premise suggests that innovative success is largely a function of the management's ability to overcome this reluctance on the part of the membership. The authors of this book make the case that the change process is highly dynamic and complex, characteristics not adequately reflected in earlier research in this area. They suggest that the more commonly accepted diffusion/adoption model used extensively by rural sociologists and others is inappropriate to the school setting and that, in general, previous change research has suffered from both methodological and conceptual shortcomings. Instead, it is argued, the many obstacles which are confronted by anyone interested in organizational change, the role which management may play in overcoming such obstacles, and the possibility of negative responses by initially supportive participants are all dimensions of the innovative process which need further study.

The innovation studied essentially concerned a redefinition of the role of the classroom teacher. This "catalytic role model" was conceived as one which would emphasize the educational process, rather than the subjective content. The innovation sought to encourage children to seek topics of interest under conditions of maximum freedom, with the teacher serving as a resource to facilitate the appropriate environment for such learning. This catalytic or guidance function was intended to ensure that the student would assume primary responsibility for directing his own education, an ideal goal frequently accepted but rarely attained in the formal schooling environment.

According to the authors, a case-study methodology was employed in order to (1) extend the knowledge base concerning the conditions which aid or hinder organizational innovation; (2) determine whether earlier research in this area evidences large omissions; and (3) define the importance of the management role in implementing the change process. Unfortunately, fiscal constraints limited the study to a relatively small elementary school in which it was anticipated that faculty members would have attitudes positively related to educational change.

Findings described indicate that both the quantity and quality of innovative effort were minimal. This modicum of success was attributed to a lack of clarity concerning the innovation itself, a lack of ability to perform in the new role, a shortage of appropriate resources, an organizational structure inadequate to the needs of the innovation, and an overall dearth of participant motivation. A further analysis of these five impediments revealed some of the more basic causes of the various difficulties encountered. In essence, however, the underlying problem reflected the failure of administrators to anticipate or resolve those problems to which teachers were exposed when they were asked to initiate the innovation.

In this study a serious effort has been made to move to a more adequate conceptual framework within which to consider social change. The authors are sensitive to the inherent weaknesses of their work, taking care to suggest further levels of inquiry. It seems unfortunate, however, that a more than adequate study which undoubtedly enriches the literature in the field suffers from being written in an extended and unexciting style.

Graduates: The Sociology of an Elite. By R. K. Kelsall, Anne Poole, and Annette Kuhn. London: Methuen & Co., 1972. Pp. 284. \$12.00.

Oliver Fulton

University of Edinburgh

This book takes its place in a distinctive tradition in British social research. Its central theme is the inequality of opportunity in British society, but the authors add an examination of sex inequality to the traditional concern with class inequality. Previous investigations have documented differences in access either to education or to occupations, but R. K. Kelsall, Anne Poole, and Annette Kuhn have been able to trace the progress of university graduates out into the occupational world and so to directly test the theory of "sponsorship mobility."

The book is based on a large mail survey, conducted in 1966, of most 1960 graduates of British universities. An overriding concern for a high response rate (they achieved 80%) led the authors to omit "sensitive" attitudinal data (p. 10) from their questionnaire and to concentrate on educational and occupational history and occupational values. They use this limited material imaginatively, and their data are obviously reliable and valuable; but much of their more hesitant speculation could have been answered by a few, not necessarily intrusive, questions. Basic summary data from the study, along with methodological details, have been published separately (*Six Years After*, by R. K. Kelsall et al. [Sheffield: Higher Education Research Unit, University of Sheffield, 1970]). Although tables crucial to the argument are reproduced in *Graduates* (which contains 82 tables in all), there is no copy of the questionnaire; given that both books are slim, it is irritating to have to refer to the earlier volume for this and other basic information. The book's style is somewhat uncertain: written partly for sociologists and partly for a wider audience, it often presents an indigestible mixture of technical argument and elementary exposition.

The first chapter ("The Recruitment of an Elite") provides a familiar portrait of a stably stratified society. "Class chances" are calculated, based on graduates' fathers' occupations: most dramatically, the children of professionally employed fathers had almost 100 times the chance of entering university (in 1956/57) compared with unskilled manual workers' children. Moreover, large numbers of students apparently from "routine nonmanual" or "skilled manual" backgrounds were found to be "sub-

merged" or "latent" middle class. The authors heavily qualify their use of the term "elite," but they vividly document the existence of "a massive component within elite groups of people whose families have enjoyed very high status for at least three generations" (p. 58).

Perhaps less predictable—and more depressing—is the discovery that the university is not a homogenizing institution. The few working-class students do not acquire the ambitions, let alone the job prospects of their middle-class contemporaries—not even at the most selective universities. "Sponsorship mobility" may be a societal ideal, encouraged by the stratified school system and the even more selective higher education system of the late 1950s, but the reality is an unequal "contest." "The most important single destiny of the educationally successful working class child . . . is, as a schoolteacher, to help prepare the sons of the middle class for those very positions to which by virtue of his upbringing, he has by and large failed to obtain access" (p. 111).

Turning to the fate of women, six years after graduation most women (71%) were married, most of these were not working, and of the working married women, many were part time. Even more than working-class men, the vast majority of working women were employed in education, presumably schoolteaching. (The authors discuss at length the compatibility of schoolteaching with traditional women's roles, then—maddeningly—lump it in with the "real" career of university teaching as "education" in their tables.) The crucial questions about sex-role attitudes were not asked; the answer—that women respondents had semitradeional "family-first" views—is inferred from their job history and from other research. Given that in some fields, such as industry, more than half the women had experienced prejudice (though elsewhere most were unaware of it), and that in all fields, their jobs were markedly less satisfying than men's, it seems a bit hard to blame female reluctance to enter careers on an internalized ideology to the exclusion of external constraints. I suspect that most active British members of women's liberation have similar job histories.

If it is taken as a portrait of the impact of university, the study is limited; its subjects entered the British university system 17 years ago, when it was far more selective than it is today. Britain is moving toward mass higher education, and each year's graduating class looks less like an "elite," however loosely defined, than the one before. Still, the study provides a wealth of information on highly educated British 35-year-olds. (As well as the topics mentioned, there are sections on choice of university, of subject and of occupation, on subsequent job history, on graduates' [incomplete] marriage and family-building histories.) But if the exclusiveness of a degree in 1960, with its supposed connotations of "sponsorship," did so little to mitigate the class backgrounds of its new possessors, its mass equivalent, with internal status differences, does not seem likely to improve matters for its recipients from lower-class backgrounds. No wonder that British educational reformers are now attempting more selective intervention elsewhere in the life-cycle, to try to modify "life chances."

Education and Emigration: Study Abroad and the Migration of Human Resources. By Robert G. Myers. New York: David McKay Co., 1972. Pp. xviii+423. \$8.95.

Dennis R. Maki

Simon Fraser University

While an attempt is made to embed the analysis in the broader framework of evaluating educational exchange programs and their role in development, and the role of international migration of talent more generally, this study makes its major contribution in analyzing the narrower area of nonreturn of foreign students studying in American postsecondary institutions. *Education and Emigration* focuses on analysis of policy recommendations to alleviate the nonreturn problem found in the literature, noting the dangers of using unreliable data and performing partial analyses which ignore crucial interactions.

Much of the book is devoted to presenting the results of two empirical analyses: one using data from the Institute of International Education (IIE) Foreign Student Census, 1964-65; the other using data the author gathered on Peruvian students in the United States in 1966. A number of techniques of data analysis are utilized, with some details and technical comments; for example, on probable nonresponse bias, reported in appendices. The empirical work is very carefully and competently done and clearly reported. Perhaps the major problem with both data bases is that nonreturn is measured by expressed intent before the fact. The literature does not provide strong evidence about the extent to which these intentions might be actualized. The author notes that a longitudinal sample would be very useful in assessing the reliability of existing data.

Using the IIE data, an analysis is performed on the correlates of nonreturn. Results indicate that the following students are more likely to remain in the United States: undergraduate and doctoral students (compared with masters' students), students under the age of 20 and over the age of 40 (compared with those in intermediate age groups), self-sponsored students (relative to sponsored), and immigrant and to a lesser extent student visa holders (compared with those with exchange visas). The author points out that policy recommendations should not be made from these results.

Cross-national rankings based upon the same data measured losses through nonreturn by country relative to three different bases: the number of students from that country studying in the United States, the number of students from that country studying at home and abroad, and the demand for high-level manpower in that country. The three measures produced markedly different rankings. In what is probably an attempt to develop a less ambiguous index of the gain or loss to a foreign country due to international educational exchange, the author discusses the human capital approach. The treatment of this approach throughout the book is curious, if not inconsistent. Considerable space is devoted to elaborating

the human capital concept and reviewing the literature, and present values of expected earnings in both the United States and the home country are calculated for the Peruvian student sample and tested for their influence on nonreturn.

The author is nonetheless critical of previous attempts to measure human capital gains and losses from study abroad, noting that they "may be more worthwhile from a propagandistic viewpoint than they are representative of 'true' human capital value" (p. 184). It is entirely consistent to agree that a concept is useful while disagreeing with the manner in which it is used. Myers explains that "as such calculations are incorporated into social decision models, their utility will become clearer" (p. 199). Despite some subsequent discussion of "social decisions," however, he never pursues this train of thought. The reasons for this are not clearly elaborated, so that one must intuit several arguments. The net loss of human capital through nonreturn could be argued to be important only as it relates to the foreign country's ability to absorb this loss (p. 198). One could also argue that reliable computations are very difficult (p. 296) or that they are not really necessary: "One cannot help but be impressed by the positive role of migration for study and by the minor place of emigration through non-return in the total picture" (p. 359).

Yet policy recommendations are made regarding ways to reduce nonreturn. These are primarily suggestions to guarantee more students employment upon return and to investigate loan schemes with forgiveness clauses conditioned upon return. The reader is left with the impression that the author is not in favor of using the human capital approach to estimating gains and losses, although neither reader nor writer is sure of the reasons.

The book contains a concise literature review and is extensively footnoted. I recommend it as required reading for all those concerned with the "brain drain"; it will also be useful to others concerned with the general migration of human resources.

Explorations in Mathematical Anthropology. Edited by Paul Kay. Cambridge, Mass.: M.I.T. Press, 1971. Pp. xviii+286. \$12.00.

Mark Granfors

University of Chicago

Despite recent advances in the construction of formal models of social structure, the formalization of theories of cultural systems has been largely neglected. The papers in this book, most of which were presented at a 1966 AAAS symposium on mathematical anthropology, represent a first attempt at filling this void. The papers, written by anthropologists (some in collaboration with statisticians), are primarily methodological, dealing with the measurement and formal description of cultural phenomena and their relations to social structural features.

A first set of articles concerns the structural configurations of sets of cultural elements. John Boyd addresses himself to some of the problems relating to the arbitrariness of componential representations of kinship terminologies. (A componential analysis renders the meaning of a set of words pertaining to a specific cultural domain in terms of intentional connotations.) Boyd's work is related to that of Harrison White (*An Anatomy of Kinship*, Prentice-Hall, Inc., 1963), but is more general in that it is applicable to all kinship systems, and may perhaps be extended beyond the domain of kinship. William Geoghegan concretizes abstract componential analyses by demonstrating a method for determining the most economical means for sequentially processing the information perceived in a cultural object. The components are placed in some order such that, on the average, the number of decisions necessary to name the object is minimized. While the presentation is a model of the axiomatic method in its rigor, it seems unnecessarily tedious at points (theorems often convey little information not already in the definitions); the gist of the method is provided in the flow diagrams, which are more comprehensible. Roger Keesing, in a related paper, demonstrates the possibility of constructing whole ethnographies through the use of flow diagrams, where the decisions account not only for the assessments of objects, but behavior as well. Roy D'Andrade attempts to predict terminological features from the organizational aspects of kinship systems. In particular, the types of distinctions made in uncle and cousin terms are related to one another and to residence rules.

Another set of papers deals with the construction of indices to measure similarities in cultural features. Volney Steffire, Peter Reich, and Marllys Steffire consider three types of contextual similarities between the words in a language: grammaticality, semantic acceptability, and truth-value as judged by informants. A computer program is used to form clusters of terms which are compared with clusters directly compiled by informants. The method, however, may require several months of informant training before results become reliable. A paper by Harold Driver and Peggy Sanday demonstrates the use of factor analysis in measuring the similarity of various types of cultural features (kinship terminology, residence, and avoidance patterns), although in another paper, by Sanday and Giandomenico Majone, the use of correlation as a measure of similarity is criticized. Here a general axiomatic approach to the problem of distance/similarity measures is proposed, and several indices currently in use are related. A reanalysis of the Driver and Sanday data is provided for comparison.

Another type of index construction, a methodology for dealing with problems of the degree of integration of a cultural pattern, is demonstrated in two papers by John Roberts and his colleagues. Robert Kozelka and Roberts, working with "concordance" statistics which measure the agreement among a set of judges of the rankings of a group of cultural objects along a specified dimension, offer an approximate statistical method for comparing two concordances. Roberts, Richard Strand, and Edwin Bur-

meister present a method of formally describing and mapping preference patterns between varying amounts of each of a pair of objects, and of comparing these patterns across individuals and cultures. The primary example is tailored clothing, but another, of interest to family planners, gives maps of preferences in numbers and sexes of children. I am somewhat skeptical of the validity of exact psychophysical measurements (required by Roberts's method) although the agreement among American subjects in the clothing example is impressive, showing that it is indeed a well-integrated cultural pattern.

Other papers are of more specialized interest or present methods which are already familiar to sociologists. Hans Hoffmann demonstrates the use of Markov chains, John Gilbert introduces computer methods (in kinship studies), and Benjamin Colby ingeniously analyzes folktales, using Philip Stone's General Inquirer program. A paper by A. Kimball Romney demonstrates a method of measuring endogamy and other marriage patterns among groups independently of demographic variation within the groups.

Finally, the article by Eliot Chapple, in contrast to the others, attempts to develop a theory of interaction based on individual biological rhythms rather than cultural rules. The piece is a programmatic one, for a precise mathematical formulation remains to be developed.

The Dialectics of Social Life: Alarms and Excursions in Anthropological Theory. By Robert Murphy. New York: Basic Books, 1971. Pp. x+261. \$7.95.

Peter J. Newcomer

University of Connecticut

Robert Murphy has given us a lively and readable history of recent anthropology, along with a much less penetrating analysis of its ills. His discussion, for instance, of the peculiar split between the British and American wings of the discipline remarks upon the large size, eclecticism, and amorphousness of the content of American anthropology and contrasts it with European scholarship, characterized as "personalistic and restrained, rigid and closed" (p. 20). In one of his epigrammatic paragraph closures, Murphy sums up British social anthropology with the remark that "its products think well, but they don't think much" (p. 20).

More important than Murphy's information or style of presenting recent anthropology is the fact that, with forceful reasoning, he explains why these differences came about. They are not, he says (p. 17), "so much a matter of intellectual influences as a function of the prevailing rules of tenure and mode of academic organization," and he shows how the boom in American university positions coupled with the appearance of the research grant have turned every American academic into an independent entrepreneur, with the by-now-familiar proliferation of ideas, theories, and "approaches" as a result. The static unexpanding nature of

British academic employment and the rigid unchanging demands of the Colonial Office have stamped an equally strong but very different mark upon their portion of the profession. This is good historiography; it not only presents the facts but accounts for them in a persuasive, processual way.

One wishes Murphy could design as good a program for the analysis of alien societies as he has for his own situation. Murphy's book remains as frothy and aphoristic as ever but loses a great deal of its persuasive power after he completes his capsule history and begins criticizing contemporary theory: at this point he abandons the habit of asking why and how things came to be the way they are. Although he finds it necessary to show us why British anthropology is what it is, he is perfectly willing to accept Durkheim's explanation that norms "are like molds in which actions are inevitably shaped" (p. 38), that they originate in social life to shape the will and action of the individual. But what determines these norms? Norms shape action; does anything shape norms? In this instance Murphy is a better practitioner than theoretician; he gives us the material basis for the norms of British social anthropology, but apparently he does not require others to specify the bases of ideas.

This discussion approaches the central riddle of the book: "What are the roots of the social order?" (p. 61). Or, phrased less philosophically, "Why doesn't it fall apart?" (p. 60). Murphy never solves the problem; on page 238 he asks again, "What are the roots of activity? What indeed are the social conditions that keep people oriented toward the outer world and impelled to do things—all sorts of things?"

Although Murphy refuses (p. 158 and elsewhere) to debate idealism versus materialism, his refusal does not eliminate the problem of the causes of social life. Bluntly conceived, it is possible either to agree with Marx that human beings have to organize socially to produce what they need to live or to attribute the roots of social organization to structures in the brain—innate ideas. It seems clear that the idealist-materialist debate, while somewhat unfashionable these days, must nevertheless be confronted by every social scientist. Murphy's very basic uncertainties become the penalty for failure to face the issue.

Having found nothing in which to root society—as he says, anthropology has destroyed a "human-nature" view of society but has put nothing in its place—Murphy is hard pressed to account for the variation among human societies. Since he has eliminated a simple "personality-writ-large" method, he tends toward a Lévi-Straussian view that all cultures express the same structures differently. In what some will see as a vulgarization of French structuralism, Murphy perceives the dialectical structure as the tension (and slippage) between thought and action, norm and activity.

Although Murphy goes to some trouble in the earlier chapters of his book to criticize positivism, his approach is ironically positivistic; that is, he offers indisputable facts as guiding theoretical principles. We know that thought and action diverge, but how does this gap cause sociocultural life to be as it is? We know that man's history is filled with contradictions.

The dialectician's question becomes, however, not "how does man's history make contradictions?" but rather "how do those contradictions make his history (and the present) what it is?"

Dialectics is not simply the study of contradictions. It is the study of a process (or development, or evolution, or history) propelled by the appearance and attempted resolution of contradictions. The study of oppositions, contradictions, etc., is not per se a dialectical enterprise. Claude Lévi-Strauss is not a dialectician and neither is Robert Murphy. Since the contradictions they study have no material roots, they can have no material social outcomes; the Bororo are as inextricably stuck in the ethnographic present as we ourselves are.

It is clear that any work which offers itself as a guide to method has two essential tasks. First, it must clarify the nature of the subject matter: in this case by answering the question "why is social life the universal mode of human existence?" Second, it must offer some opinion as to how that subject matter develops: "What is the process by which social systems change?" This is of course the question that Darwin was able to formulate for biology but which social scientists fail to confront. Dialectics was developed by Hegel and given material force by Marx as a method for solving this question, but Murphy's version is only a pale reflection, a weak offshoot, of this tradition.

Elites and Change in the Kentucky Mountains. By H. Dudley Plunkett and Mary Jean Bowman. Lexington: University of Kentucky Press, 1973. Pp. xii+204. \$11.00.

Dwight Billings

University of North Carolina

There are at least two conflicting explanations for the persistence of poverty in Appalachia. One stresses the region's traditional culture—its fatalism and familism—which ill-equips its population for participation in the modern world. A second perspective blames the region's dependence on the coal industry, which strips the land of its wealth and impoverishes its people. In view of this controversy, it is interesting that a book co-authored by economist Mary Jean Bowman, whose previous work includes an exhaustive analysis of the coal industry in eastern Kentucky, should line up so unequivocally on the side of the culturalists.

Elites and Change in the Kentucky Mountains is addressed to practitioners in Appalachia, but the authors suggest its wider applicability. However, I did not find the book of great value. It suggests some interesting ideas, especially the role of information in community change, but its research design is unsuitable for developing these ideas. The book shares the faults of other superficial research on Appalachia. Without institutional analyses the interests of local elites and their role in social change remains obscure. Without comparative data from their (or any other) published

study the authors take for granted the cultural distinctiveness of the region. (In some recent attitude research, for instance, I was unable to distinguish Appalachian from other rural southern respondents.) In the absence of good social history, it is easy to assume the region's isolation and imagine its "frontier" culture inhibiting social change. But eastern Kentucky is not the medieval society to which concepts borrowed from Tonnies or Redfield are applicable—it is integrated with a modern industrial economy through its coal production. From this perspective its poverty is not a consequence of insufficient modernization but a result of a particular kind of economic development and its political consequences.

The book assumes a Durkheimian change model which conceives modernization as the differentiation and enlargement of scope of social organization. This implies that the result of increased contacts between local areas and their wider social environment is greater social density and cultural integration with the "outside" social system. Unlike some students of Appalachia who focus exclusively on mountain isolation, H. Dudley Plunkett and Bowman borrow a diffusion-and-resistance model from cultural geography to emphasize extraregional contacts. The authors assume that human resource development is the most appropriate goal for eastern Kentucky, an area they see as having very limited economic growth potential. The purpose of their study is to discover what local resources are available for preparing youth for successful out-migration. It is assumed that the traditional culture does not facilitate this process. Their research question is: do mountain elites carry attitudes conducive to modernization? In particular, they try to identify "interstitial persons" whose strategic occupational locations in local and extra-local systems make it possible for them to function as "cultural bridges." These leaders must be sympathetic to both mountain traditions and modern values in order to bridge the "[cultural] gulf between region and nation" (p. 84). An obvious flaw of this study, which the authors clearly acknowledge, is that such an approach demands both thorough institutional analysis of the region and content analysis of communications within the region and between the region and its environment. Instead, what Plunkett and Bowman provide is a microlevel attitude survey of a sample of local occupational elites.

The survey findings are mixed, but the authors are not sanguine about prospects for change. First they measured the effects of occupation, age, and nativity on modern outlook. They found three occupational groups with, respectively, decreasing commitments to change: "ministering professionals," businessmen, and the local establishments of bankers, lawyers, and politicians. On the positive side the survey revealed that a "sizeable minority" of the occupational elites had had extensive extra-regional urban experience. They found younger members of the elite, and those born outside the region, relatively progressive. Surprisingly, the non-Baptist clergy appeared to be the most change-oriented group in the Kentucky mountains. Male school teachers were found to be moderately progressive but, as Plunkett and Bowman note, too often school positions are seen as political

prizes and as a rule public officials are "uncritical and strongly defensive" (p. 90) of local schools. Of this latter group they write (p. 87): "In view of the gerontocratic and strongly native character of the administrative elites, with their key position in local community affairs, the prevalence of unprogressive attitudes among them is one of the more serious stumbling blocks in the way of progressive change." As this quote suggests, the authors are interested in how attitudes relate to community participation. They learned that people highly involved in civic (as distinct from political) affairs are highest on modernism scores and highest in youth involvement. However, their influence is countered by members of the political elite—and by those strategic persons whose community participation is both civic and political—whose attitudes are traditional and localistic.

The Liberalization of American Protestantism: A Case Study in Complex Organizations. By Henry J. Pratt. Detroit: Wayne State University Press, 1972. Pp. 303. \$15.95.

Andrew M. Greeley

National Opinion Research Center

To begin with, *The Liberalization of American Protestantism* is a badly titled book, unless one equates the bureaucratic structure of the National Council of Churches with the whole of American Protestantism. *Liberalization of the Protestant Vatican* might be a more appropriate title, since the book deals with the turn toward social activism by the Riverside Drive bureaucracy during the 1960s.

Henry Pratt tells the story of this shift in emphasis and gives a low-level theoretical explanation for it; that is, the bureaucrats on Riverside Drive were able to "co-opt" (he does not use the word) other bureaucrats in the regional and local councils of churches and in the most active denominational affiliates of the NCC. Thus, despite the fact that there is little reason to believe the rank-and-file members of the various denominations had much sympathy with the activism of the bureaucrats, these latter were still able to put "American Protestantism" on record as supporting those fashionable causes that all good liberals supported in the 1960s. What the costs of this support might be is not an issue to which Pratt addresses himself.

As a schematic history of a bureaucracy the book is moderately interesting; as a contribution to the theory of complex organizations it is only of marginal importance. What Pratt set out to do he has done competently. One cannot fault an author for not assuming tasks additional to those he assigns himself.

Yet there are other questions that I hope some author will investigate. First of all, I wonder how effective the activism was. It was undoubtedly very satisfying to the bureaucrats themselves. They could go to East Side cocktail parties and hold their heads up high. Second, it provided employ-

ment for a number of deserving clerics; white and black, and in this day and age that is not to be eschewed. Third, it provided the basis for mass-media coverage for such colorful personages as Benjamin Payton and James Forman; coverage which publicized Payton's calling Daniel P. Moynihan a racist and helped to scuttle family assistance during the Johnson administration. The media also provided a showcase for Forman's demand for "reparations" from the white churches (which the Episcopalians actually paid). Whether any of these unquestionably admirable accomplishments had much to do with improving the lot of black people in America, however, remains to be seen.

Pratt's claim that NCC pressure was responsible for the passage of the Civil Rights Act of 1964 ought to be taken with a grain of salt. Citing Richard Russell's comments as "conclusive proof" that the NCC turned the tide on the cloture vote is not unlike the devil quoting scripture. This claim may also concede far too little to Lyndon Johnson's parliamentary skills.

Another organizational question that might be asked is how a bureaucracy legitimates for itself an activist intervention which, however admirable it seems in practice, may run counter to its own most deeply held theories. The elite clergy who staff the NCC have all been deeply influenced by Paul Tillich. I suspect that they would deplore, as he did, the "idolatry" of identifying contingent and relative social programs and policies with the message of the Gospel. Yet in practice they put Christianity officially on the line in support of very contingent social programs that, however excellent they may be, are still bound to be inadequate and imperfect and perhaps even unsuccessful. Liberal Caesaropapism is still Caesaropapism. I know from conversations with the denizens of Riverside Drive that at least some of them were troubled by this dilemma.

It may turn out that indeed they ought to have been. In any case, studying the resolution of this dilemma and its after effects would make a fascinating research project.

Motives and Goals in Groups. By Alvin Zander. New York: Academic Press, 1971. Pp. xiv+212. \$11.00.

John M. Sharp

University of South Carolina

This book brings together a series of mostly previously unpublished experimental and field studies, many of them made by Alvin Zander and his associates, which describe small groups engaged in repetitive tasks. The tasks are such that the group members are interdependent: outcomes of each trial are measurable and aspirations for the next trial collectively determined.

A complex chain of events is presumed to occur in the course of each trial. First a level of aspiration or goal is collectively determined. This

process implicitly involves a perception of the probability of group success in obtaining that goal, which in itself can influence the eagerness to approach the task. The task is then performed, with either success or failure in achieving the previously set goal. The outcome leads to members' evaluation of the adequacy of the group performance, which influences the level of motivated beliefs (desires for further participation in or for the avoidance of the group task). Given these results, the group then sets a goal for the next trial, and the entire causal sequence is repeated. The level of the goal, which influences the probability of success, and the motivated beliefs of the group are seen as particularly crucial variables in this sequence.

The jacket cover indicates that the book should be of highest interest to those who work either in or with groups: psychologists, educators, social workers, executives, and others who seek to understand the nature of group motivational processes. This impression could indicate that the book offers advice in improving group motivation. Instead, it is descriptive—reporting on a series of well-done experiments—and theoretical—attempting to interpret disparate conditions influencing group goals and performance through the concept of group-motivated beliefs, a group desire to try harder. Although Zander is not prescriptive, the following conclusions he reached might be of interest to the practitioner:

1. Nothing succeeds like success. Successful groups keep raising their goals, and, if they have been successful repeatedly, they have increased interest in the group doing well, confidence, and the ability to achieve higher goals. (Failing groups, on the other hand, tend toward lower goals and may enter a cycle of failure, losing interest in the task at hand.) A goal must not be too low, for this causes lack of interest in attempting to achieve it. In order to keep the group trying harder there must be a moderate, perceived probability of the task failing; here care must be taken, since, empirically, failures to meet goals are somewhat more likely than successes. Thus the group motivational level must be carefully shaped by a high level of success at ever more difficult tasks.

2. The reward of success is a better motivational tool than the price of failure. Coercion or costs for failure can induce avoidance of the task.

3. The achievement motivations that individuals bring to groups are factors to be considered, especially the tendency to avoid failure, since the easiest way not to fail is not to try, or, if the effort cannot be avoided, to set unrealistic goals so that failure will not be so punitive. Such negative motivations can be controlled by putting the affected individuals in important positions where they must perform to ensure group success (in part due to obvious feedback), or by slowly raising expectations for their performance levels in a shaping-of-behavior process. Complete feedback on both group and individual performance is important to improving outcomes. Thus, although a person may need to avoid failure, proper arrangement of conditions can induce him to try hard in the context of the group. Group success at difficult tasks improves individual self-concept, while

failure hurts it, unless the member can blame others for the failure since his role was peripheral.

4. Through comparison or by fiat, higher goals can be imposed from outside without harming performance, if a shaping orientation is again applied and goals are not too drastically increased.

While the author's conclusions are well supported by his experimental and field survey data, the concept of individual desire for group success is useful only in a theoretical sense. Zander made extensive use of a group analog to the Atkinson model of achievement motivation in his theoretical discussions. At the present time, however, the concept of desire for group success is probably not empirically viable in the sense that it can be independently and reliably measured in a given group. Doris Entwisle ("To Dispel Fantasies about Fantasy-based Measures of Achievement Motivation" (*Psychological Bulletin*, vol. 77 [June 1972])), in an excellent attempt to measure the reliability of fantasy-based achievement-motivation measures, found the average reliability coefficient to be only .30. Since validity is limited by reliability, she could only conclude that the concept of individual achievement motivation is not empirically valid. The problems in reliably measuring a group level of achievement motivation would appear to be far greater, since results obviously do not reflect a summation of individual motivations. Thus, unless reliable measures of the concept are developed (independently of difficulty of goals or actual performance of groups), the group desire for success remains a theoretically interesting but empirically useless concept.

Health Care: Can There Be Equity? By Odin W. Anderson. New York: John Wiley & Sons, 1972. Pp. xviii+273. \$11.95.

Robert N. Wilson

University of North Carolina

As adequate health care moves closer to becoming a commonly accepted right of all citizens in the Western liberal democracies, including the United States, questions concerning the effective distribution of that care inevitably emerge as significant political issues. Health, clearly too important a matter to be left to doctors, moves foursquare into the arena of public policy debate. In the delicate priority system characterizing even the most abundant society, health now ranks with urgencies such as well-being, national defense, and education. In his finely balanced and wise study, Odin W. Anderson makes a persuasive case for the essential similarity of the health service systems in the United States, Sweden, and England. His low-keyed but firm examination provides a welcome contrast to the shrill all-or-nothing rhetoric and assumptions exhibited by many contemporary critics of the American "nonsystem" of health care.

Anderson sets the stage for his account by clearly describing the ele-

ments of a health-care system, the relatively invariant features that any care-giving framework must embody, whatever the details of financing and organization. He then offers a compelling overview of the core values and problems of the three countries under scrutiny. In a chapter entitled "The Liberal-Democratic Political and Economic Matrix" we are presented with the shared aspirations and dilemmas of these societies as they go about what is, in historical and cross-cultural perspective, the astonishingly bold task of trying to provide a high quality of life and health for all their citizens. In particular, Anderson notes that the three countries share two fundamental prerequisites of such an effort: (1) a sufficiently affluent economy to allow for certain social options; (2) a polity increasingly dedicated to a concept of at least limited equity in the allocation of goods and services and to the proposition that there is some floor of life circumstance below which no one must be condemned to fall. This truly excellent analysis strikes a note that recurs throughout the book, namely, a sophisticated linking of health care to other salient aspects of a social system. Anderson writes as a very aware general sociologist, as well as a specialist in health studies.

After lucidly and compactly tracing historical developments of the three health systems, Anderson attempts the difficult task of comparing their efficacy. He proposes two chief axes of comparison. These are termed "adequacy" and "equity"; adequacy refers to the quality of care delivered, equity to the fairness of its distribution. In each instance the analyst confronts extremely difficult problems of measurement; there are no entirely agreed-upon ultimate indices of the quality of a society's health or of the fairness of the allocation of services. The quality of care may be assessed in several different ways or in combinations thereof. Simplest, but far from satisfactory, are the vital statistics of mortality; using these measures, the three countries prove roughly similar in the causes and rates of death in various age groups. Among the exceptions, however, are the well-known excesses of maternal and infant mortality rates in the United States. Anderson shows convincingly that invidious comparisons here may be quite deceptive given the dissimilar populations involved: the large black population whose high rates inflate the American figures. He argues that "mortality rates result from a multiple of causes," extending well beyond the health system as such. The elimination of poverty would be likely to have more effect on reducing U.S. mortality than simply improved health services. Perhaps the major conclusion here is that "it would seem to be a reasonable assumption that health services as such have less influence on mortality rates than general social conditions in economically developed countries."

Quality of care may also be gauged, potentially, by morbidity data. Again in this case, although these data are not as yet completely satisfactory, the three societies, and indeed all advanced industrial societies, show relatively little difference. Finally, the search for a measure of quality may entail an effort to tap the felt satisfactions of members of the population—their subjective judgments about the quality of life and health

care. The author observes that these nonbiological but significant "pay-offs" have not yet been conceptually worked out, nor have techniques for their measurement been developed.

Equity of care finds the United States trailing both Sweden and England, which should come as no surprise to those familiar with the operation of the health marketplace in this country. Anderson believes that at root the health-services system is an expression of a society's "concept of distributive justice"; he is moderately optimistic that America's emerging concept will in the next decade move us somewhat closer to England and Sweden, without sacrificing what he terms the "dynamism" of the pluralistic ferment we now enjoy in health matters.

In sum, Anderson presents us with a properly complicated view of what health services may be expected to accomplish. They simply do not yield to a model of perfect rationality. Although Anderson may be too sanguine in his outlook for piecemeal reform of the American system (even his modest suggestions would cost a great deal of money), he has given us an immensely valuable study.

The Police Academy: An Inside View. By Richard N. Harris. New York: John Wiley & Sons, 1973. Pp. xiii+197. \$9.50.

John H. Faris

University of Chicago

This book presents a case study of police training. The author participated in the 12-week training academy of "Rurban County Police Department" in 1969. True to its title, the study provides an inside view of the relationship of the socialization process to the role of policeman.

The strength of this volume is the frequent use of direct excerpts from the author's field notes, both of classroom instruction and of interaction among recruits. The following excerpts, for example, are used to support Harris's contention that the police organization is a "defensive bureaucracy": "We have Community Relations because the people are more educated than they were before. They also complain more. So the only way we defend ourselves is more awareness of the need of police-community relations techniques. We can't survive in today's society if we are just a punishing or law enforcing agency" (p. 36; police instructor in community relations). "There was this young kid who hadn't been on the outside much; he was on desk jobs all the time. One night a car sideswiped him. He grabbed it, got out, and identified himself as a police officer. They grabbed his nightstick and beat the hell out of him, so he drew his gun and killed one of them and shot the other. He didn't drink much, so the paper hinted he may have been coming back from a party or stopped for a drink which could have been too much for a person who didn't drink much. That's what we're up against, Rich" (p. 51).

Harris clarifies the magnitude of the discrepancy between what police

training purports to be and what the effects of training are. The pervasive feeling among recruits that they are wasting their time in the academy is encouraged by older officers, and, with more subtlety, by instructors: "Don't worry about that now. Wait till you get out of the academy. In here, we stick by the book" (p. 87). The community relations segment of the curriculum boomeranged. "Probably, as far as the recruit was concerned, the first two sections confirmed his suspicions about the irrelevance of the Community Relations Squad, added to his hostility toward a depersonalizing public, and reinforced his prejudices against minority groups" (p. 145). "Every time they discuss this shit, they have a colored guy up there" (p. 145; police recruit). Harris makes dubious the assumption that increased or "improved" police training will have the intended consequences for police behavior. He disapproves, however, of the recent elimination of discussions, community relations panels, and "unessential" field trips in the "Rurban County" academy. The reforms he suggests include a reduction of lecture instruction, increased panel discussions and group therapy sessions, the apprenticeship of academy graduates to selected patrolmen during their probationary period, and consideration of prohibiting probationary recruits from carrying a weapon.

Harris identifies the development of group solidarity as the most significant of the unintended consequences of police training and relates it to the organizational characteristics of defensiveness and depersonalization. The importance of this solidarity is manifested in the difficulty of investigating and prosecuting police corruption, demonstrated in the reports of the Knapp commission in New York City. Virtually all policemen (at least in some departments), even if not "on the take" themselves, will avoid reporting the misconduct of a fellow policeman. Unfortunately, Harris does not deal with the effect of reformed training on the formation of group solidarity. In a matched-pair study of recruit training in the Los Angeles County Sheriff's Department, the investigators found that, contrary to their expectations, the recruits who had undergone a high-stress, militaristic style of training (which is conducive to the development of solidarity) did not perform as well, either technically or in relations with civilians, as the recruits whose training was less stressful (reported in *Time*, July 31, 1972, pp. 43-44). The effect of reformed training on solidarity needs more attention.

Aside from its importance as a study of police training and organizational behavior, this work is a useful document for sociologists interested in the method of participant observation. In a candid appendix, Harris describes the difficulties he had obtaining access to conduct his study; only one department out of several contacted was willing to allow him to do research. (Three turned him down outright; two others initially agreed but later reneged.) He also recounts his difficulties in dealing with the strain between his role as researcher and his role as police recruit (although he did not graduate, he was issued a badge which was presented to him on a plaque at graduation). The staff encouraged him to keep his identity secret, which was to Harris both undesirable and, ultimately, impossible.

The resolution of this conflict was awkward. Finally, Harris's account illustrates the ways in which, despite efforts at detachment, participant observation becomes emotionally involving. "As the recruits went through the mock presentations of their certificates while I sat alone, the full significance that I was no longer a part of the group and never could be again hit me" (p. 156).

The book costs \$9.50 for less than 200 pages; it would be nice to have it in paperback.

Man and His Urban Environment: A Sociological Approach. By William Michelson. Reading, Mass.: Addison-Wesley Publishing Co., 1970. Pp. xiii+242. \$3.75 (paper).

Stephen M. Golant

University of Chicago

The assumption of a homogeneous urban population whose members have the same needs and the same preferences and exhibit the same patterns of behavior is obviously false and would lead to an unrealistic portrayal of urban life. Less obvious is that pluralism necessitates the provision of housing environments that are supportive of population diversity. Unfortunately, the mosaic of disparate housing structures and neighborhood settings that presently characterize our urban centers serve unevenly the residential needs of their occupants. A major research requirement for correcting this imbalance is a clear understanding of what kinds of housing accommodation are particularly suitable for different population groups. Urban sociologist William Michelson's stimulating book addresses this need: he carefully investigates the relationship between the diverse social and cultural characteristics of an urban population and the varied physical attributes of its members' urban environments. This volume represents an important contribution to a growing multidisciplinary literature concerned with the relationship between urban physical design and social and behavioral concepts.

The work makes three principal contributions: (1) it outlines a conceptual model in which the social, cultural, and personality variables describing an individual can be systematically related to the physical attributes of his residential setting; (2) it reviews a diffuse collection of literature relevant to the model; and (3) it suggests in modest tones a philosophy of urban planning from which both creative and functional design can proceed.

The author's point of departure is a consideration of the various schools of human ecology that traditionally have been concerned with man-environment relationships. Michelson argues that research analyses by practitioners of these schools were restricted in their perspective by a disciplinary doctrine that included too narrow a conceptualization of the environment, primarily emphasizing the "social" environment and exclud-

ing variables depicting the physical setting. In addition these studies focused on "community" social organization as the lowest order of analysis, at the expense of individual or group differences within these spatial aggregates.

Incorporating concepts from both human ecology and Parsonian theory, Michelson outlines an "intersystem congruence model" with four analytically distinct systems consisting of cultural, social, personal, and man-made physical environment sets of variables. The model focuses on the interdependencies among these four systems—in particular, on the degree of congruency between variables of the man-made physical setting with variables of the other three systems. The author is primarily concerned with analyzing "experiential congruence," that is, with "how the environment actually accommodates the characteristics and behavior of people" (p. 31). He nevertheless recognizes the importance of attitudes and perceptions that underlie any understanding of the occupancy of varied residential settings by diverse population groups.

Several guidelines are outlined for the selection of variables describing the physical environment system. The author's central emphasis is on variables describing the spatial separation of people from other people, and from nonresidential activities and land uses. These variables, moreover, should be conceived in terms that have meaning to the individual in what is referred to as an "ego-centered conception of urban form" (p. 45). The variables describing the other three systems are more specifically identified. They represent a set of concepts by which the heterogeneity of an urban population is classified according to life style, stage in the life cycle, social class, values, and pathology.

In section 2 of the book the literature relevant to the conceptual model is reviewed. Separate chapters are devoted to evaluating how individuals identified only from the point of view of a particular social concept, such as life style, appear to be accommodated by specific physical attributes of the residential setting. In the initial four chapters the author analyzes the degree of congruency between persons and their residential settings. In the final two chapters a directional or causal relationship is evaluated in which the physical setting is considered with respect to its impact on human pathology and social interaction. Interestingly, Michelson avoids dealing with a milder form of environmental determinism (than that resulting in pathology)—that is, with consequences of intersystem congruence on the urban resident's happiness, life satisfaction, or other indicators of psychological well-being.

In evaluating section 2 one must be wary of criticizing Michelson for the inconsistencies, contradictions, and omissions of the literature he is reviewing. Criticism is tempting, because the state of the art the author is synthesizing often shows a casual indifference to the rigor and orientation of his conceptual model. By treating such complementary social concepts as life style and stage in life cycle as analytically distinct frames of reference, he has provided a sensitive demonstration of the difficulty of evaluating his intersystem congruence model. This difficulty is further

reinforced by the often contradictory human needs served by the dwelling unit as opposed to the larger neighborhood setting. However, despite the tentativeness of the individual conclusions, what emerges in total is a cogent illustration of the significance of the residential setting in the daily lives of urban dwellers. It is also clear that "significance" need not be equated with "determinism," which is probably of lesser importance, as the author's balanced and unemotional evaluation of the literature concerning the deterministic role of the physical environment illustrates.

One important area of analysis is considered only tangentially by the author. This area is concerned with describing attributes of the individual that define the nature of his personal resources, reflecting his ability to deal effectively and competently with his existing or future residential environment, rather than reflecting his housing needs and preferences. These resources may be economic (income level), psychological (personality traits, awareness), physiological (health), or social (status, power). To what degree have adjustment mechanisms been initiated by the individual, allowing him to cope successfully with an existing or future housing environment, by either changing or modifying his ideal housing goals or preferences or, alternatively, by his changing or failing to initiate particular activities or patterns of behavior? This kind of query, of course, necessitates a discussion of exactly what mental or experiential congruence implies, and to what degree intersystem congruence merely reflects systematic differences among members of an urban population with respect to their ability or necessity to adapt successfully to those aspects of the residential environment that are less than optimum.

In the final chapter (section 3) the author reexamines the intersystem congruence model within the context of the empirical findings that have been reviewed, and he suggests a number of constructive research and planning strategies. One testimony to the value of this volume is illustrated by the enthusiasm it generates from both undergraduate and graduate students who find in it evidence that social and behavioral theory can play a direct and integral role in urban planning and policy.

American Society: Problems of Structure. By Jonathan H. Turner. New York: Harper & Row, 1972. Pp. xix+299. \$5.95 (paper).

Fred Roberts Crawford

Center for Research in Social Change, Emory University

Jonathan Turner suggests that his analysis of American society should "appeal to those who have at some time posed the question 'What the hell is the matter with the system?'" (p. xv). Assuming for himself "the role of social critic" (p. xvi), he chastises sociologists who make a plea usually "in the first paragraphs of social problem books . . . for the continued maintenance of a detached and scientific stance!" (p. xvi). From his subjective judgment, he places primary emphasis "on the fact that social

problems are *structural* [his italics] in origin and persistence" and "the entire structure of American society must be analyzed if an understanding of social problems is to be achieved" (p. xv).

Turner presents his interpretation of the entire structure of American society in 10 chapters which deal with such matters as "The Birth of American Cities," "The Great White Exodus," "Riots in Urban America," "Distributing the Wealth, American Style," Institutionalized Discrimination," "Problems with Police: The Twilight of the Rule of Law," "Who Should Run and Fund the Schools?" "The Culture of Pollution," "The Population Bomb," and so forth.

According to his subjective theory, Turner believes that "human societies are organized into four generic types of structures: (1) residential communities, (2) social strata, (3) social institutions, and (4) ecological communities" (p. 3). Through his subjective use of labeling he is able to encompass what could be considered diverse elements in single categories, such as war: "the United States in recent decades has engaged in numerous wars: Korea . . . , Indochina . . . , Guatemala . . . , the Congo . . . , Cuba . . . , Laos . . . , Vietnam . . . , the Dominican Republic . . . , Cambodia . . . , and Laos" (p. 168). His subjective approach to statistics enables him to take quantitative data, such as the total number of battles and skirmishes engaged in by U.S. forces "from 1776 until 1900" (p. 168) and conclude: "These figures average out to 90 violent conflicts per year for the first 125 years of American government" (p. 168). His subjective mastery of the process of social change and of higher education permits him to conclude that "higher education cannot be the vehicle of change" (p. 223). As a final example, his subjective command of demography lets him conclude that "overpopulation is a direct outgrowth of the cultural premises and structural arrangements of the American kinship system" (p. 258). Some of his other subjective conclusions are that "contraception will not stop population growth" (p. 260), which will have to be dealt with by "radical programs—such as *compulsory* [his italics] sterilization, abortion, and contraception" (p. 260), and that "abortion repeal can occur only on a state-by-state basis" (p. 268).

There are no new data presented in this volume. It serves as an excellent example of why sociologists should return to the objective, neutral, scientific approach in their work.

Society and the Policeman's Role. By Maureen E. Cain. London: Routledge & Kegan Paul, 1973. Pp.ix+315. \$13.50.

Lawrence Rosen

Temple University

With few exceptions, recent major police studies have been primarily concerned with law enforcement and the patrolman's behavior vis-à-vis the citizenry he polices. Their focus has been actual behavior (e.g., arrest,

field interrogation, frisking, harassment, illegal violence) or mental sets (e.g., authoritarianism, attitudes toward procedural law, cynicism, beliefs about minority groups). Regardless of their focus, however, the studies have emphasized law enforcement and related issues. The author of this book seems to promise, or at least hint, at the same: "This study . . . looks at the relationship between the policeman and the community he polices in terms of the power which that community has or does not have over him. It seeks to identify the bases of this power, and that of other groups" (p. 26). The author even suggests that she might provide "an empirical answer to the questions whose law do the police enforce and why?" (p. 26). These are fairly profound and weighty issues; unfortunately the study generally falls short of such aims. Instead of discussing the community's "power over" the policeman, the author speaks about the degree of congruence between the policeman's potential behavior in six hypothetical situations (all are off-duty situations, and only one or two have any direct bearing on law enforcement) and his image of how others expect him to behave. Rather than providing answers to questions like "whose laws" the policeman enforces, the author reaches conclusions about police "interdependence" with the public, wives, colleagues, and senior officers, using such indices as marital integration scores, the "desire to achieve integration with the community," or the numbers of visits to nonpolice friends. (This interdependence, with the exception of that with wives, is seen entirely through the eyes of the policeman.) These matters may have intrinsic sociological interest, but they are some distance away from the basic problems related to law enforcement and "bases of power."

The study is the author's 1969 doctoral dissertation, reflecting data collected in 1961 and 1962 on a sample of rural and urban English police. For the most part there are few changes from the original thesis, and by the author's own admission, the study does not present a valid description of contemporary English police. (Some speculations about subsequent changes in the police force are offered in the final few pages.) Both "participant observation" and structured interviews are the data sources. A total of 119 officers (mostly constables) and 87 wives were interviewed. The interviews provided primarily social psychological data, although Cain asked some questions about visiting patterns. Many of this data were handled in a fairly formal manner (for example, scales). The observational data, by contrast, are principally concerned with overt behavior and tend to be informal, unsystematic, and, at times, anecdotal.

"Interdependence" with each of the "other groups" (public, wives, colleagues, senior officers) was measured by indices specific to the group being considered (e.g., marital integration, number of contacts with non-police, desire for community integration, etc.). As a result, comparisons among the several groups were not possible. Instead the comparative analysis had to be relegated to rural-urban differences. Rural police were found to have greater "dependence" on the community and senior officers and less on their colleagues, when compared to the urban force. No rural-urban differences were evident in observing "interdependence" with wives.

The relative ability of the groups to "define the role" of the policeman was measured by one common indicator: the number of times the policeman differed in his reaction to six hypothetical situations and how he thought others expected him to react. The use of a common measure thus allowed for comparisons among the four groups. The findings were quite similar for both the rural and urban police forces: there was most agreement with colleagues and least with the "public." The wives and senior officers occupied an intermediate position. Since only one of the hypothetical situations dealt in any direct way with the central features of a policeman's role, the extent to which these findings address the problem of who defines the role of the policeman, at least in terms of his work and official duties, remains tenuous.

Attempts at a more intensive analysis of interdependence and consensus consists of a series of uninteresting bivariate comparisons (e.g., relation between marital integration and police interdependence). Because of the small number of cases in many of the tables, the conclusions are not very persuasive. In addition, the author tends to make much ado about statistically trivial differences. For example, she presents a difference of 4.2% as a finding of some consequence (p. 215). Rarely does any finding reach statistical substance, though many are statistically significant.

The observational data provide perhaps the most interesting and valuable contributions of the book. Here the author tends to deal with the central features of the policeman's role. Discussions about "perks" (rewards or gifts) and "easing" (the forms of relaxation and respite from formal duties) and the effects of role segregation on "peace keeping" and police criminal investigation provide the possibility of widening our understanding of police, especially in terms of rural-urban differences. In addition, this aspect of the book adds an important comparative perspective to the numerous studies of American police. It is unfortunate that these kinds of issues were not explored more systematically.

In sum, the book, with the possible exception of the observational data and analysis, will probably disappoint those who are seeking to enhance their knowledge of how and why police behave the way they do in the performance of their duties.

Biracial Politics: Conflict and Coalition in the Metropolitan South. By Chandler Davidson. Baton Rouge: Louisiana State University Press, 1972. Pp. 301. \$11.95.

James E. Conyers

Indiana State University

Numerous books on black politics and leadership have appeared in recent years. Most of them have been polemical, autobiographical, general, editorial, and/or case-method-oriented works. Professor Chandler Davidson's book is among the best in this last tradition. It is primarily a case study

of biracial politics in Houston, Texas, in which the author interviews 49 black leaders, analyzes election returns, monitors newspapers, and participates in relevant activities. There is no necessary logical tie between the methodologies employed and theoretical issues discussed or the conclusions reached. In fact, there is no clear methodological statement in the book. There is no description, for example, of what questions were asked of the 49 black leaders interviewed. In the absence of such a description, one cannot comment on standardization. Much of what is said in the book, therefore, could have been said without a particular methodology.

Despite methodological difficulties, Davidson provides substantively thoughtful material when he critically discusses the following:

1. The inadequacy of pluralist theory with reference to its applicability to blacks. The "ease of assimilation" thesis is uneven for some racial and ethnic groups, particularly blacks. Further, black political participation has not translated itself into political and economic benefits and rewards. "At the present rate," Davidson contends, "Houston Negroes would not achieve income parity for seventy years or more, and they would not achieve job parity for at least one hundred thirty years" (p. 137).

2. Antidemocratic contrivances and other barriers to effective black political participation, such as at-large elections, majority rule principle, political cartography, filing fees and other expenses, intimidation, and investigations.

3. The basis for an attack on the literature which claims to demonstrate a relationship between class and antidemocratic attitudes. "The simple dichotomy," says Davidson, "of upper-class racial moderation vs. lower-class prejudice is misleading" (p. 180). He undertakes a lengthy discussion of the relationship between class and prejudicial attitudes to establish a fertile ideological atmosphere in which to lay the groundwork for a likely populist alliance between blacks and lower-class whites.

4. Objections to coalition politics. Davidson, however, argues for a coalition of blacks with lower-class whites. He presents some evidence from elections which tends to show that racial liberalism and economic conservatism are not incompatible; nor are racial conservatism and economic liberalism.

Several theoretical objections to the book can be raised. First, there is not enough differentiation of race as far as blacks are concerned. Whites are differentiated by class, ideology, and various kinds of behaviors. Black is not coterminous with lower-class white. Second, a reliance on class interests and class analysis in contemporary America has made it difficult for social scientists, including Davidson, to deal adequately with discrimination and racism, or to incorporate them into their class model. There are and have been movements of political, cultural, and economic consequence which cannot be predicated adequately on a class proposition. The treatment of race as epiphenomenal has minimized understanding of revitalization and nationalistic movements of blacks and other Third World people. Third, blacks do not regard whites, at least by class alone, as logical or "natural" allies. Blacks apparently are not proceeding on the

premise that middle-class whites "love" them any more than lower-class whites. If present developments are an indication, blacks are organizing for the most part on the basis of blackness as a primary proposition, not as a necessary subset of anything else. Witness the fact that practically all of the black members of Congress come from districts that are 40% or more black, and about half of all black elected officials in the United States come from election areas that are at least 50% black. Fourth, the book is in keeping with dominant group mentality in that it places too much of the responsibility for social change on blacks. The mood of the book tends to stress what blacks must or should do. Davidson seldom raises the question of the whites' responsibility for creating a more humane South or nation for peoples of color and, thereby, for themselves.

South Africa: Sociological Perspectives. Edited by Heribert Adam. London: Oxford University Press, 1971. Pp. xii+340. \$12.00.

Michael Burawoy

University of Chicago

Heribert Adam introduces his edited volume with a declaration of intent: to avoid moralizing about the injustices of apartheid and to treat South Africa in "analytical perspective," grounding "subjective responses and their institutionalized forms within the objective social forces which have given rise to them" (p. vii). Despite the reknown of many of the contributors, the 15 essays, of which 13 are published for the first time, generally fail to live up to Adam's programmatic outline.

In recent years a glut of books have appeared dealing with South Africa; yet surprisingly few have made any original contribution to sociological theory. One explanation for their mediocrity must be the restrictions on research in a "police state," particularly in the area of interaction between races. Even so, social scientists, whether originating from outside or inside South Africa, have frequently and unnecessarily entangled themselves in the categories of race and the ideology of white supremacy. This is not to say that they endorse such policies but that they allow them to dictate the kinds of questions they ask and types of analyses they offer. Thus, for example, Adam's essay, "The South African Power-Elite: A Survey of Ideological Commitment," focuses on the commitment of different elements of the white ruling class to its own political ideology. Using a precoded questionnaire sent to selected "decision makers" in the "political, administrative and economic sectors of the white power elite," Adam shows that conformity to the ruling ideology, not surprisingly, is the general rule. Studying the attitudes of the "power elite" in isolation is justified on the assumption that significant change in South Africa will come about through changes in the ruling group. Curiously, Adam shows no interest in the voluminous literature on "power elites," the theoretical issues that focus on the concentration or dispersion of power, patterns of recruitment to the

"power elite," or mobility and linkages between its different segments and other structural features that would normally precede an attitude study.

Following Adam's approach but basing his ideas on a firmer theoretical foundation, Kurt Danziger argues that "ideological changes are important in facilitating or obstructing actual changes in the pattern of domination in a society" (p. 285). If South Africa is to develop economically, the dominant ideology must move away from *Wertrational* toward *Zweckrational*, that is, from ascription and particularism toward achievement and universalism and from an ideology of coercion to one emphasizing non-violent means of conflict resolution. For indications of such change, Danziger refers the reader to a number of social psychological studies concerned with changes in individual "cognitive styles," in particular "self-rationalization." These studies show how the cognitive orientation of the Afrikaner group has adapted over time to the changing economic environment.

In another contribution to the volume, J. W. Mann offers a useful and critical examination of much of the research conducted in the field of social psychology in South Africa. The increasingly questioned assumption that psychological factors are important independent variables in stimulating social change underlies all of these ideology and attitude studies.

Pierre van den Berghe bases his essay on the distinction between micro, meso, and macro apartheid and provides further illustration of encapsulated thinking. He suggests that micro segregation (petty apartheid) is a "luxury," meso segregation (ghettoization and urban controls) a "cornerstone in the maintenance of the *status quo*," while macro segregation (the creation of black "independent" homelands) contains the seeds of the destruction of the entire racial edifice. The creation of Bantustans (homelands) leads to expectations among blacks that are doomed to frustration, because while rule cannot afford the political and economic costs of genuine black independence. However, recent empirical research has not revealed any such revolution of "rising frustrations," even though it is now a decade since the first Bantustan—Transkei—was established. On the contrary, potential conflict has been institutionalized and thereby contained.

Adopting an approach to education akin to van den Berghe's approach to Bantustans, both Kogila Adam and H. F. Dickie-Clark point to the conflict between "universalism" and "particularism" in "tribal colleges." Adam's interesting article describes ethnic segregation and rigid government controls over black education, which give rise to the realities of "separate and unequal." Dickie-Clark stresses the role of education in fostering racial perceptions and politicization of black students. Both argue that the government's desire to reinforce cultural diversity and social segmentation conflicts with the training of manpower and the common education that these goals require. Yet both articles ignore education's contribution to vertical stratification within the subordinate racial group. Indeed, throughout the volume there is a tendency to focus on cultural divisions emphasized by the ideology of "separate development" rather than class divisions based on economic and status differentiation. Though apartheid accentuates

allegiance to traditional forms of hierarchical stratification, it is important to what extent they are being undermined by such "universalistic" institutions as schools and colleges. Apart from Leo Kuper's seminal work, little has been accomplished in the area of stratification within the black racial group. Equally little attention has been given to stratification within the dominant group. (For example, what role does the white worker play in maintaining racial domination? In what sense may we speak of a conflict between "poor whites" and "upper-class whites"? How has conflict between white workers and the mine owners affected the development of racism in South Africa?) Instead, there is a persistent stress on the cultural separation of Afrikaner and English-speaking groups. Finally, the contributors fail to consider the significance of the colored and Asian groups, who together are almost as numerous as whites.

One further consequence of a preeminent concern with the idiom of race is the tendency to stress the uniqueness of South Africa at the expense of structural similarities with other countries. Apart from Richard Ford's interesting and revealing comparison of the mobility of Afrikaners in South Africa and the Irish in America and Stanislaw Andreski's suggestion that oppression in South Africa may not be as severe as in other totalitarian or racially divided societies, the focus of description and analysis is confined to South Africa. The problem of "misplaced emphasis" is best illustrated by G. V. Doxey's essay, "Enforced Racial Stratification in the South African Labour Market," where he describes the apparatus of color bar and job reservation as "a market system where the sphere of employment is being subjected to a degree of control only equalled in a totalitarian command economy, the only difference being that in South Africa controls upon labour have no economic basis nor can they be justified upon an economic theory" (pp. 279-80). Yet job reservation and color bar are variants of what has been commonplace in America, Britain, and other countries where craft unions and professional associations have successfully sought to monopolize control over labor markets and to restrict entry to certain occupations. Though the criterion of entry has not normally been color, nonetheless structural similarities remain. The differences must therefore lie in the degrees of rigidity created by the "uniform of colour" and the powerlessness of the black worker. Where labor aristocracies in America and Britain have tended to dissolve, the South African white labor aristocracy has consolidated its position. Equally, migrant labor, voluntary and forced, can be usefully analyzed in comparative perspective. For example, close parallels can be drawn between the role of Mexico as a labor reservoir for unskilled farm labor in America and Malawi serving a similar function with regard to mine labor on the Johannesburg Reef.

Other contributors tend to lose track of what is typical in South Africa. Ellen Hellman, for example, might have awarded less attention to the conventional wisdoms of urbanization and devoted more attention to the reasons for the persistence of certain "traditional customs." Such selective persistence should be treated as rational adaptive responses to the exigencies of urbanization in South Africa rather than as a form of "cultural lag."

In this respect Philip Mayer's article stands out as a contribution to the understanding of South African urbanism. Following his earlier work, where he distinguishes between townsmen oriented to rural life ("reds") and those more firmly committed to town life ("school"), he shows how the creation of religious organizations and the persistence of "customary" associations protect the townsman from the insecurity and anxieties endemic in urban South Africa. Voluntary associations and kinship networks become functional equivalents in that both provide the framework for total institutions which "encapsulate" the townsman in a secure and predictable milieu.

Just as the examination of exceptions to the general rule of unilinear urbanization illuminate the urban process, so "deviant" cases would highlight salient features of the legal process. Thus, A. S. Mathews's general account of the use of the legal apparatus as an instrument of racial domination would be enhanced by an analysis of instances when the South African judiciary refused to comply with the interests or even demands of political power.

The amazing sociological characteristic of South Africa is its persistence. How do 4 million whites manage to keep 15 million blacks in subjection? Until recently most writers assumed that collapse was inevitable, and they therefore examined the political and economic structure for the "seeds of change." Heribert Adam has provided a long-awaited corrective to this continuing "optimism" in his latest work, where he advances the thesis that South Africa is the prototypical stable racial oligarchy. Thus, Adam redefines the central problem of the social scientist as being a rigorous examination of the South African state apparatus and the mechanisms of societal persistence. But the "seeds of change" approach dies hard, and, not surprisingly, the volume contains two essays devoted to the failure of the political resistance movements. In concluding her essay, Gwendolen Carter views the rise of nationalism as a response to "separate development" with some optimism, while Fatima Meer looks for hope outside South Africa.

The narrow concern with visible and sensational forms of protest directs efforts away from more fundamental structural analyses of protest and social movements. A more thorough analysis of protest would involve an examination of less visible and more mundane manifestations of conflict, not merely in the political arena but also within industry, religion, and education. Nor can each type of protest be considered in isolation. Interconnections through space and time must also be subjected to inquiry. Is there a substitution effect such that when, for example, political activity is suppressed, protest reappears in religious or educational institutions or breaks out in the form of labor strikes? Only recently have student movements been treated seriously. Apart from Sundkler's outstanding book, there has been little in the way of rigorous analysis of religious protest in South Africa. Even more inexcusable is the neglect of industrial conflict. Not one of the essays in the volume under review examines the position of the black worker. When he is mentioned, he is viewed as a passive,

defenseless victim of racial exploitation. Yet this has by no means been the case, and, as the recent strikes in Durban indicate, it is an area that can no longer be neglected.

It is significant that South Africa's "outward looking policy" is given scant attention. Jordan Ngubane, who perceives apartheid in terms of a conflict of moralities (the morality of fulfillment committed to freedom and the morality of survival committed to white supremacy), views the government's attempt at dialogue with black Africa as an expression of frustration with "their [the government's] inability to honor their pledged word on Bantustans" (p. 21). Throughout the volume the political imperatives of an expanding economy, leading to the search for markets outside South Africa, are ignored. In maintaining some three quarters of the population near subsistence, business interests are deprived of an internal market which could absorb the products of an expanding economy. The proximity of black Africa to sea routes to Latin America makes such poorer nations attractive markets for South African goods. It is not surprising, therefore, that certain "enlightened" (business) interests within Afrikanerdom have encouraged dialogue and rapprochement with black Africa. Such economic factors underpin the mechanisms of domination and South African expansionism. They can be ignored only at the analyst's peril.

In summary, the 15 essays are a balanced reflection of contemporary writing on South Africa in the area of social science. At the same time, they do not offer a balanced assessment of South Africa. Enshrouded in the mystique of racial ideology, they attach too much significance to what is peripheral and make light of what is central. There is little advance of sociological theory as it applies to racially divided societies, and we have progressed little in understanding what holds South Africa together. But a collection of readings of this nature is very useful if only to clarify and highlight the defects and strengths of recent writing. Hopefully it will point the way to more systematic studies of the South African social structure.

Perceptions of Work: Variations within a Factory. By H. Baynon and R. M. Blackburn. New York: Cambridge University Press, 1972. Pp. 179. \$12.50 (cloth); \$6.95 (paper).

George A. Miller

University of British Columbia

This small book reports the results of research undertaken in 1966 at the request of William Gourmet and Sons, a fictitious name for an international producer of luxury foods. The company wanted an attitude survey of its employees, and the study was conducted in a subsidiary plant located in Brompton, England. The researchers secured the cooperation of the union

and state that neither the research itself nor the writing of this report was influenced by the company.

Data were obtained from a stratified random sample of 231 workers by means of a structured interview schedule consisting of questions that had been pretested and found to be relevant and reliable. The authors state that "as this was basically a descriptive and exploratory study we have not considered it appropriate to use any complicated statistical or measurement techniques" (p. 11). The data are, therefore, presented in simple cross-classification tables, and no controls are introduced.

The purpose of the study was to examine the influences of (1) workers' orientations and (2) organizational structure in determining variations in the workers' perceptions of their work. As the authors say: "The basic theoretical point here is that involvement depends on the relationship between orientations and the objective features of the work situation" (p. 157). Orientation toward work is considered a "central organising principle whereby people make some sense of their lives" (p. 157), and as such, "it takes account of the workers' life beyond the factory gates and the relevance of this for work" (p. 3). And, not surprisingly, it is argued that "the way in which work is experienced depends neither on the work factors nor orientation alone, but on the interaction of the two" (p. 4).

On the bases of the workers' social characteristics and formal relationships with the company, four distinct groups of workers were identified that provided the major comparisons throughout the study. These groups are composed of (1) women in part-time employment, (2) women in full-time employment, (3) men working the night shift, and (4) men working the day shift. The authors expected to find that each of these groups would attract employees with different expectations concerning their work so that different perceptions of work would be evidenced in each group (p. 35). These expectations were confirmed. "The groups tended to have significantly different perceptions of work, arising, in large measure, from differences in non-work social characteristics, particularly their positions within the family" (p. 146). The authors also conclude that "as a group, working-class women form the most highly exploited section of the labour force" (p. 147).

Briefly, the major findings are as follows: *Part-time women* workers were primarily married women with children who worked to supplement their husbands' incomes and/or to escape loneliness. They considered work secondary to being a wife and mother, and thus evidenced low commitment to work. As a result, they were relatively uncritical of their jobs and were the most satisfied of all the workers. *Full-time women* workers were younger, either single or recently married, and without children. Most had to support themselves and thus stressed the importance of good pay and job security. Their major attachment to their jobs consisted of workmate friendships. *Night-shift men* workers tended to be family centered and instrumentally oriented. They had a positive perception of their work and placed high value on job security. *Day-shift men* workers were the

most dissatisfied group. Being primarily young and single, they had high expectations concerning their work. They also evidenced low attachment to the firm and relatively low job satisfaction.

The authors conclude this study by saying that they "hope to have shed some light on an important subject" (p. 161). It is useful to be reminded that one's experiences in his place of work (and elsewhere) are related to one's position in the larger social structure. However, I consider such observations to be both obvious and uninteresting. Indeed, it would have been surprising (and interesting theoretically) if the findings had been otherwise.

NRPA Recreation and Park Perspective Collection. Program of the National Recreation and Parks Association. Washington, D.C.: McGrath Publishing Co., 1972. 30 vols.

H. Douglas Sessoms

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The *NRPA Recreation and Park Perspective Collection* is a 30-volume set of reprints of selected works written about recreation and leisure since 1900. Each book in the *Collection* was chosen (according to its uniqueness) by the research staff of the National Recreation and Park Association. An advisory committee of recreation and park professionals and university librarians assisted the NRPA staff in this task. The project grew out of the expressed desire of students of the recreation movement for copies of these classics, most of which have been out of print for generations. Among the works included in the *Collection* are *The Play Movement* by Rainwater (1922); *Education through Play* by Curtis (1915); *Leisure in the Modern World* by Burns (1932); *The First Country Park System* by Zanzig (1932); and *The Spirit of Youth and the City Streets* by Addams (1930).

The study of leisure behaviors, particularly recreation pursuits, has been fashionable periodically. In the past its popularity seemed to parallel moments of national affluence and stability. It received minimum attention during periods of crisis, except occasionally to be cited by social planners as a necessary ingredient to relieve tensions and maintain stability and order. At present recreation and leisure behaviors are once again being discovered as a scholarly area of interest, and the publishing houses are responding to that revival.

There are several reasons for the new awareness of the significance and importance of organized recreation and leisure services in the United States. The advent of the four-day work week, with its implications for social structures and social institutions, excites students of time use and time behavior. Second, there has been a diminishing of such social concerns as the urban riots and civil rights movement of the 1960s and the Vietnamese peace protests of the early 1970s. Attention can now be focused upon the seemingly less critical issues; leisure behavior is one of these. Third, and

possibly most significant, is the growing awareness of recreation and leisure services as a social movement with major economic and social significance. Work no longer seems to be life's central interest, and, according to economists, leisure services are among the fastest-growing investments. New towns and similar land developments are being planned around a leisure life style; America is learning to play.

The organized recreation movement, a reflector of the growing importance of leisure in America, began during the latter half of the 19th century. It resulted from the same pressures and concerns that gave birth to social welfare reforms and publically supported educational services. By the early 1900s, nearly every major American city was embarking on a plan of providing parks and recreation for its citizens. The movement caught the attention of sociologists and social philosophers during the 1920s, and the first flow of publications and studies occurred at that time.

Although the movement continued its growth and expansion during the depression and World War II years, it ceased to be a primary concern of academicians. Until the mid-1950s only those interested in developing park and recreation specialists at the university and college level wrote seriously on the subject. Several sociologists, particularly those of the University of Chicago's Center of Leisure Studies, recognized the growth of outdoor recreation behavior and the significance of more leisure. The Outdoor Recreation Resources Review Commission was established in 1958 and embarked upon a major analysis of outdoor recreation needs and demands, publishing its historical 27 volumes in 1962.

The Outdoor Recreation Resources Review Commission's reports primarily dealt with user preferences and resource needs. There were few documents concerned with the theoretical and philosophical significance of recreation and leisure services. In fact, there has been almost no theoretical writing about recreation since the 1920s, although some writers, such as Sebastian De Grazia (*Of Time, Work and Leisure*) and Joffre Dumazedier (*Toward a Leisure Society*), have responded to this deficiency.

The development of the theory of leisure behavior and of recreation and leisure as a field of academic study has been hampered by the lack of research and theoretical materials. The majority of publications on parks and recreation have been of a practical nature. The significant writings of the 1920s were no longer in print and were often overlooked by a new generation of social scientists. The National Recreation and Parks Association, in cooperation with the McGrath Publishing Company, has taken a major step toward remedying the literature deficit by reprinting these more significant early works on recreation and play. Scholars now have a reference point for the study of this social movement.

The NRPA collection series is carefully done and includes publications on all facets of recreation and leisure behavior. The volumes deal with such subjects as play and its relationship to education and mental health, the play movement and its social significance, leisure in the modern world, the threat of leisure, the philosophical significance of play and leisure, and a description of early parks and the municipalization of reaction and park

services. The reprints range in price from \$10.00 to \$45.00 per copy. All works were reviewed previously, at the time of their publication. Thumbnail descriptions of each of the various works in the collection are provided in a comprehensive brochure, available from the NRPA.

The McGrath Publishing Company is to be commended for the technical aspects of the reprints. In addition to the *NRPA Collection*, they have done 11 reprints related to leisure and recreation. Such standard works as Jane Addams's *Philanthropy and Social Progress* and Frederick Howe's *The Modern City and Its Problems* are included. These are also identified in the NRPA publication describing the *Collection*.

In reviewing the *Collection* I focused upon its comprehensiveness and usefulness. It rates high on both accounts. The March 1910 issue of the *Annals of the American Academy of Political and Social Science* is particularly valuable to social scientists. The issue is devoted entirely to the development of public recreation as a sociological and political event. It would be interesting to compare the contents of this issue of the *Annals* with subsequent issues, particularly the September 1957 volume, which dealt with the same area of concern.

From the descriptions of the volumes comprising the *Collection*, one would believe he was reviewing a current set of works. The key words and phrases, the basic issues, are the same. It makes one wonder how far we have moved in understanding and resolving some of our problems within half a century. Reading *The Municipalization of Play and Recreation* by Joseph B. Fulk dramatizes this point.

Fulk's study was originally done as a doctoral dissertation in 1917 and published in book form in 1922. He analyzed municipal officials' growing recognition of their responsibility to provide public recreation and park services for their constituencies. The issues described and the justifications of park and recreation agencies are the same as those raised in contemporary texts on park and recreation services. Fulk even discusses the difficulty of conceptualizing leisure and recreation, citing the two popular notions or frames of reference for these terms: the labeling of leisure and recreation according to the time in which activities occur, or the conception of them as a state of mind, an attitude toward existence. If today's students of leisure and recreation read Fulk during their early years of training, they would not have to spend nearly as much time discussing the nature of the definition of recreation and leisure before coming to the same conclusion Fulk made: there are at least two ways of conceptualizing the phenomena; both are distinct and lead to different methodologies and approaches to resolving the problem.

The *Threat of Leisure* by George Cutten (1926) and *The Challenge of Leisure* by Arthur Pack (1934) offer similar insight. To what extent does increased free time alter life styles and social structures? Also, to what extent can the future be predicted? Have the technological revolutions these men envisioned really had the impact that they foresaw? Are today's forecasters with their "improved techniques of analysis" going to be better

soothsayers than were Cutten and Pack? The cycle repeats itself, and each generation seems to discover the same concerns and truths.

The *NRPA Collection* does provide benchmarks for those interested in the recreation/leisure movement. It also raises questions about the validity of forecasting, man's learning from the past, the deficiency of the educational enterprise, and our ability to resolve social problems. Individually, each book offers interesting reading. Collectively, they provide an opportunity for understanding the recreation/leisure movement, its origins, significance, and consequences. They add another dimension to our study of urban America and the dynamic forces shaping 20th-century man.

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